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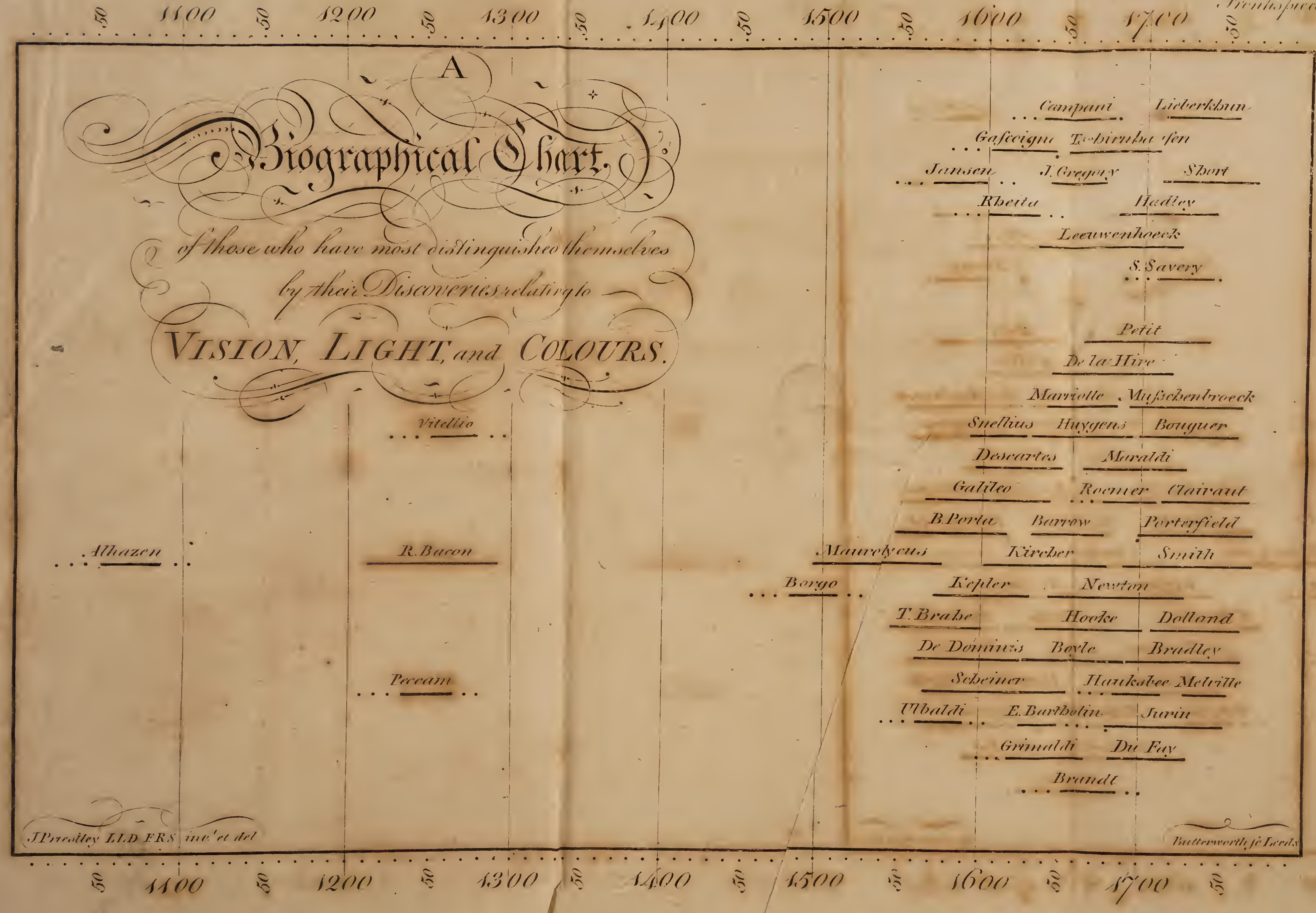

A Biographical Chart

of those who have most distinguished themselves by their Discoveries relating to

VISION, LIGHT, and COLOURS.

J. Priestley LL.D. F.R.S. inv. et del.

Butterworth sc. Leeds

THE HISTORY AND PRESENT STATE Of DISCOVERIES relating to VISION, LIGHT, AND COLOURS.

By JOSEPH PRIESTLEY, LL.D F.R.S.

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TO
HIS GRACE THE
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My Lord,

It is with peculiar ſatisfaction that I dedicate the firſt volume of ſo extenſive a work, as that of *The Hiſtory of all the Branches of Experimental Philoſophy*, to a Nobleman of your Grace's known attachment to the ſciences, and one who has, on ſo many occaſions, diſtinguiſhed himſelf by his generous patronage of them. It is for the honour of any country, and of any age, in which perſons of your rank and fortune chuſe to appear in that character.

No branch of ſcience ſtands in ſo much need of the aid of the Great as that which is the ſubject of this work. But, on the other hand, from no branch of ſcience do they derive greater advantage. For, without that *knowledge* about which it is converſant, *rank* and *fortune* would be of little value ; the elegant enjoyment of life being derived from no other ſource, than thoſe *arts* which owe their birth to our acquaintance with the *powers of nature*.

By this ſpecies of knowledge it is that mankind in general are capable of improving their ſituation in the world, making the moſt of every advantage it affords, and obviating the inconveniences to which it is liable. Theſe ſtudies, alſo, having the works of God for their object, are of eminent uſe to extend the views, and enlarge the comprehenſion of the human mind ; by which means the dignity of our nature is increaſed, and the beſt foundation is laid for our advancement in intellectual happineſs.

As the Engliſh philoſophers have particularly diſtinguiſhed themſelves by their improvements in *natural knowledge*, your Grace, who, in a variety of capacities, ſo eminently ſupports the dignity of the Engliſh nation, acts peculiarly in character, by your patronage of that branch of philoſophy for which your country has long been famous.

That your Grace may long enjoy the exalted ſatisfaction of promoting a ſcience ſo truly worthy of your taſte, is the ſincere prayer of,

My Lord,
Your Grace's
Moſt humble, and
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N. B. *b* ſignifies *from the bottom of the page.*

Page 7, line 23, *for* eye, *read* ſight.
Page 22, line 2, *b. for* circular, *read* ſpherical,
Page 65, line 2, *for* a propoſition which, *read* a fact, the phyſical reaſon of which.
Page 77, line 13, *for* teleſcope, *read* microſcope.
Page 81, line 5, *b*, *for* from, *read* upon.
Page 92, line 14, *for* another, *read* another, and to the horizon.
Page 135, line 11, *for* 1695, *read* 1663.
Page 159, line 16, *for* 40'30°, *read* 46'. 30.
Page 239, line 8, *b. for* conſidered and examined on, *read* conſidered of and examined, on.
Page 263, line 5, *for* diameters, *read* centers.
Page 265, line 17, *for* its ſides, *read* the ſides of the priſm.
Page 270, line 5, *b. for* rays, *read* red rays.
Page 296, line 8, *b. for* $\frac{16\frac{1}{2}}{100000}$ *read* $\frac{16\frac{1}{2}}{10000000}$
Page 353, line 18, *for* violet, *read* violet ray.
Page 382, line 3, *for* red hot, *read* very hot.
Page 414, 415, *b. for* Theoria, *read* Traité d' Optique.
Page 418, line 7, *for* refraction, *read* refraction and incidence.
Page 512, line 13, *for* as, *read* as, in his opinion.
Page 517. line 6, *b. for* it, *read* the firſt.
Page 634, line 1, *b.* and 635, line 12, *dele* of a minute.
In page 490, &c. references ſhould have been made to *Lambert*'s *Proprietès Remarquables, &c.*

NAMES omitted, or not received when the preceding LIST was printed.

Pierce Butler, Eſq;
Mr. William Curtis, Apothecary, London.
Mr. Matthew Dove.
James Dyer, Eſq; St. Ann's, Jamaica.
William Edwards, Eſq; Ditto
Edward Edwards, Eſq; Ditto
Bryan Edwards, Eſq; St. Mary's, Jamaica.
The Honourable John Ellis, Ditto
The Honourable Alexander Fullerton, St. Ann's, Jamaica.
The Rev. Mr. Fullerton, Ditto
Thomas Fenton, Eſq; near Leeds.
Mr. John Hawkeſley, Sheffield.
Charles Coteſworth Pinckney, Eſq; of Charles Town, South Carolina.
The Rev. John Reinhold Forſter, A. S. S.
Mr. Serjeant Glynn.
Alexander Johnſton, M. D. St. Ann's, Jamaica.
Matthew Maty, M. D. Acadd. Impp. Nat. Cur. & Petropp. Sec.
Gilbert Michell, Eſq; Thornhill.
Francis Montague, Eſq.
Mr. George Oates, Merchant, Leeds.
The Honourable Phillip Pinnock, Eſq; St. Ann's, Jamaica.
James Rigby Whitehorne, Eſq; Ditto

THE

PREFACE.

IN order to facilitate the advancement of all the branches of uſeful ſcience, two things ſeem to be principally requiſite. The firſt is, an hiſtorical account of their riſe, progreſs, and preſent ſtate; and the ſecond, an eaſy channel of communication for all new diſcoveries. Without the former of theſe helps, a perſon every way qualified for extending the bounds of ſcience, labours under great diſadvantages; wanting the lights which have been ſtruck out by others, and perpetually running the riſk of loſing his labour, and finding himſelf anticipated in the diſcoveries he makes, which is a great mortification and diſcouragement. In other reſpects, alſo, the progreſs of natural knowledge is retarded on this account; ſo that, in the preſent ſtate of ſcience, ſuch hiſtories as theſe are, in a manner, abſolutely neceſſary.

Conſidering how very few perſons are acquainted with the preſent bounds of any branch of ſcience, or, indeed, are in circumſtances in which it is poſſible for them to acquire that knowledge, and how much time and labour are even, in that caſe, neceſſary, in order to acquire it; it muſt be allowed that, were thoſe hiſtories judiciouſly compiled, real diſcoveries would be much more numerous; and were theſe, again eaſily and expeditiouſly circulated, the progreſs of knowledge might be accelerated beyond what we can now conceive.

At preſent philoſophical knowledge is ſo diſperſed in various books and languages, that the very reading of what is abſolutely neceſſary, in order to be properly acquainted with any one branch of it, would take up more time and attention than any perſon, though ever ſo much devoted to philoſophical purſuits, would ever think of beſtowing upon it; unleſs he ſhould make it his buſineſs to digeſt the materials into a hiſtory, or ſyſtem, for the uſe of *others* as well as of *himſelf*; not to mention the *expence* of purchaſing the books that he would want, which would be ſeveral hundred pounds for any one branch of the ſcience; ſince, for this purpoſe, he muſt, at leaſt, be poſſeſſed of the periodical publications of all the philoſophical ſocieties of note in Europe.

Having had my mind ſtrongly impreſſed with the idea of the great importance of full and complete hiſtories of the ſeveral branches of experimental philoſophy, from my own experience of the want of them, ever ſince I gave any attention to the ſubject, I ventured upon a plan of this kind for one of the branches, to which at that time I happened to be more particularly attached, viz. *Electricity*; and though I had very great difficulties to ſtruggle with in the execution of it, particularly from the want of a ſufficient ſtock of materials, when I firſt began to compoſe, I had ſo many other advantages, eſpecially thoſe which I derived from my intimacy with the perſons who were moſt diſtinguiſhed for their improvements in that branch of ſcience, that, I believe, the work is held in ſome degree of eſteem by the judicious and the candid. This ſucceſs has encouraged me to endeavour to carry my original deſign to its juſt extent, and to attempt the hiſtory of all the remaining branches of experimental philoſophy.

It will be ſeen, in the preface to the firſt edition of the hiſtory of Electricity, that I then conſidered the hiſtory of *all* the branches of experimental philoſophy as too great an undertaking for any one perſon; but, like the fox with reſpect to the lion, a nearer view has familiarized it to me, and I now look upon it not only without dread, but with a great deal of pleaſure; conſidering it not only as a very practicable *buſineſs*, but even as an agreeable *amuſement*. I am ſatisfied that it is not to be compared to many works which have been executed by ſingle perſons, of moderate abilities, but pretty cloſe application, and who have not been poſſeſſed of more zeal and good will to their undertakings than I have for this. A very great degree of enthuſiaſm would not anſwer the purpoſe. That could only carry a man through ſome ſingle and great effort; whereas the thing that is wanted is a moderate and conſtant attention for a courſe of ſome years.

Such an attention, however, can hardly be expected to be kept up by a mere *writer*, compiler and hiſtorian. It is in a manner neceſſary, for this purpoſe, that a man have ſuch a taſte for experimental philoſophy, as will engage him in ſimilar purſuits. This will give him a clearer idea of the buſineſs about which he writes. In ſhort, he will have the ſame advantage in a work of this kind that Xenophon, Cæſar, and other warriors are allowed to have had, in deſcribing a battle, and which eminent ſtateſmen, well verſed in public affairs, have had in writing general hiſtory. I hardly need to obſerve, however, that *a mere philoſopher* is as little qualified for this undertaking as a mere writer. To execute a work of this extenſive kind to advantage, a perſon muſt have acquired ſuch a ſkill in ſelecting, abridging, and arranging the materials that come before him, and ſuch a readineſs in compoſition, as, like other arts and habits, can only be acquired by much practice.

The pleaſure I have in this undertaking is not ſo great, but that it will depend upon the reception my labours meet with, whether I continue them or not. If thoſe perſons for whoſe opinion I have an eſteem, favour me with their approbation, and the ſale of the work indemnify me for the very great expences I have been at on account of it, I ſhall perſevere with pleaſure. If the reception be unfavourable in theſe reſpects, I ſhall certainly deſiſt, and be thankful that I can betake myſelf to other ſtudies, not leſs agreeable to me. As to the opinion of thoſe for whom I have no eſteem, on account of the unworthy motives by which their judgments are influenced, they will not weigh with me at all. A man muſt have had little experience of life, and have acquired very little ſtrength of mind, indeed, to be diverted from an undertaking which he believes to be uſeful, and important to mankind, on account of trifling, unjuſt, or unreaſonable cenſure. It cannot be expected, however, but that a work of this extenſive kind, though it were undertaken by ever ſo many perſons, even the ableſt, and, in all reſpects, the moſt proper for the purpoſe, ſhould be free from imperfections and errors; and the uniformity, and other advantages, which muſt reſult from its being executed by a ſingle perſon, will, it is hoped, be a ſufficient plea for the greater indulgence which he will neceſſarily ſtand in need of.

Some will probably think that it would have been more adviſeable for a perſon who meant to do the whole of this work, to have undertaken to do no more than one branch of the ſcience, at one time, and not to have done any thing with reſpect to another, till that had been finiſhed. But, beſides that the expence of executing any one part completely is not much leſs than the whole will require, two other capital advantages attend the undertaking of the whole at once. For by this means there is no occaſion for any philoſophical treatiſe to go through the writer's hands more than once, in order to common place all the materials it contains; and then, having his whole ſubject in view, nothing that is to his purpoſe can well eſcape his notice; whereas upon the other plan, he would be under the neceſſity of examining a great number of volumes many times over; and, never having more than one ſingle object in view, articles lodged in unſuſpected places would intirely eſcape him. Indeed, let a man uſe all the precautions he can poſſibly think of, he muſt expect to overlook ſome things, even though he had acceſs to all the books that he wanted, unleſs he ſhould actually compoſe the whole before any one part went to the preſs, which is much too hazardous for any perſon to think of. The reaſon of this is that the titles of books and papers are no ſufficient indication of their contents, on account of their often being either very miſcellaneous, or ill arranged: and if a perſon actually read every page of an author, he may almoſt as well write a full account of it, for the uſe of his work.

I would not have my reader imagine that this work, though written by a ſingle perſon, contains nothing more than his own obſervations, which might well be concluded to be very inadequate to the purpoſe, I have not the vanity to pretend to an univerſal knowledge of philoſophy. Indeed no man can pretend

tend to it; but I am happy in my friendſhips with thoſe who are allowed to be the moſt eminent in each of the branches, and no part of the work will be put to the preſs, till it has received the benefit of all the aſſiſtance that they can afford with reſpect to it; and none of them will ſay that I am not very thankful for any information they give me, and very ready to correct the miſtakes that I fall into. With reſpect to the preſent volume, the reader will eaſily perceive, that both he and I are obliged to my friend and neighbour the Rev. Mr. Michell, for many original communications; and my obligations to him are much more conſiderable than will appear on the face of the work. Indeed, ſeveral of the obſervations, will be ſeen to bear the marks of a more accurate acquaintance with the ſubject than I can pretend to. With reſpect to ſeveral articles, however, I was ſo circumſtanced, as to be under a neceſſity of ſending them to the preſs without conſulting either Mr. Michell, or any other perſon whatever; ſo that, if any miſtake be diſcovered in the work, myſelf only am anſwerable for it.

Senſible how inadequate the information and furniture of any ſingle perſon muſt neceſſarily be for a work of this extent, I have made uſe of every precaution that my prudence could point out to me to remedy that defect. I began with publiſhing *printed propoſals* for the undertaking, with liſts of the books which I had procured, and alſo of thoſe which I had heard of, and which I wanted for the purpoſe. This method having anſwered my deſign, I repeated my addreſs to the public, with new and larger catalogues; and now I think it unneceſſary to do it any more, imagining that I am poſſeſſed of all the philoſophical writings that can be of much uſe to my deſign. Leſt, however, I ſhould have overlooked any work of value, I ſhall at the cloſe of this, and alſo of every other volume, print a catalogue of all the philoſophical books that I have acceſs to, that thoſe perſons who have it in their power may have an opportunity of giving me the information or aſſiſtance that I ſtill may want, and for which I ſhall be very thankful.

I have adopted the *hiſtorical method*, becauſe it appears to me to have many obvious advantages over any other for my purpoſe; being peculiarly calculated to engage the attention, and to communicate knowledge with the greateſt eaſe, certainty, and pleaſure. Moreover, the recital of the labours of philoſophers in an hiſtorical method gives a writer a better opportunity than a ſyſtematical method would do, of tranſmitting them to poſterity in ſuch a manner as will operate moſt powerfully on the minds of the readers, and be a motive with them to exert and diſtinguiſh themſelves in philoſophical purſuits. To ſuch conſiderations as theſe the mind of man is never wholly inſenſible.

It will, however, be found, that, though the method I have choſen be *hiſtorical*, it is, at the ſame time, ſufficiently *ſyſtematical*; and, indeed, pretty ſtrictly ſo with reſpect to each diſtinct article in the ſeveral periods into which the work is divided; ſo that any perſon, if he chuſe it, may ſee, without interruption, all the diſcoveries that have been made relating to any ſingle head or diviſion of every ſubject. In order to be more ſyſtematical, and thereby to make the work more uſeful to young ſtudents, I have imitated the conduct of the beſt civil hiſtorians, in making the order of time give place, with reſpect to many articles, to more natural, and therefore more important connections. Thus, when later writers have added but little to the obſervations of thoſe who preceded them, I have given the whole of a ſubject in an earlier period, and when former writers contain very little, I have given the whole in a later period. This method cannot but be more agreeable to all my readers than a mere journal, or a ſtrictly chronological narrative of tranſactions.

That the generality of readers may not be diſcouraged from purchaſing this work, I ſhall endeavour to make every thing, at leaſt every thing of much value in it, perfectly intelligible to thoſe who have little or no knowledge of Mathematics, provided they give a proper degree of attention to the ſubject. I ſhall alſo endeavour to avail myſelf of every circumſtance or incident, that can contribute to render the narration pleaſing and intereſting to the reader; at the ſame time that I ſhall be careful never to loſe ſight of my principal object, which is to bring into a moderate compaſs, and to digeſt into an eaſy method, an account of every real improvement or diſcovery in ſcience, in ſuch a manner as ſhall enable the philoſophers of the preſent age to reſume and purſue the inquiries of their predeceſſors to the greateſt advantage.

At the cloſe of each part of the hiſtory, I ſhall endeavour to give a comprehenſive view of all the diſcoveries which have been made in that branch of ſcience to which it is appropriated, pointing out their natural connection, noting what I ſhall imagine to be the principal *deſiderata*, and giving hints for the extenſion of it. To each volume I ſhall alſo ſubjoin an explanation of all the *technical terms* that occur in it; and, in order to give a clearer view of the progreſs of the ſcience, I ſhall, whenever it can be done conveniently, exhibit a view of all thoſe perſons who have made the greateſt figure in the hiſtory, after the manner of my *Chart of Biography*.

That my reader may the more eaſily ſatisfy himſelf, from the fountain head, with reſpect to any article, concerning which he may wiſh to have fuller information than I have given him, I have always been careful to refer, at the bottom of the page, to the very authors that I have made uſe of in compiling the work. Neither in this volume, nor in the *hiſtory of electricity*, ſhall I be found to have made a parade of quoting any author that I have not actually conſulted. If I have met with any hiſtorical account of diſcoveries that I had reaſon to depend upon, I was glad to avail myſelf of ſuch aſſiſtance; but the reader will always perceive when I have done ſo; and, with reſpect to no fact of conſequence, have I contented myſelf with a ſecond hand account, if it was poſſible for me to come to the original; being truly conſcious to myſelf, that I have never ſpared any labour, when I have thought that it would contribute to the real value of my work.

In ſome caſes, I have found hiſtorical accounts of experiments and diſcoveries ſo full, and ſo well digeſted, that when I have, afterwards, met with the originals, I have ſeen no reaſon to make any alteration. In this caſe I have generally let the firſt reference ſtand, becauſe it pointed to the account which I had actually copied, or abridged; but if I afterwards collected any additional circumſtance from the original writer, I have ſometimes quoted both. But, in general, I have found the accounts of diſcoveries and experiments given by ſubſequent writers, and even thoſe of the greateſt reputation, to be exceedingly lame and ſuperficial; ſo that after I met with the original authors, I have been obliged to cancel the whole of the account which I had before drawn up.

As to the *figures* which I have made uſe of, eſpecially thoſe which relate to the elementary parts of the work. I have not ſcrupled to make free with thoſe of any writer that happened to ſuit my purpoſe. Moſt of them will be found in Smith, Muſchenbroeck, and Rowning.

If I be ſo happy as to give my philoſophical readers reaſonable ſatisfaction with reſpect to the execution of this volume, I think I may venture to promiſe them, that, as I ſhall not fail to uſe the ſame care and attention, they will have no leſs reaſon to be ſatisfied with the remaining volumes; as I am pretty confident that the ſubject of this volume is, on ſeveral accounts, attended with more difficulty than that of any other that is yet to come before me. At the ſame time, I imagine that it is of ſuch a nature, that the hiſtory of it may be expected to be as generally entertaining, as that of any other within the whole compaſs of philoſophy.

This part of my work will exhibit a number of the fineſt gradations in the diſcoveries of different perſons, a view of the greateſt and happieſt exertions of human genius, and the labours of thoſe who are the moſt celebrated for their philoſophical purſuits. The appearances in nature which theſe diſcoveries explain are conſtantly before our eyes, and muſt be perpetually exciting the curioſity of all thinking perſons; the inſtruments, to the conſtruction of which they led, are in daily uſe; and every thing relating to Opticks is capable of the moſt perfect illuſtration by figures.

For a variety of general obſervations on the ſubject of experimental philoſophy, reſpecting the pleaſure and advantage reſulting from the ſtudy, I muſt refer to the preface to my *Hiſtory of*

Electricity, which may be conſidered as the *firſt volume* of the work which I am now carrying on.

In the laſt of my printed propoſals I hinted that, in the next place, I ſhould probably undertake the hiſtory of *Magnetiſm*; but having been lately engaged in a courſe of experiments relating to AIR, eſpecially *with reſpect to vegetation and reſpiration*, and given more particular attention to thoſe kinds which are noxious (as that which is commonly called fixed air, the ſeveral ſpecies of inflammable air, that which is affected with the breath of animals or putrid matter, &c) and as theſe experiments have terminated in ſeveral original obſervations, ſome of which I flatter myſelf will be thought curious and important; I now think that I ſhall next apply to the *hiſtory of diſcoveries relating* to AIR. But I propoſe to wait ſome time before I proceed with this work, that I may have an opportunity of judging whether it be agreeable to the public that I continue it or not.

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THE

THE

HISTORY

AND PRESENT STATE

Of DISCOVERIES relating to

VISION, LIGHT, and COLOURS.

PERIOD I.

To the revival of letters in Europe.

THE phenomena of light and viſion could not paſs wholly unnoticed by thoſe who gave the leaſt attention to the works of nature, eſpecially if they had any deſire to know the cauſe of appearances, and the manner in which they are produced. But the firſt conjectures we meet with upon this ſubject, it muſt be owned, were not very promiſing.

Among thoſe who were firſt diſtinguiſhed by the appellation of *Philoſophers*, there was a doubt, whether objects be viſible by means of any thing that proceeds from them, or of ſomething that iſſues from the eye of the ſpectator. It was the opinion of Pythagoras that viſion is cauſed by particles continually flying from the ſurfaces of bodies, and entering the pupil of the eye; but Empedocles and Plato ſuppoſed that the cauſe of viſion is ſomething emitted from the eye, which meeting with ſomething elſe that proceeds from the object, is thereby reflected back again.* Strange as this hypotheſis appears, I do not admit with Montucla, † that it is any proof of a ſympathy between the human mind and error, that it was adopted by the moſt enlightened perſons of that age.

Miſtaken as the Platonic Philoſophers were with reſpect to the progreſs of viſion, I find that they were acquainted with two very important obſervations concerning it, viz. That *light*, from whatever it proceeds, *is propagated in right lines*, and that, when it is reflected from the ſurfaces of poliſhed bodies, *the angle of incidence is equal to the angle of reflexion*. Who it was that firſt made theſe important obſervations is not known. But, indeed, important as they are, and the foundation of a great part of even the preſent ſyſtem of opticks, it is poſſible that, if he were known, he might not be allowed to have any ſhare of merit, at leaſt for the former of them; the fact is ſo very obvious, and ſo eaſily aſcertained. The difference between light and ſound, in this reſpect, is ſtriking. The latter is perceived by us whatever objects be interpoſed between us and the ſounding body, provided there be a free paſſage through the air, though it be ever ſo winding; whereas the interpoſition of any opaque ſubſtance in a right line between the eye and an object, effectually conceals it from our ſight.

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* Saverien's Hiſtoire, p. 236.

† Hiſtoire Des Mathematiques, Vol. 1. p. 202.

That the angle of incidence is equal to the angle of reflexion was, probably, firſt diſcovered by obſerving a ray of the ſun reflected from the ſurface of water, or ſome other poliſhed body; or from obſerving the images of objects reflected by ſuch ſurfaces. If philoſophers attended to this phenomenon at all, they could not but take notice that, if the ray fell nearly perpendicular upon ſuch a ſurface, it was reflected near the perpendicular, and if it fell obliquely, it was reflected obliquely; and if they thought of applying any kind of meaſures to theſe angles, however coarſe and imperfect, they could not but ſee that there was ſufficient reaſon to aſſert their equality. At the ſame time they could not but know that the incident and reflected rays were both in the ſame plane.

To illuſtrate this, let AB, fig. 1, be the ſurface of any medium of a different denſity from the air, water for inſtance, or a plate of poliſhed metal, and let CD be a ray of light falling upon it at D, making an angle CDE with the perpendicular DE; and it will be reflected in the line DF, making the angle EDF, equal to EDC.

It is in vain that we look for any obſervation, or diſcovery of importance upon this ſubject, among any of the ſects of the Grecian philoſophers; ſo that I doubt not, but that I ſhall acquit myſelf to the ſatisfaction of my reader with reſpect to them, if, to what has been already mentioned, I add the obſervations of Ariſtotle upon this ſubject. His abilities and induſtry were, perhaps, inferior to thoſe of no man; and he was the moſt both of a *naturaliſt*, and a *natural philoſopher* of any of the antients.

Ariſtotle maintained, contrary, as he ſays, to the opinion of Empedocles, and ſome others, that light is incorporeal. If it were not a mere *quality*, but a real *ſubſtance*, the motion of it he ſays, could not be inſenſible, in paſſing from the eaſt to the weſt, though it might eſcape our notice in a ſmaller diſtance.* When he is reaſoning concerning the cauſe of tranſparency, he obſerves, that it is a property of ſome bodies, which, from being *potential* only, is rendered *actual* by the preſence of light; and therefore, with reſpect to this circumſtance merely, he defines light to be *the act of (or rather energy,* ενεργεια *) a tranſparent body as ſuch.*†

This philoſopher was ſenſible, that it is owing to the reflexion of light by the atmoſphere, that it is not abſolutely dark in all places where the ſun doth not ſhine. §

The principal phenomena of the rainbow, halo's, parhelia, &c. had not eſcaped the notice of the antients, who claſſed all theſe appearances under the common denomination of *meteors.* It is obſerved by Ariſtotle, that the halo is a complete circle, ſurrounding the ſun, the moon, or any very bright ſtar; but that the rainbow is never more than a ſemi-circle, and only when the ſun is juſt riſing or ſetting; and that the higher the ſun is advanced in the heavens, the leſs is the arch of the bow; ſo that, in the ſummer, no rainbow can be ſeen, in Greece, at noon. The diſtinct colours of the rainbow he made to be three; and he obſerves that, of the two bows, the colours of the outermoſt are always leſs vivid. ‡

It was ſuppoſed by many perſons before Ariſtotle, that a rainbow could not be made by the moon; and he ſays it is not ſurpriſing that they ſhould have entertained that opinion, ſince it can be viſible only at the full moon, and alſo in a variety of other circumſtances that ſeldom concur. He ſays that he himſelf, in above fifty years, had never ſeen more than two.

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* De Anima, Lib. 2. Cap. 6. † Ib. § Ib. Cap. 7. ‡ Meteor, Lib. 3. Cap. 2.

A rainbow, this author obſerves, may be made by the daſhing of an oar, or by any other method of ſprinkling water in ſmall drops, and that it will be viſible to a perſon who turns his back to the ſun, in the ſame manner as in the caſe of the natural rainbow.

Ariſtotle was of opinion that rainbows, halo's, and mock ſuns, were all occaſioned by the reflexion of the ſun beams in different circumſtances, by which an imperfect image of his body was produced, the colour only being exhibited, and not his proper figure. The image, he ſays, is not ſingle, as in a mirror; for each drop of rain is too ſmall to reflect a viſible image, but the conjunction of all the images is viſible. *

The reaſon which he gives for rainbows and halo's being circular, ſhows how very imperfectly this ſubject was then underſtood; but the very ſame continued to be aſſigned even after the revival of literature in Europe. Near, that is directly oppoſite to the ſun, his rays, he ſays, are ſo powerful, that the air, or vapour, is not able to ſtop their paſſage; and at a conſiderable diſtance on either hand they are too weak to be reflected in that manner; ſo that at ſome preciſe diſtance from the point oppoſite to the ſun, and conſequently in a circle, theſe appearances are produced. † They who thought that viſion was performed by means of ſomething that iſſued from the eye, repreſent the *ocular beams* as having the ſame power, when they are projected ſtraight forwards.

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* Meteor, Cap. 3.

† Cap. 6.

It is to as little purpoſe that we examine the mere opinions of Epicurus, or any others of the heads of ſects; as they are altogether unſupported by experiment. They had all a confuſed kind of notion, that, as we feel bodies at a diſtance from us by the intervention of a ſtick, &c. ſo the eye perceives them by the intervention of light. This, however, is a favourite idea with Lucretius, who followed his maſter Epicurus.

The antients were not inattentive to the common appearances of nature, though they made few or no experiments; nor did their imaginations want any fertility with reſpect to hypotheſes, to account for them. We find a great number of optical queſtions in Ariſtotle, and ſomething advanced by way of ſolution to them all; but hardly any thing worth reciting in this hiſtory, as his opinions were neither true, nor did they contribute to the diſcovery of the truth, with reſpect to any of them. Among other queſtions of his, we find one concerning the reaſon why a ſtick appears bent when it is held obliquely in water; which ſhews that the *refraction*, as well as the *reflexion* of light had not paſſed altogether unnoticed by the antients; and the broken appearance of a ſtraight ſtick in water was, probably, the circumſtance that gave occaſion to the introduction of the term. Archimedes is even ſaid to have written a book about the appearance of a ring, or circle, under water, and therefore could not but have attended to this property of refraction, and the fallacy in viſion that ariſes from it. The knowledge of theſe two properties of light may, therefore, be deemed to have been almoſt coeval with viſion, and therefore the taking notice of them can give no man a title to be conſidered as a philoſopher.

The antients had not only obſerved the effects of common refraction, but were ſenſible of the production of *colours* in refracted light. Seneca ſays, that if the light of the ſun ſhine through an angular piece of glaſs, it will give all the colours of the rainbow. But, without inquiring into the nature of the phenomenon, which had, probably, been obſerved ages before his time, he contents himſelf with ſaying, that this appearance is not of any *real*, but only a ſpecies of *falſe* colour, ſuch as is ſeen in the neck of a pigeon, which changes with the poſition. Of the ſame nature, he ſays, is a ſpeculum, which, without having any colour of its own, aſſumes that of any other body*

The antients were not unacquainted with the *magnifying power* of denſe mediums of certain figures, though they were far from underſtanding the cauſe of this effect. Seneca ſays that ſmall and obſcure letters appear larger and brighter through a glaſs globe filled with water. He alſo obſerves that apples ſwimming in ſuch a glaſs appear more beautiful than they otherwiſe would do. †

If I have not already made a ſufficient apology to my reader, for giving him ſo little of the *wiſdom of the antients* upon optical ſubjects, I doubt not but I ſhall have done it, when I have informed him, that the reaſon which Seneca gives for the magnifying power of his glaſs globe is, that the eye ſlides in the water, and cannot lay hold of its object: *(quod acies noſtra in humido labitur, nec apprehendere quod vult fideliter poteſt)* and that Alexander Aphrodiſienſis, the great commentator upon Ariſtotle, who flouriſhed near two centuries after Seneca, ſays, that the reaſon why apples appear large when they are immerſed in water is, that the water which is contiguous to any body is affected with the ſame quality and colour, ſo that the eye is deceived, in imagining the body itſelf to be larger.*

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* Nat. Queſt. Lib. 1. Cap. 7. † Ib. Cap. 6.

The antient engravers of gems are ſuppoſed to have made uſe of a glaſs globe filled with water to magnify their figures, and thereby to work to more advantage. The authority for this is a book publiſhed by Natter, upon that ſubject.

That the power of tranſparent bodies of a ſpherical form, &c. in magnifying or burning, was not wholly unknown to the antients, is farther probable from certain gems preſerved in the cabinets of the curious, which are ſuppoſed to have belonged to the Druids. They are made of rock cryſtal of various forms, amongſt which are found ſome that are ſpherical, and others lenticular; and though they are not ſo well wrought as to perform their office as well as they might have done, if they had been more judiciouſly executed, yet they are ſo well wrought, that one can hardly ſuppoſe it poſſible, that their effect, in *magnifying* at leaſt, could have eſcaped the notice of thoſe who had often occaſion to handle them; if indeed, in the ſpherical or lenticular form, they were not purpoſely intended for the uſe of magnifying or burning. One of theſe, of the ſpherical kind, of about an inch and an half in diameter, is preſerved amongſt the foſſils given to the univerſity of Cambridge by Dr. Woodward, in whoſe catalogue it is to be found.

To give a brief illuſtration of the nature of refraction, as I have juſt done concerning reflection: let AB, fig. 2, be the ſurface of water, or any fluid medium denſer than air, and let CD be a ſtraight rod held obliquely in it. To an eye that looks upon it in theſe circumſtances it will not appear to go on in a ſtraight line to D, but will ſeem to be broken where it enters the ſurface at E, and from thence go on in another ſtraight line to F; the part that is under water appearing higher than it would otherwiſe do.

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* B. Porta de Refractione, p. 20.

To give a more philoſophical explanation: Let CD, fig. 1, be a ray of light falling obliquely upon the ſurface AB. When it comes to D, it will not go on in the ſtraight line DG; but, excepting what is reflected in DF (as mentioned before) will be refracted into the line DH, which is nearer to the perpendicular DI.

For the ſame reaſon that a ray of light is bent *towards* the perpendicular in paſſing from a rarer into a denſer medium, it is bent *from* the perpendicular in paſſing from a denſer into a rarer medium. Thus, if the ray HD, fig. 1, be ſuppoſed to iſſue from under the water, when it arrives at D it will not go in the ſtraight line DK, but will be refracted farther from the perpendicular DE, into DC; ſo that, to an eye at C, it will ſeem to have proceeded from G, which is higher in the water than H.

Next to a ſtraight ſtick appearing bent, when part of it is held obliquely in the water, the apparent elevation of a piece of money, or any other object, placed at the bottom of a diſh, when water is poured upon it, was, probably, as old an obſervation as that of any thing that depends upon refraction. This circumſtance ſtill makes a pleaſing experiment, and never fails agreeably to ſurpriſe thoſe who are unacquainted with the nature of light; as they do not readily comprehend how the filling of the veſſel with water can make them ſee an object placed at the bottom of it, which was not viſible to them before, being concealed by the edge of the veſſel. This fact is referred to by the oldeſt optical writers, and is eaſily explained by fig. 1. For ſuppoſing H to be the place of the piece of money, and the line AD ſome opaque ſubſtance, through which light could not be tranſmitted; if all the ſpace, both above and below the line AB were the ſame medium, as air, rays of light proceeding from H would go ſtraight on in the line HK, ſo as never to come to an eye at C. But if all below AB be water, the rays will be refracted, or bent at D and turned into the direction DC; and conſequently the object H that was inviſible before, will be viſible in the line DG.

Without inquiring any farther into the nature of light or viſion, the antient Geometricians contented themſelves with deducing a ſyſtem of opticks from the two obſervations mentioned above, viz. the rectilinear progreſs of light, and the equality of the angles of incidence and reflexion. The treatiſe of opticks which has been aſcribed to Euclid is employed about determining the apparent ſize and figure of objects, from the angle under which they appear, or which the extremities of them ſubtend at the eye, and the apparent place of the image of an object reflected from a poliſhed mirror; which he fixes at the place where the reflected ray meets a perpendicular to the mirror drawn through the object. But this work is ſo imperfect, and ſo inaccurately drawn up, that, I believe, it is not generally thought to be the production of that great Geometrician.

As the antients ſeem to have been pretty well acquainted with the phenomena of reflected light, I ſhall here introduce a brief explanation of the principal caſes from it, deduced from the ſingle obſervation, that the angles of incidence and reflexion are equal, and applied to *plane*, *convex*, and *concave* mirrors. This, is, in fact, the ſubſtance of the latter part of Euclid's work.

If the reflecting ſurface be plane, it evidently follows from this principle, that, if a number of rays fall upon it, they will be reflected with the ſame inclination to one another that they had before their incidence; becauſe all the perpendiculars to the ſurface are parallel to one another; ſo that if the rays were parallel before their incidence, they will be parallel afterwards, and if they were converging or diverging before, they will be converging or diverging afterwards, and in the ſame angle. But this effect will be different, if the ſurface be convex or concave; becauſe all the perpendiculars to thoſe ſurfaces muſt be drawn through the center of the ſphere of convexity or concavity.

Let AB, fig. 3, be a ray of light falling upon the convex ſurface XY. When it arrives at B, it will be reflected in the line BE, making the angle ABD (CD being drawn from the center of the ſphere, through the point of incidence B) equal to DBE. In like manner, if another ray FG, parallel to the former ray AB, fall upon another point of the ſurface at G, it will be reflected into GH, making the angle FGI equal to IGH. But ſince the perpendiculars CD and CI diverge from one another, the lines BE and GH will diverge likewiſe.

On the other hand, if the ray AB, fig. 4, fall upon the concave ſurface XY, it will be reflected into BD, making the angle ABC (C being the center of the ſphere) equal to CBD; and another ray EF, parallel to AB, will be reflected into FH, making the angle EFC equal to CFH. And ſince the perpendiculars, CB and CF, converge from the ſurface, the reflected rays BD and FD will alſo converge.

To apply this to the caſe of mirrors: Let AB, fig. 5, be an object viewed by the eye at HK, in a plain mirror SV. The point A will appear to be ſomewhere in the perpendicular AI. And ſince the rays AF and AG, iſſuing from the point A, will be reflected with the ſame inclination to one another that they had before, if HF and KG be produced beyond the reflecting ſurface, they will meet in the point I, at the ſame diſtance from the ſurface with the point A. For the ſame reaſon, M will be the place of the image of the point B. And thus the whole image IM will appear to be of the ſame ſize with the object AB.

Let AB, fig. 6, be an object ſeen by reflexion from the convex mirror SV. The point A muſt, by the rule laid down above, appear ſomewhere in the line AC; and ſince the rays AF and AG will diverge more after reflexion than they did before, they muſt ſeem to meet in ſome point, as in I, nearer to the reflecting ſurface than A. For the ſame reaſon, the image of the point B will be in M; and the whole image IM will be leſs than the object AB.

Laſtly, let AB, fig. 7, be an object viewed in the concave mirror SV. The image of the point A will be ſeen in CA produced; and ſince the rays AR and AG will diverge leſs after reflexion than before, they will not ſeem to meet till they come to a point I, that is more diſtant from the reflecting ſurface than the point A. For the ſame reaſon, the image of the point B will be in M; ſo that the whole image IM will be larger than the object AB.

There is a remarkable phenomenon produced by a concave mirror, which could not be unknown to the antients; and indeed we find attempts to explain it, as of a thing that had been long obſerved, in the oldeſt optical treatiſes that we meet with. In the treatiſe aſcribed to Euclid above-mentioned, it is the ſubject of the

29th theorem of the 2d book. It is the image of an object appearing between the ſpectator and the mirror, as if it were pendulous in the air. It is produced whenever the object is placed farther from the mirror than the center of the ſphere. For, in this caſe, if any perſon will trace the progreſs of the rays, in the manner mentioned above, he will find that the rays of each pencil, from every ſeparate point of an object, will meet in ſome place between the center and the mirror. Thus, let MI, fig. 8, be an object, from every point of which pencils of rays iſſue, and are received by the concave mirror SV. If the angles of incidence and reflexion be made equal, they will all come to their foci at AB. Here, therefore, will be an image of the object; and an eye properly ſituated may ſee it in the air. If a perſon go nearer, to view it more attentively, it will not fly from him; and though it hath the appearance of a ſolid ſubſtance, yet if he attempt to lay hold on it, he will find nothing. This curious image will be inverted with reſpect to the object, becauſe the rays which iſſue from M will be collected in B, and thoſe from I in A, each on the oppoſite ſide. The magnifying power of concave mirrors is mentioned both by Seneca and Pliny.

It appears from a circumſtance in the hiſtory of Socrates, that the effects of *burning glaſſes* had alſo been obſerved by the antients; and as it is probable that the Romans had a method of lighting their ſacred fire by means of reflecting concave ſpeculums, it ſeems to have been known pretty early, that there is an increaſe of heat in the place where rays of light meet, when they are reflected from a concave mirror. The burning power of concave mirrors is taken notice of by Euclid, in the ſecond book of the treatiſe above-mentioned, theorem 31. If we give but a ſmall degree of credit to what ſome antient hiſtorians are ſaid to have written concerning the exploits of Archimedes, we ſhall be induced to think that he made great uſe of this principle, in conſtructing ſome very powerful burning mirrors; but nothing being ſaid of other perſons making uſe of his inventions, the whole account is very doubtful. It is allowed, however, that this eminent geometrician did write a treatiſe on the ſubject of burning mirrors, though it be not now extant.

B. Porta ſuppoſes* that the burning mirrors of the antients were of metal, in the form of a ſection of a parabola. It follows from the properties of this curve, that all the rays which fall upon it, parallel to its axis, will meet in the ſame point at the focus. Conſequently, if the vertex of the parabola be cut off, as in fig. 9, it will make a convenient burning mirror. In ſome drawings of this inſtrument the fruſtum is ſo ſmall, as to look like a ring. With an inſtrument of this kind, it is thought, that the Romans lighted their ſacred fire. Some have alſo thought, that this was the form of the mirror with which Archimedes burned the Roman fleet; uſing either a lens, to throw the rays parallel, when they had been brought to a focus, or applying a ſmaller parabolic mirror for this purpoſe, as is repreſented fig. 10, which is copied from Bettinus's Apiaria Vol. 7. Catoptrics p. 41. But Dechales ſhows that it is impoſſible to convey any rays in a direction parallel to one another, except thoſe that come from the ſame point in the ſun's diſk. †

We find nothing on the ſubject of opticks from the time of Euclid to Seneca, who repeats the crude ſentiments of Ariſtotle together with ſome of his own, concerning the rainbow; as that the circular figure both of this phenomenon, and alſo of the halo, is occaſioned by the motion communicated to the atmoſphere, and the vapours it contains, by the impulſe of the light; the effect of which, he ſays, muſt neceſſarily be circular, juſt as the waves ſpread in a circular manner from the place where a ſtone is thrown into a ſtill lake. *

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* Natural Magic. Lib. 17. Cap. 14. 15. † Curſus Mathematicus, Vol. 3. p. 722.

The rainbow, he ſays, is the image of the ſun reflected from a hollow and moiſt cloud. It is a bad image on account of the nature and figure of the ſpeculum, and it is of different colours, becauſe part of them come from the ſun, and part from the cloud itſelf. The mixture of colours from theſe two ſources produce, as he thought, all the varieties in the colour of the rainbow. † The drops of water made by the daſhing of an oar, he ſays, fall too quick to give a perfect image of the ſun. § That the image of the ſun is much enlarged, and diſtorted in the clouds is, he ſays, no wonder; it being the nature of many ſpeculums to produce the ſame effect. He likewiſe obſerves that, in water every thing is ſeen larger, and therefore, that it cannot be ſurprizing that this image of the ſun ſhould be ſo large; ſince it is reflected from a moiſt cloud, which partakes both of the nature of glaſs and of water. ‡

Heliodorus Lariſſeus, a writer of an uncertain age, but poſterior to Tiberius, of whom he makes mention, was of opinion that viſion is performed by the emiſſion of light from the eye, in which reſpect, he ſays, it reſembles the ſun; viſual rays and ſolar rays being reflected in the ſame manner. Theſe aſſertions he thinks are ſufficiently evident from the very form of the eye,

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* Qu. Nat. Cap. 29. † Ib. Cap. 3. § Ib. Cap. 5. ‡ Ib. Cap. 6.

which, being protuberant, is by no means formed for the reception of any thing, and from the conſideration of ſome animals being able to ſee in the dark. *

A treatiſe of opticks written by the famous Ptolemy, who lived 150 years after Chriſt, is loſt; but, from the accounts of others, we find, that he treated of aſtronomical refractions. Though refraction in general had been obſerved very early, it is poſſible that it might not have occurred to any philoſophers much before his time, that the light of the ſun, moon, and ſtars muſt undergo a ſimilar refraction, in conſequence of falling obliquely upon the groſs atmoſphere that ſurrounds the earth; and that they muſt, by that means, be turned out of their rectilinear courſe, ſo as to cauſe thoſe luminaries to appear higher in the heavens than they would otherwiſe do. The firſt aſtronomers were not aware that the intervals between ſtars appear leſs near the horizon than in the meridian, and, on this account, they muſt have been much embarraſſed in their obſervations. But it is evident that Ptolemy was aware of this circumſtance, by the caution that he he gives to allow ſomething for it, upon every recourſe to antient obſervations. †

This philoſopher, alſo, advances a very ſenſible hypotheſis to account for the remarkably greater apparent ſize of the ſun and moon when ſeen near the horizon. The mind, he ſays, judges of the ſize of objects by means of a pre-conceived idea of their diſtance from us; and this diſtance is fancied to be greater when a number of objects are interpoſed between the eye and the body we are viewing; which is the caſe when we ſee the heavenly bodies near the horizon. § In his Almageſt, however,

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* Lib. 1. Cap. 1, 2, 3, 13.

† Smith's Opticks. Remarks p. 54.

§ Montucla's Hiſtoire des Mathematiques, Vol. 1. p. 309.

he aſcribes this appearance, to a refraction of the rays by vapours, which actually enlarge the angle under which the luminaries appear, juſt as the angle is enlarged by which an object is ſeen from under water. * But we ſhall ſee much more light thrown upon this ſubject in the laſt period of this hiſtory.

After Ptolemy we find another great chaſm in the hiſtory of opticks, which, together with other branches of mathematicks and philoſophy, were cultivated chiefly by the Arabs, during the dark ages of Europe. The firſt optical writer of this nation, that we have any account of, is Al Farabi, who flouriſhed 900 years after Chriſt, and of his work we know nothing. In A. D. 1000, Ebn Haithem wrote more largely on this ſubject, treating diſtinctly of direct, reflected, and refracted viſion, and alſo of burning mirrors. But this treatiſe is alſo loſt.

The only work that remains to us of the Arabian philoſophers is that of Alhazen, who flouriſhed in the 12th century; and his treatiſe of opticks is, indeed, well worth our notice, as it diſcovers a great deal of attention to the ſubject. He gives a tolerable deſcription of the eye, and diſcourſes largely concerning the nature of viſion; maintaining that the cryſtalline humour is of principal uſe for this purpoſe, but without conſidering it as a lens; and aſſerting that viſion is not completed, till the ideas of external objects are conveyed by the optic nerves to the brain, p. 8, &c. He accounts for ſingle viſion by two eyes, by ſuppoſing that when two correſponding parts of the retina are affected, the mind perceives but one image. p. 29. And he treats very largely of *optical deceptions*, both in direct viſion, and alſo in viſion by reflected and refracted light.

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* Robins's Tracts, Vol. 2, p. 235.

Alhazen ſeems, particularly, to have been more inquiſitive into the nature of refraction than the more antient writers; in ſo much that, having made experiments upon it at the common ſurface between air and water, air and glaſs, water and glaſs or cryſtal; and, being prepoſſeſſed with the antient opinion of cryſtalline orbs in the regions above the atmoſphere, he even ſuſpected a refraction there alſo, and fancied that he could prove it by aſtronomical obſervations. This author deduces from hence ſeveral properties of atmoſpherical refraction, as that it increaſes the altitudes of all objects in the heavens; and he firſt advanced that the ſtars are ſometimes ſeen above the horizon by means of refraction, when they are really below it. This obſervation was confirmed by Vitellio, B. Waltherus, and eſpecially by the exquiſite obſervations of Tycho Brahe. * Alhazen obſerved that refraction contracts the diameters and diſtances of the heavenly bodies, and that it is the cauſe of the twinkling of the ſtars. But we do not find that either he, or his follower Vitellio, knew any thing of its juſt quantity. Indeed it is too ſmall to be determined except by very accurate inſtruments, and therefore we hear little more of it till about the year 1500; at which time great attention was paid to it by Bernard Walther, Mæſtlin, and others, but chiefly by Tycho Brahe. †

Alhazen ſuppoſed that the refraction of the atmoſphere did not depend upon the vapours in it, as was probably the opinion of philoſophers before his time, but on the different tranſparency, by which, as Montucla conjectures, he meant the denſity of the groſs air contiguous to the earth, and the æther, or ſubtle air, that lies beyond it.

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* Kepleri Paral. p. 150.

† Smith's Opticks. Remarks, p. 58, 59.

In examining the effects of refraction, he endeavours to prove that it is ſo far from being the cauſe of the heavenly bodies appearing larger near the horizon, that it would make them appear leſs; two ſtars, he ſays, appearing nearer together in the horizon, than near the meridian. *

This phenomenon he ranks among *optical deceptions*. We judge of diſtance, he ſays, by comparing the angle under which objects appear with their ſuppoſed diſtance; ſo that, if thoſe angles be nearly equal, and the diſtance of one object be conceived to be greater than that of the other, it will be imagined to be the larger. And the ſky near the horizon, he ſays, is always imagined to be farther from us than any other part of the concave ſurface, p. 202. Roger Bacon aſcribes this account of the horizontal moon to Ptolemy, and as ſuch, it is examined, and objected to by B. Porta. †

In the writings of this philoſopher, we find the firſt diſtinct account of the magnifying power of glaſſes, and it is not improbable, but that what he wrote upon this ſubject gave riſe to that moſt uſeful invention of *ſpectacles*. For he ſays that, if an object be applied cloſe to the baſe of the larger ſegment of a ſphere of glaſs, it will appear magnified. He alſo treats of the appearance of an object through a globe; and he ſays that he was the firſt who obſerved the refraction of rays into it. §

Montucla ſays, ‡ that this writer is by no means exact in his rules concerning the laws of refraction, the foci of glaſs ſpheres, or the apparent ſize of objects ſeen through them; and Saverien obſerves, || that his demonſtrations are ſo embaraſſed, that it is difficult to underſtand him. I will add, that his method of writing is exceedingly prolix, and that he abounds with Ariſtotelian ſubtlety and refinement.

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* Montucla, Vol. 1. p. 352. † De Refractione, p. 24, 128. § Smith's Opticks. Remarks, p. 15. ‡ Hiſtoire, p. 625. || Ib. p. 242.

Vitellio, a native of Poland, took great pains to illuſtrate Alhazen's opticks, in a treatiſe publiſhed in 1270. It contains almoſt every thing that is valuable in that of Alhazen, and is digeſted in a much more metnodical and intelligible manner. He takes particular notice of the light that is loſt by reflexion and refraction, in conſequence of which all objects appear leſs luminous; but he does not pretend to eſtimate the quantity of it.*

The reſult of his experiments on the refractive powers of air, water, and glaſs, correſponding to different angles of incidence, he reduced into a table.†

In his account of the horizontal moon, he agrees exactly with Alhazen, obſerving that, in the horizon, the ſky ſeems to touch the earth, and appears much more diſtant from us than in the zenith, on account of the intermediate ſpace containing a greater variety of objects, upon the viſible ſurface of the earth.§

This author aſcribes the twinkling of the ſtars to the motion of the air, in which the light is refracted; and to illuſtrate this hypotheſis, he obſerves that they twinkle ſtill more, when they are viewed in water put into motion.‡

He ſhews that refraction, as well as reflexion, is neceſſary to form the rainbow; becauſe the body which the rays fall upon is a tranſparent ſubſtance, at the ſurface of which, one part of the light is always reflected, and another refracted. But he ſeems to conſider the refraction as ſerving only to condenſe the light, and thereby enabling it to make a ſtronger impreſſion upon the eye.‖

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* Optica, p. 190, 414. † Ib. p. 412. § Ib. p. 124, 442. ‡ Ib. 449. ‖ Ib. p. 458.

After much obſervation, he defines the colours of the rainbow to be three; but with Seneca, he ſuppoſes them to be produced by a mixture of the brightneſs of the ſun's light, with the blackneſs of the cloud from which it is reflected; in the ſame manner as the colours of all bodies ſeen by reflexion are tinged with the colour of the reflecting ſurface.* He makes many other obſervations on the rainbow, particularly, on the height of it, with reſpect to that of the ſun; by which he demonſtrates that, in ſome countries, a rainbow cannot be ſeen at midday, namely, when the ſun is higher than the ſemi-diameter of the bow, the centers of the ſun and of the bow being always in a right line that paſſes through the eye of the ſpectator. Some, ſays he, have obſerved that the height of the bow and of the ſun together are always juſt 42 degrees; but he contends that the refraction of the atmoſphere will make ſome ſmall difference in this caſe.†

As this writer had ſo very imperfect a knowledge of the rainbow, it is not worth my while to recite his obſervations on halo's, and parhelia, to the explication of which he and his predeceſſors were ſtill more unequal.

This philoſopher contrived to exhibit colours *ſimilar*, as he calls them, to thoſe of the rainbow, in a circular glaſs veſſel full of water. For, laying a piece of white paper under ſuch a veſſel, expoſed to the rays of the ſun, he obſerved that it was tinged with ſuch colours, which, he ſays, are more beautiful when the veſſel is wetted on the outſide. But theſe, he ſays, are not the ſame colours with thoſe of the rainbow, becauſe they are not in the ſame number, and do not come to the eye by reflexion, but are ſeen by direct viſion, in the ſame manner as light itſelf, or any other colours. This is plainly a very inadequate deſcription of this appearance, and his reaſoning upon it, ſhews how very little was underſtood of this ſubject, in his time. He doth not ſeem to be aware that the circular form of his veſſel was not a neceſſary circumſtance in the experiment. *

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* Optica, p. 461.

† Ib. p. 471.

Vitellio makes ſome ingenious attempts to explain refraction, or to aſcertain the law of it. He alſo conſiders the foci of glaſs ſpheres, and the apparent ſize of objects ſeen through them; but Montucla ſays, § that he is not at all exact upon any of theſe ſubjects.

B. Porta thinks Vitellio to be almoſt always wrong when he departs from Alhazen, whoſe ape he calls him. † This he obſerves with reſpect to his miſtake concerning the place to which the rays of the ſun would be refracted in a glaſs globe. For Vitellio aſſerts that, becauſe the focus of the perpendicular rays will be the center, its greateſt power will, probably be there; and becauſe an experiment of its burning power could not be made in that place, he wiſhed that a hemiſphere, or a ſtill leſs portion of a ſphere might be made, in order to make the trial.§§

But Porta ſhould have done Vitellio the juſtice to obſerve, that he at the ſame time demonſtrates the poſſibility of firing bodies placed beyond the ſphere, by the rays of the ſun tranſmitted through it.‡ It is ſufficient to ſhew the ſtate of knowledge, or rather of ignorance in theſe times, that both Vitellio and his maſter Alhazen, endeavour to account for objects appearing larger, when they are ſeen under water, by the circular figure of its ſurface; ſince, being fluid, it conforms to the figure of the earth.||

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* Optica, p. 474. § Hiſtoire, Vol. 1, p. 625. † De Refractione, p. 76. §§ Ib. p. 64.
‡ Optica, p. 443. || Alhazen's Optics, p. 276. Vitellio's, p. 440.

Ten years after the publication of Vitellio's work, Peccam, Archbiſhop of Canterbury, wrote a treatiſe of *direct opticks,* which was then called *perſpective,* by which was underſtood viſion without reflexion or refraction; but it does not appear that he made any addition to the ſtock of optical knowledge. His treatiſe is conciſe and judicious, and among other things it contains a very clear and diſtinct account of the reaſon why the ſky near the horizon appears more diſtant from us than it does at any other place, p. 20.

Cotemporary with Vitellio and Peccam was our countryman Roger Bacon, a man of an almoſt univerſal genius, and who wrote upon almoſt every branch of ſcience. He frequently quotes Alhazen on the ſubject of opticks, and ſeems to have carefully ſtudied his writings, as well as thoſe of other Arabians, which were the fountains of natural knowledge in thoſe days, and which had been introduced into Europe by means of the Moors in Spain. Notwithſtanding the pains this great man took with the ſubject of opticks, it does not appear that, with reſpect to theory, he made any conſiderable advance upon what Alhazen had done before him.

Great as Bacon undoubtedly was, he was far from being free from the miſtakes and prejudices of thoſe who went before him. Even ſome of the wildeſt and moſt abſurd opinions of the antients have the ſanction of his approbation and authority. He does not heſitate to aſſent to an opinion adopted by many of the antients, and indeed by moſt philoſophers till his time, that viſual rays proceed from the eye; giving this reaſon for it, that every thing in nature is qualified to diſcharge its proper functions by its own powers, in the ſame manner as the ſun, and other celeſtial bodies.* He acknowledges, however, that the preſence of light, as well as ſeveral other circumſtances, is neceſſary to viſion. †

In his *Specula Mathematica* he endeavoured to ſolve the problem of Alhazen, concerning the *foci* of ſpherical mirrors, and added ſome obſervations on the refraction of the light of the ſtars, on the apparent ſize of objects, on the extraordinary ſize of the ſun and moon in the horizon, and upon of the roundneſs the image of the ſun made by rays of light paſſing through an angular aperture; a phenomenon which had employed Ariſtotle and his diſciples. But on all theſe ſubjects Bacon did very little more than his maſter, and what he advances concerning them is not very exact. §

In his *Opus Majus* he demonſtrates, that if a tranſparent body interpoſed between the eye, and an object be convex towards the eye, the object will appear magnified. This obſervation our author certainly had from Alhazen, from whom I have already quoted it. He only recommends the ſmaller ſegment of a ſphere, whereas Alhazen prefers the larger, for which, as Dr. Smith obſerves, he had good reaſon. There can hardly be any doubt, however, but that Bacon had ſeen objects magnified with his leſſer ſegment of a ſphere. It is not, in the leaſt degree, probable, that any perſon ſhould conjecture ſo much from ſpeculation only. Optical knowledge was then too much in its infancy for this purpoſe. Beſides, this writer gives us figures, repreſenting the progreſs of rays of light through his ſpherical ſegment, as well as endeavours to give reaſons why objects are

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[*] Opus Majus, p. 289. [†] Ib. p. 290. [§] Saverien's Hiſtoire, p. 244.

magnified by it. Theſe I ſhall recite from himſelf, but not exactly in his own words.

Let the object to be viewed, be ſuppoſed to be placed beyond the ſegment of the ſphere, fig. 11; and the rays iſſuing through the glaſs, at *b* and *m*, will be refracted, ſo as to make the image in the eye equal to *gr*, and larger than *ac*, which it would have been without the glaſs.

The miſtakes that he makes in his reaſoning upon this ſubject ſeem to ſhew that he wrote from obſervation. When the object, he ſays, is placed nearer to the eye than the center of the glaſs (in conſequence of its being a leſſer ſegment of the ſphere) and the convex ſide of the glaſs is towards the eye, every thing concurs to magnify it. For the angle under which it is ſeen is greater, the image is larger, and the place of the image is nearer. And therefore, ſays he, this inſtrument *(inſtrumentum)* is uſeful to old perſons, and thoſe who have weak eyes. For by the help of it they may ſee the ſmalleſt letters ſufficiently large. But, ſays he, if a larger ſegment of a ſphere, or only one half of it be made uſe of; then, indeed, the angle of viſion is greater, and the image is larger, but it is not ſo near, lying beyond the object.*

From the writings of Alhazen and theſe obſervations and experiments (as I muſt call them) of Bacon together, it is not improbable that ſome monks gradually hit upon the conſtruction of ſpectacles, to which Bacon's leſſer ſegment, notwithſtanding his miſtake concerning it, was a nearer approach than Alhazen's larger one. Whoever they were that purſued the diſcoveries of Bacon, they probably obſerved, that a very ſmall convex glaſs, when held at a greater diſtance from the book, would magnify the letters more than when it was placed cloſe to them, in which poſition only Bacon ſeems to have uſed it. In the next place, they might try whether two of theſe ſmall ſegments of a ſphere placed together, or a glaſs convex on both ſides, would not magnify more than one of them. They would then find, that two of theſe glaſſes, one for each eye, would anſwer the purpoſe of reading better than one; and laſtly they might find, that different degrees of convexity, ſuited different perſons.

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* Opus Majus, by Jebb, p. 352.

It is certain that ſpectacles were well known in the 13th century, and not long before. It is ſaid that Alexander Spina, a native of Piſa, who died in 1313, and who was very ingenious in executing whatever he ſaw or heard of, as having been done by others, happened to ſee a pair of ſpectacles in the hands of a perſon who would not explain them to him, but that he ſucceeded in making a pair for himſelf, and immediately made the conſtruction public, for the good of others.* I, alſo, find that it is inſcribed on the tomb of Salvinus Armatus, a nobleman of Florence, who died 1317, that he was the inventor of ſpectacles; but this is the only authority that I have met with for the fact.† It is pretty evident, from Vitellio's want of a hemiſphere, or a leſs portion of a ſphere of glaſs, as was mentioned above, that he was not acquainted with ſpectacles.

It would certainly have been a great ſatisfaction to us to have been able to trace all the actual ſteps in the progreſs of this moſt uſeful invention, without which moſt perſons who have a taſte for reading muſt have had the melancholy proſpect of paſſing a very dull and joyleſs old age; and muſt have been deprived of the pleaſure of entertaining themſelves by converſing with the abſent and the dead, when they were no longer capable of acting their part among the living. Teleſcopes and microſcopes are to be numbered among the ſuperfluities of life when compared with ſpectacles, which may now be ranked almoſt among the neceſſaries of it; ſince the arts of reading and writing are almoſt univerſal.

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* Smith's Opticks. Remarks, p. 12. † Muſchenbroeck's Introduct. Vol. 2, p. 786.

The uſe of concave glaſſes, to help thoſe perſons who are ſhort ſighted, was, probably, a diſcovery that did not follow long after that of convex ones, for the relief of thoſe whoſe ſight is defective in the contrary extreme, though we find no trace of this improvement. At leaſt Maurolycus, who is the firſt philoſopher that makes his appearance in the next period of our hiſtory, explains the uſe of both kinds of glaſſes in their reſpective applications. Whoever made this diſcovery, it was, probably, the reſult of nothing more than a random experiment. Perhaps a perſon who was ſhort ſighted, finding that convex glaſſes did him more harm than good, had the curioſity to make trial of a contrary curvature of the glaſs.

Bacon, in his Opus Majus, throws out a happy thought concerning another advantage that, he imagined, might be drawn from the doctrine of the refraction of light. For he conceived that it was poſſible, by means ſimilar to thoſe by which ſmall and near objects are magnified, to bring remote objects nearer, or remove them to greater diſtances; in ſhort, both to enlarge and diminiſh their dimenſions, even *ad infinitum*, ſo as to bring the ſun and moon upon the earth. He even deſcribes the method of accompliſhing this. He was alſo ſatisfied that one might to a great advantage view objects in a concave mirror, how diſtant ſoever they were. *

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* Saverien's hiſtoire, p. 244.

In theſe aſſertions we clearly ſee ſome approach towards the diſcovery both of teleſcopes and microſcopes, and conſidering how unlikely it is that a writer of ſo early an age ſhould have been able to have hit upon the theory of theſe inſtruments, by any reaſoning a priori; when even teleſcopes had actually been uſed many years, before any tolerable account was given of the *rationale* of them, we can hardly help concluding, that this friar had actually put glaſſes together, in ſuch a manner as to have produced, at leaſt, ſomething like the effects that he mentions, though he did not complete the conſtruction of thoſe admirable inſtruments. Dr. Smith, however, who had certainly conſidered this ſubject, and who muſt be allowed to have been a very good judge concerning it, is of opinion, that Bacon wrote only hypothetically, without having made any actual trial of the things he mentions. He is led to this concluſion by obſerving that our author mentions ſeveral things, as to be done by glaſſes, that are abſolutely impoſſible. But it is no uncommon thing for a perſon who has done a little towards any new diſcovery, to be too ſanguine in his expectations from it. There is ſome more weight, however, in his obſerving that Bacon makes no mention of any particular trials that he made of the uſe of his inſtruments.*

This writer alſo obſerves, that perſons who have not been uſed to teleſcopes expect that when they view any object, as the face of a man, through a teleſcope that magnifies 100 times, it will appear even larger than it does when they are cloſe to it; and he was ſatisfied that this was Bacon's notion of the matter, and for this reaſon he concludes that he had never looked through a teleſcope.†

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* Smith's Opticks, Remarks, p. 22.

† Ib. p. 41.

PERIOD II.

FROM THE REVIVAL OF LETTERS IN EUROPE TO THE DISCOVERIES OF SNELLIUS AND DESCARTES.

SECTION I.

DISCOVERIES IN OPTICKS INDEPENDENT OF TELESCOPES AND MICROSCOPES, AND BEFORE THOSE OF KEPLER.

FROM the origin of philoſophy among the Greeks to the commencement of the period I am now entering upon, there were no leſs than two thouſand years. In this long interval, there was never any failure of perſons who gave attention to the buſineſs of philoſophy, though ſome portions of the time were more fruitful in this kind of literature than others; and the obſervation of the phenomena of nature, and the inveſtigation of its powers always held a conſiderable rank in the eſteem of men of letters: and yet, though I have endeavoured to make the moſt of what remains concerning the knowledge of opticks within this period, collecting from all nations indiſcriminately, the reader has ſeen to how little it amounts. Some important obſervations did not, and, indeed, could not well eſcape the notice of the antients, and conſequently we have ſeen a foundation laid for ſome uſeful branches of the ſyſtem of opticks; but we have hardly ſeen one juſt idea concerning the *cauſe* of any appearance, nothing of the true *philoſophy* of light or viſion. Though ſpectacles were conſtructed towards the cloſe of the abovementioned period, and uſed by philoſophers, as much as others, it was ſome centuries, before any ſatisfactory account was given of their nature and effects.

To thoſe who conſider the world as having now arrived at its ſtate of perfect manhood with reſpect to ſcience, two thouſand years muſt appear a very diſproportionate term of infancy. But he who conſiders that no bounds can be ſet to our advances in this kind of knowledge (ſince the works of God are, like their author, infinite) that every new diſcovery is but an opening to ſeveral more, and, conſequently, that the progreſs of real knowledge may be expected to go on, not merely in an uniform manner, but to be conſtantly accelerated; and who ſhall reflect upon the aſtoniſhing improvements that have been made in this branch, and, indeed, in all the branches of real knowledge, in little more than two centuries that have elapſed ſince the expiration of that long period of darkneſs, cannot help forming the moſt glorious expectations. He will be looking for new light and new ſatisfaction every day; and, conſidering what has been done in very late years, together with the increaſing avidity with which the ſtudy of nature is now purſued, and the greater numbers, in all parts of Europe and America, who are applying themſelves to it, he may be almoſt aſſured that he will not be diſappointed, provided the ſources of information be eaſily open to him.

By the *revival of letters* in Europe, we mean, chiefly, the revival of what is generally called *polite literature*, which, at that time conſiſted, principally, in the ſtudy of the Greek and Latin authors and antiquities. But in the ſame age there were not wanting other perſons, who employed themſelves in the cultivation of the more important branches of knowledge. One of the firſt who diſtinguiſhed himſelf, as a mathematician and philoſopher, was MAUROLYCUS, teacher of mathematics at Meſſina; and to him we are indebted for ſome capital improvements in the ſcience of optics.

This writer, in his treatiſe, *De Lumine et umbra*, publiſhed in 1575, demonſtrates that the cryſtaline humour of the eye is a lens, that collects the rays of light iſſuing from external objects, and throws them upon the retina, where is the focus of each pencil. From this principle he was led to diſcover the reaſon why ſome perſons are, what is called, *ſhort ſighted*, and others *long ſighted*. In the former caſe he ſhews that the pencils of rays converge too faſt, and come to a focus before they reach the retina. On the other hand, he ſhews that, in the eye of a Preſbyte, or a perſon who is long ſighted, thoſe pencils do not converge faſt enough; ſo that the foci are beyond the retina; and that in both theſe caſes viſion is indiſtinct. From the ſame principles he diſcovered the reaſon why ſhort-ſighted perſons are relieved by concave glaſſes, and long-ſighted perſons by convex ones.* A concave lens makes the rays diverge more before they enter the pupil of the eye, and conſequently makes it more difficult for them to converge ſo faſt as they would have done before, after paſſing the cryſtaline humour, whereas a convex lens makes the rays converge more, and faſter than they would otherwiſe have done.

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* Montucla's Hiſtoire, Vol. 1, p. 626.

It is ſurprizing that the perſon who diſcovered the proper uſe of the cryſtaline humour, as a lens, to collect the rays of light; and to bring every pencil that enters the eye to its proper focus, ſhould not have advanced what is, indeed, in reality, hardly a ſtep farther, and have found that, by this means, the rays of light iſſuing, in pencils, from every point of an object, make a real image of it upon the retina. But he does not ſeem to have been acquainted with this circumſtance relating to the theory of viſion. Montucla conjectures that he was prevented from coming to this concluſion by the difficulty of accounting for objects appearing upright, when their images, made by rays of light upon the retina, muſt be inverted. Even Kepler, who made this diſcovery, was very near miſſing it on the ſame account.

To make theſe diſcoveries concerning the nature of viſion intelligible to my younger readers, I muſt, in this place, give ſo much of the deſcription of the eye as may be neceſſary for this purpoſe. This inſtrument of viſion, repreſented in fig. 12, is nearly in the form of a ſphere, divided into three different cavities, which are filled with what are generally called *humours*, though only two of them are liquid. The eye has alſo three *coats*, or integuments; of which the outermoſt, *aa*, is called the *ſclerotica*, and is very thick and ſtrong, and the fore part of it, AA, called the *cornea*, is tranſparent, and at the ſame time a little protuberant. The next, *cc*, called the *choroides*, has a perforation at *pp*, which, being viſible through the tranſparent cornea, is called the *pupil.* The reſt of the choroides *bp*, *bp*, which lies under the cornea, is differently coloured in different perſons, and is called the *iris*. It conſiſts of muſcular fibres, ſome of which are circular, and others croſſing them at right angles; by the former of which the pupil is contracted, and by the latter dilated.

At a ſmall diſtance beyond the pupil, there is ſuſpended a ſoft but tranſparent ſubſtance, CC, in the form of a double convex lens, called the *cryſtalline humour*; and the ligaments *l l*, by which it is ſuſpended, are called *ligamenta ciliaria*, or *proceſſus ciliares*. On the back part of the eye, but not exactly oppoſite to the pupil, is the inſertion of the *optic nerve*, NN, the expanſion of which each way, as far as the ligamenta ciliaria, makes what is called the *retina*.

Beſides the two coats above mentioned, a third was diſcovered by Ruyſch, and from him it is called *tunica Ruyſchiana*. It adheres cloſely to the choroides, except that it is ſeparated from it at the ligamenta ciliaria, and goes on the back of the cryſtalline humour. All the inſide of the eye, except the back part of the cryſtalline humour, and that part of the retina which is oppoſite to it, is covered with a thick black ſubſtance, which abſorbs all the light that is reflected upon it from the retina, and which would otherwiſe have been reflected back again upon it, and have made the images of objects indiſtinct.

The cavity contained between the cryſtalline humour and the cornea is filled with a thin liquor, called the *aqueous humour*, and the larger cavity, beyond the cryſtalline humour, is occupied by what is called the *vitreous humour*.

According to Maurolycus, therefore, the cryſtalline humour HH, fig. 13, is a lens, by means of which pencils of rays coming from every point of any external object, as the arrow STR, are collected into foci upon the retina at *str*.

The ſame philoſopher alſo, hit upon the true ſolution of the problem concerning the image of the ſun appearing round, though the rays that form it are tranſmitted into a dark room through an angular aperture. For he conſidered that, ſince rays of light are continually proceeding from every part of the ſun's diſk, they muſt be croſſing one another from the extreme edge of it, in every point of the aperture; ſo that every ſuch point will be the apex of two cones; of which the baſe of the one is the ſun's diſk, and that of the other his image, on the oppoſite wall, ſubtending an equal angle. Conſequently, if theſe images be taken at a conſiderable diſtance from the aperture, and therefore be pretty large, while the aperture itſelf is ſmall; ſince the whole image conſiſts of a number of images, all of which are circular, the image of the ſun formed by the aperture, of whatever form it be, muſt be circular alſo; and it will approach the nearer to a perfect circle, the ſmaller is the aperture, and the more diſtant the image.*

About the ſame time that Maurolycus made ſuch great advances towards the diſcovery of the nature of viſion, Johannes Baptiſta Porta, of Naples, made a curious diſcovery, which throws ſtill more light upon the ſame ſubject. For his was the invention of the *Camera Obſcura*, which furniſhes one of the moſt amuſing and pleaſing experiments in the whole compaſs of opticks.

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* Montucla's Hiſtoire, p. 627. Saverien's, p. 245.

This gentleman was particularly attentive to the ſtudy of medicine and natural philoſophy, and he ſpared no pains or expence, either in travelling, to collect accounts of every thing that was curious, or in making experiments, to aſcertain the truth of what he had been informed; and, conſidering the great number of vulgar errors that prevailed in his time, it can be no wonder that traces of not a few of them ſhould ſtill be found in his writings. His houſe was conſtantly reſorted to by all the ingenious perſons at Naples, whom he formed into what he called *an academy of ſecrets*, each member being obliged to contribute ſomething that was not generally known, and that might be uſeful.

By this means he was furniſhed with materials for his *Magia Naturalis*, which contains his account of the *camera obſcura*, and the firſt edition of which was publiſhed, as he informs us, when he was not quite fifteen years old, that is, about the year 1560, 15 years before the publication of Maurolycus's treatiſe above mentioned. On this account I might have conſidered him as the earlier of the two; but Maurolycus being the much older man (for he died at 81, in the very year in which his book was publiſhed) and not being certain that the account of the camera obſcura was in the firſt edition of the Magia Naturalis, which was republiſhed thirty years after, with great enlargements; I have choſe to place thoſe philoſophers according to their age, eſpecially as they ſeem to have had no connection with one another; for I do not recollect that Porta ever quotes Maurolycus, or, indeed, that he appears to have been acquainted with his diſcoveries, which, I own, ſurpriſes me much.

The court of Rome took umbrage at the aſſemblies that were held at B. Porta's houſe for the purpoſe of natural knowledge, and forbad them; which will not be wondered at, conſidering the time in which he flouriſhed, though he ſeems to have been a very good catholic. For, ſpeaking of a Frenchman who had called him a magician and a conjuror, he found, he ſays, upon inquiry, that he was a heretic, who by throwing himſelf headlong from a tower, very narrowly eſcaped being maſſacred at Paris on St. Bartholomew's day, and he adds, that, as becomes a nobleman and a chriſtian, he ſincerely prayed to God for his converſion to the catholic faith, that he might eſcape being burned alive. See the Preface to Magia Naturalis.

This book was immediately on its firſt publication tranſlated into Italian, French, Spaniſh, and Arabic, and went through many editions in various countries. It is an amazing collection of every thing that was known in his time, and that was wonderful or curious, in the whole compaſs of nature or art. B. Porta's manner of compoſition is generally very agreeable. He appears to have been well acquainted with the opinions of the antients on all philoſophical ſubjects, and makes uſe of his learning to embelliſh his own writings. This is particularly the caſe with reſpect to his treatiſe on refraction.

His deſcription of the camera obſcura is not large, but ſufficiently intelligible. In the 17th chapter of his Magia Naturalis, he obſerves, that, if a ſmall hole be made in the ſhutter of a window, all external objects will be viſible, in their proper colours, on the oppoſite wall. And, farther, that if a convex lens be fixed in the hole, the images, in the focus of that lens will be rendered much more diſtinct; ſo diſtinct, he ſays, that even the features of perſons out of the room may be diſtinguiſhed by thoſe within.

Many perſons, he ſays, have tried to make the images of external objects appear erect, and not inverted, as they commonly are in the camera obſcura, but he ſays it was to no purpoſe, ſince if the images were erect, they were ſo much obſcured, that it was no pleaſure to view them; but he ſays that this end will be gained, if the images be received upon a concave mirror, properly adapted to the convex lens, and held at a juſt diſtance from the hole. The effect of this expedient, he ſays, cannot be ſufficiently admired.

B. Porta, did not overlook ſome obvious uſes of his contrivance, for he obſerves, that, by means of his camera obſcura, the pictures of perſons and things may be drawn with great exactneſs, and that eclipſes of the ſun may be viewed to great advantage in this manner. *

A drawing may aſſiſt the reader in conceiving diſtinctly the effects of a camera obſcura, and if he will conſult fig. 14, he will ſee a houſe, and a tree behind it, repreſented in this way; the figure within the room correſponding to the object without, but diminiſhed, and alſo inverted, becauſe the rays that enter the room from all parts of the external object, neceſſarily croſs one another in the hole, through which they are admitted.

B. Porta, finding that he could exhibit the appearance of real external objects upon his white wall, amuſed himſelf and his friends with ſubſtituting ſmall images, ſo diſpoſed, as that they might be magnified by his lens in the window ſhutter. Juſt before his lens, he fixed a hollow cube of paper, the front ſide of which was very thin; and upon this he drew whatever objects he pleaſed; that, being placed at a proper diſtance from the lens, when the ſun's

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* Magia Naturalis, Lib. 17. Cap. 6.

rays came into the room, the form of the images might be ſeen upon the wall. And as, by making this front ſide of the cube moveable, he could give his images what motion he pleaſed, the effect, in thoſe rude ages, ſeemed to proceed from the power of magic. In this manner, he ſays, that he exhibited the appearance of a hunting, of a battle, and other extraordinary ſights, to the aſtoniſhment of all the ſpectators. This contrivance gave the hint to Kircher, on which he conſtructed the *magic lantern*, doing that in the night, and in many reſpects more conveniently, which Porta exhibited in the day. *

In the manner of B. Porta, Kircher ſays, that he once ſaw an admirable repreſentation of the crucifixion; and in the ſame method the Emperor Rodolph was entertained, by the mathematicians of his age, with a proceſſion of all the Roman Emperors, from Julius Cæſar to himſelf.

B. Porta's experiments with the camera obſcura, convinced him that viſion is performed by the *intromiſſion* of ſomething into the eye, and not by *viſual rays*, proceeding from the eye, as had been the general opinion before his time; and he was the firſt who fully ſatisfied himſelf and others upon this ſubject; though ſeveral philoſophers ſtill adhered to the old opinion. He gives an excellent enumeration of the opinions of the antients upon this ſubject. † Indeed, the reſemblance between experiments in the camera obſcura, and the manner in which viſion is performed in the eye, was too ſtriking to eſcape the obſervation of a leſs ingenious perſon. But when he ſays that the eye is a camera obſcura, and the pupil the hole in the window ſhutter, he was ſo far miſtaken, as to ſuppoſe that it was the cryſtalline humour that correſponds to the wall which receives the images. It was Kepler who, in the year 1604, firſt obſerved that the retina is the tablet, on which the images of external objects are depicted.[*]

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* Schott's Magia Univerſalis, Vol. 1, p. 198. † De Refractione, p. 87.

B. Porta ſays, that the images of external objects are tranſmitted through the pupil to the cryſtalline, to which it is ſubſervient, like a faithful porter.[†] That the cryſtalline is the principal organ of viſion, he thought was evident from its ſituation, in the center of the eye, which is the principal place, and where the neighbouring parts of the eye may moſt conveniently miniſter to it; and he very particularly explains the manner in which he ſuppoſes that each of them is ſubſervient to it.[§]

In order to give ſome idea of the ſtate of optical knowledge in the age of B. Porta, I ſhall recite ſome more of his obſervations reſpecting viſion, the theory of light in general, and its principal phenomena.

He makes a variety of juſt obſervations concerning viſion, and particularly explains ſeveral caſes in which we may imagine things to be without the eye, when the appearances are occaſioned by ſome affection of the eye itſelf, or ſome motion within the eye. And, after examining various opinions concerning perſons ſeeing in the dark, upon firſt opening their eyes in a morning, he ſhews very clearly, that this is chiefly owing to the great dilatation of the pupil in thoſe circumſtances.[||]

He obſerves that the pupil is contracted involuntarily when it is expoſed to a ſtrong light, and opens of itſelf when the light is ſmall. Any perſon, he ſays, may ſatisfy himſelf with reſpect to this circumſtance by looking upon the eye of another perſon, firſt when he ſtands with his face towards the ſun, and immediately after in a ſhady place. *

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[*] De Refractione, p. 91. [†] Ib. p. 73. [§] Ib. p. 87. [||] Ib. p. 158.

It is poſſible, however, that this might not be an obſervation of B. Porta's own, as it is pretty evident that it was made by his countryman and cotemporary, the famous Father Paul of Venice. Galen firſt obſerved the dilatation of the pupil of one eye when the other was ſhut or loſt, and the contraction of it when the other was opened or recovered; and in this opinion he was followed by all naturaliſts and philoſophers, till Fabricius ab Aquapendente, profeſſor at Padua, and ſucceſſor to the great Fallopius, by obſerving the eye of a cat, found that the pupil not only dilated and contracted itſelf when one of the eyes was ſhut, but alſo when both were open.

This unexpected diſcovery at firſt very much ſurpriſed Aquapendente, becauſe it was contrary to the prevailing opinion founded on the authority of Galen; more eſpecially as he obſerved that the pupils of both eyes contracted and dilated frequently in a very ſhort ſpace of time, though the light continued the ſame. This circumſtance led him to ſuſpect that theſe motions were altogether voluntary, like that of other muſcles, though he was at a loſs how to account for it, as he could find no muſcles for performing theſe motions; and as he does not ſeem to have known any thing of the circular and longitudinal muſcular fibres of the uvea or iris. But having communicated theſe obſervations to ſome of his learned friends, and in particular to Father Paul, this great man found, from repeated experiments, that the pupil not only of cats, but alſo of men, and other animals, always contracted itſelf when the eye was expoſed

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* De Refractione, p. 74.

to a bright light, and again dilated itſelf when the light was faint and languid. This he always obſerved, as well when both eyes were open, as when one of them was ſhut: and therefore this learned father juſtly deſerves the honour that is due to this diſcovery. The ſame remark had indeed been made by Achillinus, in a treatiſe publiſhed in 1522, but it ſeems to have been overlooked by philoſophers. [*]

Without ſeeming to be acquainted with any thing that had been done by Maurolycus, or any perſon elſe, B. Porta gives reaſons why the ſight is relieved by convex and concave glaſſes. [||] But his opinions on this ſubject cannot be ſuppoſed to be of much value, when he accounts for old perſons being obliged to place objects at a greater diſtance than young perſons, in order to ſee them diſtinctly, to the greater dilatation of their pupils, and to the thickneſs of the humours of the eye. [†] Whereas ſome young men, he ſays, do not ſee diſtinctly, on account of their pupils being too narrow. Such perſons he ſays, are relieved by concave glaſſes. [§]

He recites at length all the hypotheſes concerning the cauſe of ſingle viſion with two eyes, and concludes that, in fact, we never ſee with more than one eye at one time, as he pretends to prove by experiment. Odd as this opinion is, and contrary to many plain facts, it was maintained by writers in a more enlightened age than that of B. Porta.

After obſerving that a concave lens will aſſiſt a perſon in viewing remote objects, and a convex one thoſe that are near, he adds that, if a perſon can arrange them properly, he will be aſſiſted in diſtinguiſhing objects both near at hand, and at a diſtance from him. He even ſays, that he himſelf had, by this means, put ſome of his friends, whoſe ſight was not very good, into the way of ſeeing more diſtinctly. This muſt be acknowledged, as in the caſe of Friar Bacon, to look like the conſtruction of a teleſcope. Yet other conſiderations make it probable, that this Neapolitan was never in poſſeſſion of any ſuch inſtrument, and that he only lengthened or ſhortened the foci of ſingle lenſes by his combination of them. *

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[*] Porterfield on the eye, Vol. 2, p. 93. [||] De Refractione, p. 175. [†] Ib. p. 137. [§] Ib. p. 188.

B. Porta makes the following juſt obſervation concerning the effects of refraction, viz. that a round and flat ſurface, plunged in water, will appear hollow, as well as magnified, to an eye perpendicularly over it, the extremities being elevated by refraction; and he very well explains the manner in which it is done by a figure. † A particular inveſtigation of the nature of this curvature will be found in a later period of this hiſtory. This diſcovery, if it may be called one, our author certainly made by actual obſervation; for it cannot be ſuppoſed that he could have concluded ſo much by reaſoning *a priori*. But it muſt have been from reaſoning only, and not experiment, that he aſſerted that gunpowder may be fired at a great diſtance, and that caſtles and rocks may be blown up by it, if the rays of the ſun be firſt brought to a focus by a concave mirror, and be tranſmitted parallel, by a plane mirror, to the diſtance required.

Alhazen had adopted the opinion, that light is not tranſmitted inſtantaneouſly, grounding his opinion on ſubtle and metaphyſical reaſonings, which B. Porta undertakes to refute, and to prove that the paſſage of light is really inſtantaneous; but excepting that he ſays the contrary is not agreeable to obſervation, his own arguments are equally metaphyſical. One of them is derived from the conſideration of light being incorporeal.*

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* Montucla's Hiſtoire, Vol. 1, p. 629. † De Refractione, p. 19.

B. Porta alſo maintains the diſtinction between true and apparent colour, adopted by the antients, and treats of the difference between them.† He was of opinion, that light itſelf is colourleſs, but that it is capable of having colour communicated to it, or rather ſuperinduced upon it, from foreign cauſes, as from tranſparent ſubſtances, in certain circumſtances.§ He maintained that refraction, and not reflexion, was the cauſe of colour in the rainbow, but he did not mean refraction in ſingle drops, but in the whole maſs of falling rain, as one medium, of a different denſity from the air;‡ and he gives a particular account of the generation of each of the colours of the rainbow, from a mixture of light and the denſer or rarer parts of the air; diſliking the opinion of moſt of the antients, that colour conſiſts of a mixture of light and darkneſs.‖

The great Lord Bacon, who was cotemporary with B. Porta, but the younger man of the two, and who had ſo juſt an idea of the imperfect ſtate of natural ſcience in his time, and of the cauſes of it, and who ſuggeſted ſo many admirable hints for advancing the bounds of it (hints which have been purſued to great advantage ſince his time) complains that the *form* and *origin of light* had been but little inquired into; and he mentions ſeveral facts, which ſhew how little philoſophers were then acquainted with the ſubject. This neglect, he thought, was extraordinary, conſidering what pains had been taken with the ſubject of Perſpective. But in this branch of ſcience he obſerves, that the progreſs of rays of light is conſidered *mathematically* only, and that writers upon it never paid much attention to them *phyſically* conſidered. This, therefore, he places among the *deſiderata* of his time, and deſires that inquiry may be made into it. *

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* De Refractione, p. 95. † Ib. p. 215. § Ib. p. 190. ‡ Ib. p. 202. ‖ Ib. p. 190.

In another work he mentions twelve different heads of inquiry concerning light, the peruſal of which cannot but give pleaſure to thoſe who are attentive to the progreſs of ſcience; as they demonſtrate both how very imperfectly this ſubject was underſtood in his time, and how ſenſible he was of that imperfection. One of the doubts which this great man wiſhed to have removed by experiment, was, whether light be diffuſed equally in all directions; for heat, he ſuppoſed, to aſcend: alſo, whether air be neceſſary to the propagation of light, and whether, like ſound, it be not obſtructed by winds. † Things viſible and audible, he ſays, agree in this, that they neither emit any corporeal ſubſtance, nor excite any manifeſt motion in the ſurrounding medium, but only certain *ſpiritual ſpecies (ſpecies ſpirituales)* the nature of which is altogether unknown. § He thought that air was the moſt convenient medium both of ſound and viſion, and that a ſtrong wind would not obſtruct the paſſage of light ſo much as it does that of ſound. ‡

The extreme caution with which this philoſopher would have us proceed in our inquiries into nature, appears from his heſitating to infer, that, becauſe objects under water, which is a denſer medium than air, appear magnified to an eye in the air, objects in the air will appear diminiſhed to an eye under water. *How this worketh,* ſays he, *I know not.* ||

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* De Augmentis Scientiarum in Bacon's Works, Vol. 2. p. 136. † Ib. Vol. 2, p. 213.
§ Ib. p. 198. ‡ Ib. p. 199. || Ib. p. 154.

How little a way men of the greateſt genius are able to proceed without actual experiments, even in things in which they have the cleareſt analogy to guide them, may be ſeen in Lord Bacon's ſuppoſing the poſſibility of ſeeing an image from a reflecting ſurface without ſeeing the reflecting ſurface itſelf. As the whole paſſage in which he mentions this circumſtance is not long, and extremely curious, I ſhall inſert it intire, as an encouragement to philoſophers not to be aſhamed of making random conjectures, if they have the moſt diſtant proſpect of their contributing to the diſcovery of truth, when they are countenanced by ſo great a man as Lord Bacon.

" It would be well bolted out, whether great refractions may " not be made upon reflections, as well as upon direct beams. " For example, we ſee that, take an empty baſon, put an angel " of gold, or what you will into it; then go ſo far from the " baſon till you cannot ſee the angel, becauſe it is not in a right " line; then fill the baſon with water, and you ſhall ſee it out " of its place, becauſe of the refraction. To proceed, there" fore, put a looking glaſs into a baſon of water. I ſuppoſe you " ſhall not ſee the image in a right line, or at equal angles, but " wide. I know not whether this experiment may not be ex" tended ſo as you might ſee the image and not the glaſs, which, " for beauty and ſtrangeneſs, were a fine proof, for then you " ſhould ſee the image, like a ſpirit in the air. As for example, " if there be a ciſtern, or pool of water, you ſhall place over a" gainſt it the picture of the devil, or what you will, ſo as that " you do not ſee the water. Then put a looking-glaſs in the " water; now, if you can ſee the devil's picture aſide, not ſee" ing the water, it would look like the devil indeed. They have "an old tale in Oxford, that Friar Bacon walked between two "Steeples, which was thought to be done by glaſſes, when he "walked upon the ground." *

This whole buſineſs of ſeeing images in the air may be traced up to Vitellio, and what he ſaid upon the ſubject ſeems to have paſſed from writer to writer, with conſiderable additions, to the time of Lord Bacon. What Vitellio endeavours to ſhew is, that it is poſſible, by means of a cylindrical convex ſpeculum, to ſee the images of objects in the air, out of the ſpeculum, when the objects themſelves cannot be ſeen. † But if his deſcription of the apparatus requiſite for this experiment be attended to, it will be ſeen that the eye was to be directed towards the ſpeculum, which was placed within a room, while both the object and the ſpectator were without it. But, though he recommends this obſervation to the diligent ſtudy of his readers, he has not deſcribed it in ſuch a manner as is very intelligible; and, indeed, it is certain, that no ſuch effect can be produced by a convex mirror. If he himſelf did make any trial with the apparatus that he deſcribes for this purpoſe, he muſt have been under ſome deception with reſpect to it.

B. Porta ſays that this effect may be produced by a plane mirror only; and that an ingenious perſon may ſucceed in it; but his more particular deſcription of a method to produce this extraordinary appearance is by a plane mirror and a concave one combined. §

Kircher, alſo, ſpeaks of the poſſibility of exhibiting theſe pendulous images, and ſuppoſes that they are reflected from the denſe air; and the moſt perfect and pleaſing deception depending

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* Bacon's Works, Vol. 3, p. 155. † Optica, p. 308. Lib. 7. Cap. 60.
§ Magia Naturalis, Lib. 7. Cap. 5.

ing upon images in the air is one of which this writer gives a particular account in his *Ars Magna Lucis et Umbræ*, p. 783. In this caſe the image is placed at the bottom of a hollow poliſhed cylinder, by which means it appears like a real ſolid ſubſtance, ſuſpended within the mouth of the veſſel. In this manner, he ſays, he once exhibited a repreſentation of the aſcenſion of Chriſt; when the images were ſo perfect, that the ſpectators could not be perſuaded, but by attempting to handle them, that they were not real ſubſtances. A copy of the drawing he has given us of this inſtrument may be ſeen, fig. 15.

SECTION II.

DISCOVERIES CONCERNING THE RAINBOW.

ABOUT the ſame time that the effects of ſingle lenſes were engaging the attention of philoſophers, we find them diſcuſſing another optical phenomenon, which was much above the capacity of that age. This was the cauſe of the colours in the rainbow. How little the philoſophers of that time were prepared for ſuch an inquiry may be imagined, when it is conſidered, that they had no tolerable hypotheſis about the nature of colour in general.

What the antients, and the philoſophers of the middle ages, have written concerning the rainbow has been recited. Their obſervations were ſuch as could not have eſcaped the notice of the moſt illiterate huſbandmen who gazed at the ſky, and their hypotheſes were ſuch as I have mentioned for the amuſement, and not the inſtruction of my reader. It was a conſiderable time even after the dawn of true philoſophy in this weſtern part of the world, before we find any diſcovery of importance on this ſubject. Maurolycus was the firſt who pretended to have meaſured the diameters of the two rainbows, with much exactneſs, and he reports that he found that of the inner bow to be 45 degrees, and that of the outer bow 56; from which Deſcartes takes occaſion to obſerve, how little we can depend upon the obſervations of thoſe who were not acquainted with the cauſes of appearances.*

One Clichtovæus (the ſame, I ſuppoſe, who diſtinguiſhed himſelf by his oppoſition to Luther, and who died in 1543) had maintained, that the ſecond bow is the image of the firſt, as he thought was evident from the inverted order of the colours. For, ſaid he, when we look into the water, all the images that we ſee reflected by it are inverted with reſpect to the objects themſelves; the tops of the trees, for inſtance, that ſtand near the brink, appearing lower than the roots. But Gilbert, who wrote the treatiſe *De Magnete*, and whoſe diſcoveries in electricity I have related, takes upon himſelf to be very much offended at this account of the matter, and treats the author with very great ſeverity, calling his hypotheſis a *ſtupid* one, and *worthy of Ariſtotelian ſubtilty*; and he gravely anſwers him, by obſerving that, upon his principles, the whole *form* of the outer bow, as well as the *colours* of it, ought to be inverted with reſpect to the inner bow, and conſequently that its arched part ſhould be downwards.† However, the whole of Gilbert's own book abounds with ſcholaſtic refinement, and contains the moſt frivolous diſtinctions and definitions. This, indeed, was not his fault, but that of the time in which he lived, and of which he and his adverſary equally partook. None of Gilbert's opinions on any optical ſubjects are worth reciting.

That the rainbow is oppoſite to the ſun, had always been obſerved. It was, therefore, natural to imagine, that the colours of it were produced by ſome kind of reflexion of the rays of light from drops of rain, or vapour. The *regular order of the colours* was another circumſtance that could not have eſcaped the notice of any perſon. But, notwithſtanding mere reflexion had in no other caſe been obſerved to produce colours; and it could not but have been obſerved that refraction is frequently attended with that phenomenon, yet no perſon ſeems to have thought of having recourſe to a proper refraction in this caſe before one Fletcher of Breſlau, who, in a treatiſe which he publiſhed in 1571, endeavoured to account for the colours of the rainbow by means of a double refraction and one reflexion. But he imagined that a ray of light, after entering a drop of rain, and ſuffering a refraction, both at its entrance and exit, was afterwards reflected from another drop, before it reached the eye of the ſpectator. He ſeems to have overlooked the reflexion at the farther ſide of the drop, or to have imagined that all the bendings of the light within the drop would not make a ſufficient curvature, to bring the ray of the ſun to the eye of the ſpectator. That he ſhould think of two refractions was the neceſſary conſequence of his ſuppoſing that the ray entered the drop at all. This ſuppoſition, therefore, was all the light that he threw upon the ſubject, B. Porta, as we have ſeen, ſuppoſed that the rainbow is produced by the refraction of light in the whole body of rain or vapour, but not in the ſeparate drops.

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* Dioptrics, p. 224.

† Phyſiologia, p. 273.

After all, it was a man whom no writers allow to have had any pretenſions to philoſophy, that hit upon this curious diſcovery. This was Antonio De Dominis, Biſhop of Spalatro, whoſe treatiſe *De Radiis Viſus et lucis*, was publiſhed by J. Bartolus in 1611. He firſt advanced that the double refraction of Fletcher, with an intervening reflexion, was ſufficient to produce the colours of the bow, and alſo to bring the rays that formed them to the eye of the ſpectator, without any ſubſequent reflexion. He diſtinctly deſcribes the progreſs of a ray of light entering the upper part of the drop, where it ſuffers one refraction, and after being thereby thrown upon the back part of the inner ſurface, is from thence reflected to the lower part of the drop; at which place undergoing a ſecond refraction, it is thereby bent, ſo as to come directly to the eye. To verify this hypotheſis, this perſon (no philoſopher as he was) proceeded in a very ſenſible and philoſophical manner. For he procured a ſmall globe of ſolid glaſs, and viewing it when it was expoſed to the rays of the ſun, in the ſame manner in which he had ſuppoſed that the drops of rain were ſituated with reſpect to them, he actually obſerved the ſame colours which he had ſeen in the true rainbow, and in the ſame order. *

Thus the circumſtances in which the colours of the rainbow were formed, and the progreſs of a ray of light through a drop of water were clearly underſtood; but philoſophers were a long time at a loſs when they endeavoured to aſſign reaſons for all the particular colours, and for the order of them. Indeed nothing but the doctrine of the different refrangibility of the rays of light, which was a diſcovery reſerved for the great Sir Iſaac Newton, could furniſh a compleat ſolution of this difficulty. De Dominis ſuppoſed that the red rays were thoſe which had traverſed the leaſt ſpace in the inſide of a drop of water, and therefore retained more of their native force, and conſequently ſtriking the eye

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* Where Montucla, from whom I collect this circumſtance, was informed concerning this experiment of De Dominis, I cannot tell: for I have looked through his treatiſe pretty carefully, without being able to find any account of it. I am therefore, inclined to think, that Montucla has aſcribed too much to our author, and that Deſcartes was the firſt who verified the theory of the rainbow by an experiment with a glaſs globe.

more briſkly, and gave it a ſtronger ſenſation; that the green and blue colours were produced by thoſe rays, the force of which had been, in ſome meaſure, obtunded in paſſing through a greater body of water, and that all the intermediate colours were compoſed (according to the hypotheſis which generally prevailed at that time) of a mixture of theſe three primary ones.* That the different colours were cauſed by ſome difference in the impulſe of light upon the eye, and the greater or leſs impreſſion that was thereby made upon it, was an opinion which had been adopted by many perſons, who had ventured to depart from the authority of Ariſtotle.†

Afterwards the ſame De Dominis obſerved, that all the rays of the ſame colour muſt leave the drop of water in a part ſimilarly ſituated with reſpect to the eye, in order that each of the colours may appear in a circle, the center of which is a point of the heavens, in a line drawn from the ſun through the eye of the ſpectator. The red rays, he obſerved, muſt iſſue from the drop neareſt to the bottom of it, in order that the circle of red may be the outermoſt, and therefore the moſt elevated in the bow. §

The progreſs of a ray of light in the formation of the rainbow, according to the diſcovery of De Dominis, may be ſeen in fig. 16, where SD repreſents a ray of light proceeding from the ſun, and after entering the drop of water DH, at the upper part of it, D, is there refracted, and thrown upon the inſide of the middle part; and being thence reflected to the bottom of the drop H, is again refracted at its exit, before it falls upon the eye at A. In conſequence of theſe refractions, it is divided into rays of different colours, from the violet at V, to the red at R. Alſo, while one drop ſends a ray of one colour to the eye, another drop, ſituated a little higher or lower, ſends a ray of a different colour. For the eye is not wide enough to receive rays of all colours from the ſame drop. Or, though rays of all colours do reach the eye from every drop; yet all that are not effectual to conſtitute the rainbow are, either in ſo ſmall a quantity, or ſo equally mixed with others, of all the other colours, falling upon the ſame part of the eye (which produces mere whiteneſs) that their effect is not to be conſidered in this caſe. Thus while the drop CG, at the top of the bow, ſends a red ray to the eye at A, the drop DH, in the lower part of the bow, ſends a violet ray; and the intermediate drops ſend rays of intermediate colours. If a drop be ſituated higher than that which ſends the red light as B, or lower than that which ſends the violet, as E, no ray proceeding from them will reach the eye at all.

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* Montucla, Vol. 1. p. 630. † Saverien's Hiſtoire, p. 252. § Montucla, Vol. 1. p. 631.

It is no wonder that one who wrote ſo early as De Dominis, ſhould adopt many miſtakes concerning the nature of light and viſion, ſuch as the difference between *real* and *apparent* colour, and the opinion that colours ariſe from a mixture of light and ſhade. Following B. Porta, he contends that the ſeat of viſion is in the pupil, by which, like him, he means the ſurface of the cryſtalline, and that there is no refraction of light within the eye. * He gives the preference to the opinion of viſion being performed by *intromiſſion*, but he thinks all the phenomena are conſiſtent enough with the Platonic doctrine of the *emiſſion* of viſual rays. Denying the refractive power of the humours of the eye, it is no wonder that he was embarraſſed in explaining the cauſe of the defects of ſight, and the remedy of them by glaſſes. He thought with B. Porta, that the long ſighted were thoſe whoſe cryſtalline was too dry and hard, whereas in thoſe who were ſhort ſighted, he thought that it was too ſoft and moiſt. *

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* De Radiis, p. 6.

Notwithſtanding De Dominis conceived ſo juſtly of the manner in which the inner rainbow is formed, he was far from having as juſt an idea of the cauſe of the *exterior* bow. This he endeavoured to explain in the very ſame manner in which he had done the interior, viz. by one reflexion of the light within the drop, preceded and followed by a refraction; ſuppoſing only that the rays which formed the exterior bow, were returned to the eye by a part of the drop lower than that which tranſmitted the red of the interior bow. He alſo ſuppoſed that the rays which formed one of the bows came from the ſuperior part of the ſun's diſk, and thoſe which formed the other from the inferior part of it. He did not conſider that, upon thoſe principles, the two bows ought to have been contiguous, or rather, that an indefinite number of bows would have had their colours all intermixed, which would have been no bow at all. Montucla alſo ſays, that, upon this hypotheſis, the order of the colours in the two bows would be the ſame; but De Dominis endeavours to obviate that objection, ſhewing, by a diagram, that below the diameter of the drop, the rays which come to the eye from the lower part traverſe the leaſt ſpace of water; but that above the diameter, it is the very reverſe. §

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* De Radiis, p. 16. 18.

§ Ib. p. 63. Montucla, Vol. 1. p. 631.

SECTION

SECTION III.

OF THE INVENTION OF TELESCOPES AND MICROSCOPES, WITH THEIR FIRST IMPROVEMENTS.

IT was in this period of my hiſtory that mankind began to derive an advantage from the ſcience of opticks, which muſt have appeared, *a priori*, to have been out of the power of ſcience to beſtow. For who could have imagined that the refraction of light in glaſs, and other tranſparent ſubſtances, the ſame power by which a ſtraight rod appears crooked in water, whereby viſion is variouſly diſtorted, and whereby we are liable to innumerable deceptions, ſhould ever be ſo circumſtanced, as to extend the bounds of ſight, enabling us to diſtinguiſh objects vaſtly too remote, or too ſmall, for our natural organs. Upon this principle, however, depends the conſtruction of the *teleſcope*, which not only gives us a diſtinct view of diſtant terreſtrial objects, without the trouble of conveying ourſelves to them, but enables us to extend our inquiries to the utmoſt boundary of the ſolar ſyſtem, and even carries us far beyond it.

The application of the ſame powers in nature alſo produced the *microſcope*, which gives us an inſight into the minute, but no leſs wonderful works of God in the creation around us; unfolding the admirable ſtructure of plants and animals, and diſplaying to us the exquiſite texture of their conſtituent parts. By means of theſe inſtruments, the bounds of human knowledge have been amazingly extended, and by the ſame helps new and exhauſtleſs ſources of information and pleaſure are continually opening to us; ſo that a perſon who is poſſeſſed of theſe inſtruments, and who has a taſte which every man ought to be aſhamed to be deſtitute of, can never want ſubjects of the moſt rational entertainment.

With reſpect to this great addition to our furniture, both for the purſuits of ſcience, and for the elegant enjoyment of life, human genius has but little to boaſt; the *invention*, if it may be ſo called, having been as caſual, and as unexpected, as it is, in its own nature, extraordinary. This hiſtory, therefore, furniſhes a ſtriking leſſon to all philoſophers, not to deſpiſe the moſt trifling obſervation; or to withdraw their attention and ſtudy from thoſe powers of nature, or even thoſe ſingle facts, which may ſeem, at firſt ſight, to be the moſt inſignificant, and the moſt remote from every poſſible uſe. Every new fact, or property of any of the conſtituent parts of nature, ſhould be carefully examined, as a treaſure of unknown value, the real worth of which time, and the diſcovery of other kindred powers in nature, may bring to light.

The very great importance of the teleſcope, has made the firſt diſcovery of it an intereſting ſubject of inquiry; and notwithſtanding it is agreed, on all hands, that the firſt conſtruction of this inſtrument was a caſual thing, and that the *rationale* of it was not known till many years after, we find ſeveral candidates for this ſmall portion of honour. Deſcartes conſiders James Metius, a perſon who was no mathematician, though his father and brother had applied to thoſe ſciences, as the firſt conſtructor of a teleſcope; and ſays that, as he was amuſing himſelf with making mirrors, and burning glaſſes, he caſually thought of looking through two of his lenſes at a time; and that happening to take one that was convex, and another that was concave; and happening alſo, to hit upon a pretty good adjuſtment of them, he found that, by looking through them, diſtant objects appeared very large and diſtinct. In fact, without knowing it, he had made a teleſcope.

Other perſons ſay, that this great diſcovery was firſt made by John Lipperſheim, a maker of ſpectacles at Middleburgh, or rather by his children; who, like Metius, were diverting themſelves with looking through two glaſſes at a time, and placing them at different diſtances from one another. But Borellus, the author of a book intitled *De vero teleſcopii inventore*, gives this honour to Zacharias Joannides, i. e. Janſen, another maker of ſpectacles at the ſame place, who made the firſt teleſcope in 1590; and it ſeems now to be the general opinion that this account of Borellus is the moſt probable.

Indeed, Borellus's account of the diſcovery of teleſcopes is ſo circumſtantial, and ſo well authenticated, that it does not ſeem poſſible to call it in queſtion. It is not true, he ſays, that this great diſcovery was made by a perſon who was no philoſopher; for Zacharias Janſen was a diligent enquirer into nature; and being engaged in theſe purſuits, he was trying what uſes could be made of lenſes for thoſe purpoſes, when he fortunately hit upon the conſtruction.

This ingenious mechanic, or rather philoſopher, had no ſooner found the arrangement of glaſſes that produced the effect he deſired, than he encloſed them in a tube, and ran with his inſtrument to Prince Maurice, who immediately conceiving that it might be of uſe to him in his wars, deſired the author to keep it a ſecret. But this, though attempted for ſome time, was found to be impoſſible; and ſeveral perſons in that city immediately applied themſelves to the making and ſelling of teleſcopes. One of the moſt diſtinguiſhed of theſe was Hans Laprey, called Lipperſheim by Sirturus. By him ſome perſon in Holland being very early ſupplied with a teleſcope, he paſſed with many for the inventor; but both Metius above-mentioned, and Cornelius Drebell, of Alcmar, in Holland, applied to the inventor himſelf in 1620; as alſo did Galileo, and many others.* The firſt teleſcope made by Janſen did not exceed 15 or 16 inches in length; but Sirturus, who ſays that he had ſeen it, and made uſe of it, thought it the beſt that he had ever examined. †

Janſen, having a philoſophical turn, preſently applied his inſtrument to ſuch purpoſes as he had in view when he hit upon the conſtruction. Directing it towards celeſtial objects, he diſtinctly viewed the ſpots on the face of the moon, and diſcovered many new ſtars, particularly ſeven pretty conſiderable ones in the great bear. His ſon Joannes Zachariæ, noted the lucid circle near the limb of the moon, from whence ſeveral bright rays ſeem to dart in different directions; and he ſays that, the full moon viewed through this inſtrument, did not appear flat, but was evidently ſpherical, the middle part being prominent. § Jupiter alſo, he ſays, appeared round, and rather ſpherical; and ſometimes he perceived two, ſometimes three, and at the moſt four ſmall ſtars, a little above or below him; and, as far as he could obſerve, they performed revolutions round him; but this, he ſays, he leaves to the conſideration of aſtronomers. || This, I make no doubt, was the firſt obſervation of the ſatellites of Jupiter, though the perſon who made it was not aware of the importance of his diſcovery.

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* De Vero teleſcopii inventore, p. 37. † Ib. p. 24, 30. § Ib. p. 39. || Ib. p. 40.

One Francis Fontana, an Italian, alſo claims the invention; but as he did not pretend to have made it before the year 1608; and as it is well known that the inſtruments were made and ſold in Holland, ſome time before, his pretenſions to a ſecond diſcovery are not much regarded.

There are ſome, who ſay, that Galileo was the inventor of teleſcopes; but he himſelf acknowledges, that he firſt heard of the inſtrument from a German;* but he ſays that, being informed of nothing more than the effects of it, firſt by common report, and a few days after by a French nobleman, J. Badovere, at Paris, he himſelf diſcovered the conſtruction, by conſidering the nature of refraction.† If this be true, he had much more real merit than the inventor himſelf. But Montucla queſtions the veracity of this great man in this caſe, eſpecially as he pretended that he did not know ſo much as the form of the glaſſes which the Dutch inſtrument makers made uſe of, and that he diſcovered *a priori*, that both a convex and a concave glaſs were neceſſary for the purpoſe, which is not true in fact. To me, however, it appears very probable, that this philoſopher might, before he purchaſed any teleſcope of Janſen, have received a very imperfect account of the inſtrument, perhaps from a perſon who had only looked through it, and who knew nothing of its conſtruction. But being merely certified of the poſſibility of effecting the purpoſe of a teleſcope, may well be ſuppoſed to have put a man of ſo much genius and curioſity as Galileo upon making trials, which might enable him to accompliſh the thing, even though he could not give a perfectly rational account of the powers of it; and might, in many reſpects, be very much miſtaken concerning it. I am not willing, without the cleareſt grounds, to queſtion the veracity of ſo reſpectable a perſon as Galileo.

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* Saverien's Hiſtoire, p. 247. † Nuncius Sidereus, p. 4.

The account of what Galileo actually did in this buſineſs, is ſo circumſtantially related by the author of his life, prefixed to the quarto edition of his works, printed at Venice, in 1744, and it contains ſo many particulars, which cannot but be pleaſing to every perſon who is intereſted in the hiſtory of teleſcopes, that I ſhall abridge a part of it, intermixing circumſtances collected from other accounts.

About April or May, in 1609, it was reported at Venice, where Galileo (who was profeſſor of mathematics in the univerſity of Padua) then happened to be, that a Dutchman had preſented to Count Maurice of Naſſau, a certain optical inſtrument, by means of which, diſtant objects appeared as if they were near; but no farther account of the diſcovery had reached that place, though this was near twenty years after the firſt diſcovery. Struck, however, with this account, Galileo inſtantly returned to Padua, conſidering what kind of an inſtrument this muſt be. The night following the conſtruction occurred to him; and the day after, putting the parts of the inſtrument together, as he had previouſly conceived of it, and notwithſtanding the imperfection of the glaſſes that he could then procure, the effect anſwered his expectations, as he preſently acquainted his friends at Venice; to which place he, ſix days afterwards, carried another, and a better inſtrument that he had made, and where, from ſeveral eminences, he ſhewed to ſome of the principal ſenators of that republic, a variety of diſtant objects, to their very great aſtoniſhment. When he had made farther improvements in the inſtrument, he, with his uſual generoſity, and frankneſs in communicating his diſcoveries, made a preſent of one of them to the Doge, Leonardo Donati, and, at the ſame time, to all the ſenate of Venice; giving along with the inſtrument, a written paper, in which he explained the ſtructure and wonderful uſes that might be made of it, both by land and at ſea. In return for ſo noble an entertainment, the republic, on the 25th of Auguſt, in the ſame year, more than trippled his ſalary as profeſſor.

Our philoſopher having amuſed himſelf for ſome time with the view of terreſtrial objects, at length directed his tube towards the heavens; and, obſerving the moon, he found that the ſurface of it was diverſified with hills and valleys, like the earth. He found that the *via lactea* and *nebulæ* conſiſted of a collection of fixed ſtars, which, on account either of their vaſt diſtance, or extreme ſmallneſs, were inviſible to the naked eye. He alſo diſcovered innumerable fixed ſtars diſperſed over the face of the heavens, which had been unknown to all the antients; and examining Jupiter, with a better inſtrument than any he had made before, he found that he was accompanied by four ſtars, which, in certain fixed periods, performed revolutions round him; and which, in honour of the houſe of Medici, he called *Medicean planets*.

This diſcovery he made in January 1610, new ſtyle; and continuing his obſervations the whole of February following, in the beginning of March next he publiſhed an account of all his diſcoveries, in his *Nuncius Sidereus*, printed at Venice, and dedicated to Coſimo, great Duke of Tuſcany, who, by a letter which he wrote to him on the 10th of July, 1610, invited him to quit Padua, and aſſigned him an ample ſtipend, as primate and extraordinary profeſſor at Piſa, but without any obligation to read lectures, or to reſide.

The extraordinary diſcoveries contained in the Nuncius Sidereus, which was immediately reprinted, both in Germany and France, was the cauſe of much ſpeculation and debate among the philoſophers and aſtronomers of that time; many of whom could not be brought to give any credit to Galileo's account, while others endeavoured to decry his diſcoveries, as being nothing more than fictions, or illuſions. Some could not be prevailed upon even to look through a teleſcope; ſo devoted were they to the ſyſtem of Ariſtotle, and ſo averſe to admit any other ſource of knowledge beſides his writings.* When it was found to be in vain to oppoſe the evidence of ſenſe, ſome did not ſcruple to aſſert, that the invention was taken from Ariſtotle; and producing a paſſage from his writings, in which he attempts to give a reaſon why ſtars are ſeen in the day time from the bottom of a deep well, ſaid that the well correſponded to the tube of the teleſcope, and that the vapours which aroſe from it, gave the hint of putting glaſſes into it, and laſtly, that, in both caſes, the ſight is ſtrengthened by the tranſmiſſion of the rays, through a thick and dark medium. Galileo himſelf tells this ſtory with a great deal of humour, comparing ſuch men to alchymiſts, who imagine that the art of making gold was known to the antients, but lay concealed under the fables of the poets.†

In the beginning of July, of the ſame year 1610, Galileo, being ſtill at Padua, and getting an imperfect view of Saturn's ring, imagined that that planet conſiſted of three parts; and therefore, in the account which he gave of this diſcovery to his friends, he calls it *planetam tergeminum*.

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* Vita del Galileo, p. 57, &c.

† Galileo's Works, Vol. 4, p. 91.

Whilſt he was ſtill at Padua, which muſt have been either in the ſame month of July, or the beginning of Auguſt following, he obſerved ſome ſpots on the face of the ſun; but, contrary to his uſual cuſtom, he did not chuſe, at that time, to publiſh his diſcovery; partly for fear of incurring more of the hatred of many obſtinate Peripatetics; and partly, in order to make more exact obſervations on this remarkable phenomenon, and to form ſome conjecture concerning the probable cauſe of it. He therefore contented himſelf with communicating his obſervations to ſome of his friends at Padua and Venice, among whom I find the name of father Paul. This delay, however, was the cauſe of this diſcovery being conteſted with him by the famous Scheiner, who likewiſe made the ſame obſervation in October 1611, and I ſuppoſe had anticipated Galileo in the publication of it.

About the end of Auguſt, Galileo left Padua and went to Florence, and in November following he was ſatisfied that, from the September preceeding, Venus had been continually encreaſing in bulk, and that ſhe changed her *phaſes* like the moon. About the end of March 1611, Galileo went to Rome, where he gratified the cardinals, and all the principal nobility, with a view of the new wonders he had diſcovered in the heavens, and among others the ſolar ſpots.

From theſe diſcoveries, Galileo obtained the name of *Lynceus*, who was famous in antiquity for the acuteneſs of his ſight; and moreover the Marquis of Monticelli inſtituted an academy, with the title of *De' Lincei,* and made him a member of it.* Twenty-nine years Galileo enjoyed the uſe of his teleſcope, continually enriching aſtronomy with his obſervations; but by too cloſe an application to that inſtrument, and the detriment he received from the nocturnal air, his eyes grew gradually weaker, till, in 1639, he became totally blind; a calamity which he bore with patience and reſignation, worthy of a great philoſopher. It neither broke his ſpirits, nor interrupted the courſe of his ſtudies.† That blindneſs, in the caſe of Galileo, who could reflect that he loſt his eyes in the cauſe of *ſcience,* or of Milton, who had the ſtill greater ſatisfaction to have loſt his in the cauſe of *public liberty,* might not be felt as a very great misfortune, will eaſily be conceived by all my readers.

The firſt teleſcope that Galileo conſtructed, magnified only three times; but preſently after he made another, which magnified eighteen times, and afterwards, with great trouble and expence, he conſtructed one that magnified thirty-three times, and with this, it was, that he diſcovered the ſatellites of Jupiter, and the ſpots of the ſun.§

Notwithſtanding Galileo muſt be allowed to have conſiderable merit with reſpect to teleſcopes, it was neither that of the perſon who firſt hit upon the conſtruction, nor that of him who thoroughly explained the *rationale* of the inſtrument. This important ſervice to ſcience was performed by JOHN KEPLER, whoſe name is famous on many accounts in the annals of philoſophy, and eſpecially by his diſcovery of the great law of motion, reſpecting the heavenly bodies; which is, that the ſquares of their periodical times, are as the cubes of their diſtances from the body about which they revolve; a propoſition which, however, was not demonſtrated before Sir Iſaac Newton. Kepler was aſtronomer to ſeveral of the emperors of Germany, he was the aſſociate of the celebrated aſtronomer Tycho Brahe, and the maſter of Deſcartes.

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* Vita del Galileo, p. 60. † General Biographical Dictionary, Vol. 5, p. 269.
§ Montucla, Vol. 2, p. 168.

Kepler made ſeveral diſcoveries relating to the nature of viſion, and not only explained the rationale of the teleſcope which he found in uſe, but alſo pointed out methods of conſtructing others, of ſuperior powers, and more commodious application. In order to give my reader a juſt idea of theſe great improvements in opticks, I muſt explain the doctrine of refraction through mediums of different forms, for which we are principally indebted to Kepler, though ſomething of this kind was done by Maurolycus. *

To explain the effects of convex and concave glaſſes to beginners: Let DA, fig. 17, be a ray of light falling upon a convex denſe medium, the center of which is E. Ee, then, being the perpendicular to the point of incidence; when the ray arrives at A, it will not go on in the ſtraight line A*t*; but, according to the law explained before, will be thrown nearer to that perpendicular, as into the line AT. In the ſame manner it may be ſhewn that another ray, incident in B, at an equal diſtance on the other ſide of the central point C, and parallel to the former ray DA, muſt be refracted into the ſame point T. It will alſo be found, that all the intermediate parallel rays will converge very nearly to the ſame point.

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* I wiſh it were in my power exactly to aſcertain the claims of theſe two philoſophers, as I ſometimes find the ſame diſcoveries aſcribed to them both; but, not having ſeen Maurolycus's own treatiſe, I am not able to do it. As B. Porta does not mention the diſcoveries of Maurolycus, and many reſpectable writers refer to Kepler only for the diſcoveries relating to the theory of viſion, and eſpecially the uſe of convex and concave glaſſes to different conformations of the eye; I rather ſuſpect that Montucla, whom, for want of any other guide, I have been obliged to follow, in my account of what was done by Maurolycus, has given rather too much to the Sicilian philoſopher, eſpecially as he himſelf, by an overſight, which it would be very imprudent in me to cenſure, has aſcribed ſome of the ſame diſcoveries to them both. There can hardly be a doubt but that Kepler muſt have been acquainted with the works of Maurolycus; and yet I do not find that he quotes him in his *Paralipomena ad Vitellionem*, where one would moſt naturally expect it.

On the other hand, if the rays fall parallel on the inſide of this denſer medium, as in fig. 18, they will converge to a point T in the air, or any rarer medium. For the ray DA, arriving at A, will not go on in a ſtraight line D*x*, but will be bent from the perpendicular E*f*, into AT. Alſo the ray incident in B, at the ſame diſtance from the central point C, will converge to the ſame place T, together with all the intermediate parallel rays.

Since, therefore, rays are made, to converge when they paſs either from a rarer or a denſer medium, terminated by a convex ſurface, and converge again when they paſs from the ſame medium convex towards the rarer, a lens which is convex on both ſides muſt, on both accounts, make parallel rays converge to a point beyond it. Thus in fig. 19, the parallel rays between A and B, falling upon the convex ſurface of the glaſs AB, would, in that denſe medium, have converged to T; but that medium being terminated by another convex ſurface, they will be made more converging, and be collected in a place nearer to the lens, as at F.

To explain the effects of a concave glaſs, let AB, fig. 20, be the concave ſide of a denſe medium, the center of concavity being at E. In this caſe, DA will be refracted towards the perpendicular EA; and ſo alſo will the ray incident at B; in conſequence of which they will diverge from one another within the denſe medium. The intermediate rays will alſo diverge more or leſs, as they recede from the central ray TC, which, being in the perpendicular, will go ſtraight on.

If the rays be parallel within the denſe medium, as in fig. 21, they will diverge when they paſs from thence into a rarer medium, through a concave ſurface. For the ray DA will be refracted from the perpendicular AE, as will alſo the ray that is incident at B, together with all the intermediate rays, in proportion to their diſtance from the central ray TC.

If, therefore, a denſe medium, as the glaſs AB, fig. 22, be terminated by two concave ſurfaces, parallel rays paſſing through it will be made to diverge by both the ſides of it. Thus the firſt ſurface AB will make them diverge as if they had come from the point T; and with the effect of the ſecond ſurface added to this, they will diverge as from a nearer point, F.

It was Kepler, who by theſe inveſtigations firſt gave a clear explication of the effects of lenſes, in making the rays of a pencil of light, converge or diverge. He ſhewed that a plano-convex lens makes rays that were parallel to its axis, to meet at the diſtance of the diameter of the ſphere of convexity; but that if both ſides of the lens be equally convex, the rays will have their focus at the diſtance of the radius of the circle, correſponding to that degree of convexity. But he did not inveſtigate any rule for the foci of lenſes unequally convex. He only ſays, in general, that they will fall ſomewhere in the medium, between the foci belonging to the two different degrees of convexity. It is to Cavallieri that we owe this inveſtigation. He laid down this rule. As the ſum of both the diameters is to one of them, ſo is the other to the diſtance of the focus. All theſe rules concerning convex lenſes are applicable to thoſe that are concave, with this difference, that the focus is on the contrary ſide of the glaſs. *

Upon this principle it was not difficult to find the foci of pencils of rays iſſuing from any point of the axis of the lens; ſince thoſe that are parallel will meet in the focus; and if they iſſue from the focus, they will be parallel on the other ſide. If they iſſue from a point between the focus and the glaſs, they will continue to diverge after paſſing the lens, but leſs than before; and thoſe that come from beyond the focus will converge after paſſing the glaſs, and will meet in a place beyond the oppoſite focus. This philoſopher particularly obſerved, that rays which iſſue from twice the diſtance of the focus will meet at the ſame diſtance on the other ſide. The moſt important of theſe obſervations have been already illuſtrated by proper figures, and from them the reſt may be pretty eaſily conceived. Later optical writers have aſſigned the diſtances at which rays iſſuing from any other place in the axis of a lens will meet; but Kepler was too much intent upon his aſtronomical and other purſuits, to give much attention to geometry. As it is not my deſign in this work to give the hiſtory of inveſtigations that are purely geometrical, in the ſcience of opticks, I ſhall content myſelf with ſubjoining, from Montucla, the following rule concerning this ſubject. As the exceſs of the diſtance of the object from the glaſs, above the diſtance of the focus, is to the diſtance of the focus; ſo is this diſtance, to the place of convergency beyond the glaſs. The ſame rule will find the point of divergency, when the rays iſſue

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* Montucla, Vol. 2. p. 176.

from any place between the lens and the focus. For, then, the exceſs of the diſtance of the object from the glaſs, above that of the focus, is negative, which is the ſame diſtance taken the contrary way.*

If my reader be maſter of the principles above mentioned, for which we are chiefly indebted to Kepler, he will not find it difficult to underſtand the *rationale* of teleſcopes and microſcopes, a general idea of which (ſufficient, however, for my purpoſe) I ſhall, in this ſection, endeavour to give him, applied to the principal conſtructions which are now in uſe.

The principal effects of teleſcopes depend upon theſe plain maxims, viz. that objects appear larger in proportion to the angles which they ſubtend at the eye; and the effect is the ſame whether the pencils of rays, by which objects are viſible to us, come directly from the objects themſelves, or from any place nearer to the eye, where they may have been united, ſo as to form an image of the object; becauſe they iſſue again from thoſe points, where there is no real ſubſtance, in certain directions, in the ſame manner as they did from the correſponding points in the objects themſelves.

In fact, therefore, all that is effected by a teleſcope is, firſt to make ſuch an image of a diſtant object, by means of a lens or mirror, and then to give the eye ſome aſſiſtance for viewing that image as near as poſſible; ſo that the angle which it ſhall ſubtend at the eye, may be very large, compared with the angle which the object itſelf would ſubtend in the ſame ſituation. This is done by means of an *eye glaſs*, which ſo refracts the pencils of rays, as that they may afterwards be brought to their ſeveral foci, by the natural humours of the eye. But if the eye was ſo formed as to be able to ſee the image, with ſufficient diſtinctneſs, at the ſame diſtance, without any eye glaſs, it would appear to him as much magnified as it does to another perſon who makes uſe of a glaſs for that purpoſe, though he would not, in all caſes, have ſo large a field of view.

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* Montucla, Vol. 2. p. 177.

If, inſtead of an eye glaſs, an object, or the image of an object, be looked at through a ſmall hole, in a thin plate or piece of paper, held cloſe to the eye, it may be viewed very near to the eye, and, at the ſame diſtance, the apparent magnitude of the object will be the ſame in both caſes. For if the hole be ſo ſmall as to admit but a ſingle ray from every diſtinct point of the object, theſe rays will fall upon the retina, in as many other diſtinct points, and make a diſtinct image. * They are only pencils, or cones of rays, which have a ſenſible baſe, as the breadth of the pupil, that are capable, by their ſpreading on the retina, of producing an indiſtinct image. As very few rays, however, can be admitted through a ſmall hole, there will ſeldom be light ſufficient to view any object to advantage in this manner. †

If no image be actually formed by the foci of the pencils without the eye, yet if, by the help of any eye glaſs, the pencils of rays ſhall enter the pupil, juſt as they would have done from any place without the eye, the *viſual angle* will be the ſame, as if an image had actually been formed in that place.

Upon theſe principles the effect of a Galilean teleſcope is eaſily explained. Let AB, fig. 23, be a diſtant object, from every point of which pencils of rays iſſue, and falling upon the convex glaſs, DE, tend to their foci at FSG. But a concave lens, IH (the focus of which is at FG) being interpoſed, the converging rays of each pencil are made parallel when they reach the pupil; ſo that, by the refractive power of the humours of the eye, they can eaſily be brought to a focus on the retina, at PQR. Alſo, the pencils themſelves diverging, as if they had come from X, MXO is the angle under which the image will appear, which is much larger than the angle under which the object itſelf would have appeared. Objects will not appear inverted through this teleſcope, becauſe the pencils which form the images of them, only croſs one another once, viz. at the object glaſs, as, in natural viſion, they do in the pupil of the eye.

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* Smith's Opticks, Vol. 1, p. 37.

† Dr. Franklin informs me, that his eye is ſuch, that he can, to great advantage, make uſe of many object glaſſes, without any eye glaſs; ſo that he has no occaſion for more than a ſingle lens to make himſelf a good teleſcope.

Such is the teleſcope that was firſt diſcovered, and uſed by philoſophers; and it is remarkable that it ſhould be of a much more difficult conſtruction than ſome other kinds that have been invented ſince. The great inconvenience attending it is, that the field of view is exceedingly ſmall. For ſince the pencils of rays enter the eye very much diverging from one another, but few of them can be intercepted by the pupil; and this inconvenience increaſes with the magnifying power of the teleſcope; ſo that philoſophers at this day cannot help wondering, that it was poſſible, with ſuch an inſtrument, for Galileo and others to have made the diſcoveries they did. It muſt have required incredible patience and addreſs. No other teleſcope, however, than this, was ſo much as thought of for many years after the diſcovery. Deſcartes, who wrote thirty years after, mentions no other as actually conſtructed, though Kepler had ſuggeſted ſome.

It is to this great man that we are indebted for the conſtruction of what we now call the *aſtronomical teleſcope*, being beſt adapted for the purpoſe of viewing the heavenly bodies. The *rationale* of this inſtrument is explained, and the advantages of it clearly pointed out by this philoſopher in his Catoptricks; but, what is very ſurpriſing, he never actually reduced his excellent theory into practice. Montucla conjectures, that the reaſon why he did not make trial of his new conſtruction was, his not bebeing aware of the great increaſe of the field of view; ſo that, being engaged in other purſuits, he might not think it of much conſequence to take any pains about the conſtruction of an inſtrument, which could do little more than anſwer the ſame purpoſe with thoſe of which he was already poſſeſſed. He muſt alſo have foreſeen, that the length of this teleſcope muſt have been greater in proportion to its magnifying power; ſo that, it might appear to him to be, upon the whole, not quite ſo good a conſtruction as the former.

It was not long, however, before Kepler's new ſcheme of a teleſcope was executed; and the firſt perſon who actually made an inſtrument of this conſtruction, was Father Scheiner, who has given a deſcription of it in his Roſa Urſina, publiſhed in 1630. If, ſays he, you inſert two ſimilar lenſes (that is, both convex) in a tube, and place your eye at a convenient diſtance, you will ſee all terreſtrial objects inverted, indeed, but magnified and very diſtinct, with a conſiderable extent of view. He afterwards ſubjoins an account of a teleſcope of a different conſtruction, with two convex eye glaſſes, which again reverſes the images, and makes them appear in their natural poſition. This diſpoſition of the lenſes had alſo been pointed out by Kepler, but had not been reduced to practice by him, any more than the former. This conſtruction, however, anſwered the end but very imperfectly; and Father Rheita, preſently after, hit upon a better conſtruction, uſing three eye glaſſes inſtead of two. *

As the firſt and laſt of theſe conſtructions are thoſe which are now in common uſe, I ſhall give a brief deſcription of them in this place. The rays of each pencil iſſuing from every point of the object ABC, fig. 24, paſſing through the object glaſs DEF, become converging, and meet in their foci at IHG, where an image of the object will be formed. If then another convex lens, KM, of a ſhorter focal length, be ſo placed, as that its focus ſhall be in IHG, the rays of each pencil, after paſſing through it, will become nearly parallel, ſo as to meet upon the retina, and form an enlarged image of the object at RST. If the progreſs of the rays be traced, it will preſently be perceived, that this image muſt be inverted. For the pencil that iſſues from A has its focus in G, and again in R, on the ſame ſide with A. But as there is always one inverſion in ſimple viſion, this want of inverſion produces juſt the reverſe of the natural appearance. The field of view in this teleſcope will be large, becauſe all the pencils that can be received on the ſurface of the lens KM, being converging after paſſing through it, are thrown into the pupil of the eye, placed at the common interſection of the pencils at P.

The proportion in which this teleſcope magnifies, is as the focal length of the object glaſs to that of the eye glaſs. For, neglecting the length of the teleſcope, as bearing no ſenſible proportion to the diſtance of an object, the angle under which the

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* Montucla, Vol. 2, p. 170.

object is viewed is AEC, which is equal to KEM; but the angle with which the pencils, that form the image come to the eye is KPM, or its equal ILG (the pencil MP being refracted parallel to GL, and KP to IL) and theſe angles are to one another as LH to HE.

The magnifying power of the Galilean teleſcope is alſo eſtimated by the ſame rule; the only difference between them being this, that the pencils by which the extremities of any object are ſeen in this caſe, enter the eye diverging, whereas, in the other, they enter it converging; but if the ſphere of concavity in the eye glaſs of the Galilean teleſcope be equal to the ſphere of convexity in the eye glaſs of another teleſcope, their magnifying power will be the ſame. The concave eye glaſs, however, being placed between the object glaſs and its focus, the Galilean teleſcope will be ſhorter than the other, by twice the focal length of the eye glaſs. Conſequently, if the length of the teleſcopes be the ſame, the Galilean will have the greater magnifying power.

Viſion is alſo more diſtinct in theſe teleſcopes, owing perhaps, in part, to there being no intermediate image between the eye and the object. Beſides, the eye glaſs being very thin in the center, the rays will be leſs liable to be diſtorted by irregularities in the ſubſtance of the glaſs. Whatever be the cauſe, we can ſometimes ſee Jupiter's ſatellites very clearly in a Galilean teleſcope not more than twenty inches, or two feet long; when one of four or five feet, of the common ſort, will hardly make them viſible.*

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* Martin's New Elements of Opticks, p. 18.

In the teleſcope with three eye glaſſes, called the *terreſtrial teleſcope,* becauſe it is chiefly uſed for terreſtrial objects, after the rays have paſſed the firſt eye glaſs, HI, fig. 25, as in the former conſtruction, inſtead of being there received by the eye, they paſs on to another equally convex lens, ſituated at twice its focal diſtance from the other, ſo that the rays of each pencil, being parallel, in all that interval, thoſe pencils croſs one another in the common focus, and the rays conſtituting them are tranſmitted parallel to the ſecond eye glaſs LM; after which the rays of each pencil converge to other foci at NO, where a ſecond image of the object is formed, but inverted with reſpect to the former image in EF. This image, then, being viewed by a third eye glaſs QR, is painted upon the retina at XZY, exactly as before, only in a contrary poſition; for the pencil that iſſues from A, after coming to a focus in F and N, forms the image of that point at laſt at Y, in the ſame poſition as it would have done without the intervention of any lens at all.

The ſame Father Rheita, to whom we are indebted for this uſeful conſtruction of a teleſcope for land objects, invented a *binocular teleſcope,* which Father Cherubin, of Orleans, endeavoured to bring into uſe afterwards. It conſiſts of two teleſcopes faſtened together, and made to point to the ſame object. When this inſtrument is well fixed, the object appears larger, and nearer to the eye when it is ſeen through both the teleſcopes, than through one of them only, though they have the very ſame magnifying power. But this is only an illuſion, occaſioned by the ſtronger impreſſion that two equal images, equally illuminated, make upon the eye. This advantage, however, is counterbalanced by the inconvenience attending the uſe of it.*

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* Montucla, Vol. 2. p. 173.

The invention of MICROSCOPES was not much later than that of teleſcopes, and, according to Borellus, whoſe account I do not find to have been called in queſtion by any perſon, we are indebted for them to the ſame author, at leaſt to Z. Janſen, in conjunction with his ſon; and for this latter favour we may, perhaps, be conſidered as under more obligation to them than for the former, the microſcope having more various and extenſive uſes, with reſpect to philoſophy, than the teleſcope. In our ideas, however, it appears ſomething *greater*, and more extraordinary, to be able to ſee objects too diſtant to be perceived by the naked eye, than thoſe that are too near to be ſeen by us; and therefore there is more of the *ſublime* in the teleſcope than the microſcope. Theſe two inſtruments, though different in their application, are, notwithſtanding, very ſimilar; as both of them aſſiſt us in the diſcovery of objects that we muſt otherwiſe have remained unacquainted with; by enlarging the angle which they ſubtend at the eye.

The Janſens, however, have not always enjoyed, undiſturbed, that ſhare of reputation to which they ſeem to be intitled, with reſpect either to the teleſcope or the microſcope. The diſcovery of the latter, in particular, has generally been conſidered as more uncertain than that of the former. All that many writers ſay, we can depend upon is, that microſcopes were firſt uſed in Germany, about the year 1621. Others ſay poſitively, that this inſtrument was the contrivance of Cornelius Drebell, * no philoſopher, but a man of curioſity and ingenuity, who alſo invented the thermometer.

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* Saverien's Hiſtoire, p. 257.

According to Borellus, Zacharias Janſen and his ſon preſented the firſt microſcopes they had conſtructed to prince Maurice, and Albert, Archduke of Auſtria. William Borell, who gives this account, in a letter to his brother Peter, ſays, that when he was ambaſſador in England, in 1619, Cornelius Drebell, with whom he was intimately acquainted, ſhewed him a microſcope, which he ſaid was the ſame that the Archduke had given him, and had been made by Janſen himſelf. This inſtrument was not ſo ſhort as they are generally made at preſent, but was ſix feet long, conſiſting of a tube of gilt copper, an inch in diameter, ſupported by three braſs pillars, in the ſhape of Dolphins, on a baſe of ebony, on which the ſmall objects were placed.*

This teleſcope of Drebell's, was evidently a compound one, or, rather ſomething betwixt a teleſcope and a microſcope, what we ſhould now, perhaps, chuſe to call a *megalaſcope*; ſo that it is poſſible that *ſingle* microſcopes might have been known, and in uſe, ſome time before: but perhaps no body thought of giving that name to ſingle lenſes, though, from the firſt uſe of lenſes, they could not but have been uſed for the purpoſe of magnifying ſmall objects. In this ſenſe we have ſeen, that even the antients were in poſſeſſion of microſcopes; and it appears from Jamblicus and Plutarch, quoted by Dr. Rogers, that they gave ſuch inſtruments as they uſed for this purpoſe the name of *dioptra*. As ſpectacles were certainly in uſe long before the invention of teleſcopes, one can hardly help concluding, that lenſes muſt have been made ſmaller, and more convex, for the purpoſe of magnifying of minute objects; eſpecially as the application of this kind of microſcope was nearly the ſame with that of a ſpectacle glaſs, both of them being held cloſe to the eye. At what time lenſes were made ſo ſmall as we now generally uſe them, for magnifying in ſingle microſcopes, I have not found. But as this muſt neceſſarily have been done gradually, the only proper object of inquiry is the invention of the *double*, or *compound microſcope*; and this is clearly given, by the evidence of Borellus above-mentioned, to Zacharias Janſen, the inventor of the teleſcope, or his ſon.

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* Borellus de vero teleſcopii inventore, p. 35.

The invention of compound microſcopes is claimed by the ſame Fontana, who claimed the diſcovery of teleſcopes; and though he did not publiſh any account of this improvement till the year 1646 (notwithſtanding he pretended to have made the diſcovery in 1618) Montucla, not having attended perhaps to the teſtimony of Borellus, is willing to allow his claim, as he thought there was no other perſon who ſeemed to have any better right to it.

The ſimple microſcope conſiſts of nothing more than a ſingle ſmall lens, AB, fig. 26, interpoſed between the eye and the object CD. When the pencils of rays have been tranſmitted through this lens, they meet upon the retina at GH. The proper uſe of this microſcope is that, the lens being placed in the focus of parallel rays, the rays of each pencil, though they iſſue from a place much nearer to the eye than the neareſt limit of diſtinct viſion, may nevertheleſs enter the eye parallel, and therefore be eaſily united in their foci upon the retina. But if it were poſſible for the object to be ſeen diſtinctly at this diſtance by the eye, unaſſiſted with any glaſs, the image upon the retina would be juſt as large; ſo that the magnifying power of the ſingle microſcope is in the proportion of the neareſt limit of diſtinct viſion, to the focal length of the lens. If, for inſtance, the glaſs be a ſmall round globule, whoſe focal diſtance is $\frac{1}{20}$ of an inch, and the neareſt limit of diſtinct viſion be eight inches, this globule will magnify as 8 to $\frac{1}{20}$ or as 160 to one.

The compound, or double microſcope, is an inſtrument ſimilar to the aſtronomical teleſcope, conſiſting of two convex lenſes, an object glaſs and an eye glaſs; by the former of which an image of the object is formed, which image the eye is enabled to view diſtinctly, at a near diſtance, by the help of the latter. But they differ in this, that in the teleſcope the rays of each pencil fall upon the object glaſs nearly parallel, and are therefore united in its focus; but in the microſcope they fall upon it very much diverging from one another, and therefore form the image in a place beyond the focus of the glaſs, and conſequently larger than the object itſelf. All this will be evident from fig. 27, in which when the pencils of rays, iſſuing from the points of the object KCL, have been tranſmitted through the object lens AB, their foci will be at MN; where, conſequently, there will be an image of the object, which is viewed by another lens, or eye glaſs EF, the focus of which is at MN. By this means the object appears inverted upon the retina, at OQP. But the field of diſtinct viſion is larger than in the ſingle microſcope; becauſe all the rays that can be received by the lens EF are thrown into the pupil of the eye.

Through this microſcope, an object appears magnified on two accounts; the image itſelf being larger than the object, and we being able to view it diſtinctly at a leſs diſtance. If, for inſtance, the image be twenty times larger than the object, and, by the help of the eye glaſs, we are able to view it five times nearer than we could have done with the naked eye, it will, on both theſe accounts together, be magnified 5 times 20, or 100 times.

Compound microſcopes are now generally made with two eye glaſſes, which give a much larger field of view, and likewiſe increaſe their magnifying power. In fig. 28, GK repreſents this additional eye glaſs, which may be of a conſiderable diameter, ſince the rays converge from thence to the next and proper eye glaſs. By this glaſs GK pencils of rays, coming from the object glaſs, are refracted ſo as to tend to a focus at O; but being intercepted by the proper eye glaſs DF, they are brought together at I, which is nearer to that lens than its proper focus at L; ſo that the angle DIF, under which the object now appears, is larger than DLF, under which it would have appeared without this additional glaſs; and conſequently the object is more magnified in the ſame proportion. * Dr. Hooke tells us that in moſt of his obſervations he made uſe of a double microſcope, with this broad middle glaſs, when he wanted to ſee much of an object at one view, and taking it out when he would examine the ſmall parts of an object more accurately; for the fewer refractions there are, the more bright and clear the object appears. †

I ſhall conclude this account of teleſcopes and microſcopes, with the following obſervations from Dr. Smith. To fit theſe inſtruments to ſhort-ſighted eyes, the object glaſs, and the eye glaſs muſt be placed a little nearer together; ſo that the rays of each pencil may not emerge parallel, but may fall diverging upon the eye; and then the apparent magnitude of the object will be altered a little, but ſcarce ſenſibly.

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* Martin's Philoſophia Britannica, Vol. 3, p. 46 † Smith's Opticks, Vol. 2, p. 402. Preface to Hook's Micrography.

The *brightneſs* of the appearance through a given teleſcope or microſcope is more or leſs, in proportion to the aperture of the object glaſs; for, ſuppoſing it to be covered with paper, except a ſmall place in the middle, the magnitudes of the pictures in the foci of the glaſſes, and alſo upon the retina, would not be altered; but, fewer rays of every pencil being admitted, the object would appear more obſcure. If the aperture and object glaſs remain the ſame, objects will appear more or leſs bright, according as the focal diſtance of the eye glaſs is longer or ſhorter; that is, according as the teleſcope or microſcope magnifies leſs or more. For the ſame quantity of light ſpread over a ſmaller picture muſt make it brighter, and ſpread over a larger picture muſt make it duller.

The following general obſervations concerning viſion, through glaſſes, he ſays, are worth remembering, viz. that the apparent *diſtinctneſs*, or confuſion, of an object, depends upon the mutual inclination of the rays in any one pencil to each other, when they fall from the eye; the apparent *magnitude* depends upon the inclination of the rays of different pencils to each other, the apparent *ſituation* depends upon the real ſituation of the extreme pencils; and laſtly, the apparent *brightneſs*, or obſcurity, depends upon the quantity of rays in every pencil. *

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* Smith's Opticks, Vol. 1. p. 42.

SECTION IV.

Miscellaneous discoveries of KEPLER and his cotemporaries.

BESIDES the doctrine of *lenſes,* and the *rationale of teleſcopes,* depending upon it, we are indebted to Kepler for his attention to ſeveral other branches of optical ſcience, particularly the buſineſs of *refraction,* and the nature of *viſion.* With reſpect to the latter, I have already hinted, that he has conſiderable merit. With reſpect to the former, his obſervations are not of ſo much value.

The inquiries that philoſophers were induced to make into the rationale of teleſcopes and microſcopes, led them to conſider the degree in which the rays of light are refracted, when they paſs out of air into glaſs; and Kepler, who was ſo happy in inveſtigating the progreſs of the rays of light through thoſe inſtruments, laboured as much, but not with equal ſucceſs, in the inveſtigation of the law of refraction. The neareſt the truth that he was able to come, was his obſerving, that when the incident ray does not make an angle of more than thirty degrees with the perpendicular, the refracted ray proceeds in an angle that is about two thirds of it. And as the object glaſſes of teleſcopes have ſeldom more than 20 degrees from the center to the extremity, he thought that the truth of his obſervation might be depended upon. Indeed, it does not appear that he was miſled by it. *

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* Montucla, Vol. 2. p. 175.

This attentive philoſopher obſerved, that, when light paſſes out of a denſer medium into a rarer, the greater is the angle of incidence, the farther doth the refracted ray recede from the perpendicular; till, at length, it is parallel to the refracting ſurface. In glaſs, he obſerves, that this angle is 42 degrees and ſome minutes; and if the inclination be made any greater, the light will not be refracted at all, but will be wholly reflected back into the glaſs, with an angle equal to that of incidence; in the ſame manner as when the reflexion is made from the ſurface of any denſer medium into a rarer. *

It was in the time of Kepler, i. e. about 1600, that the ſubject of the refraction of the atmoſphere was revived and cultivated by Bernard Walther, Mœſtlin, and others; but chiefly by Tycho Brahe, who, perceiving the importance of it to the perfection of aſtronomy, could not reſt till, after incredible diligence, in contriving inſtruments, and obſerving with them, he had ſettled the quantity of theſe refractions, at certain altitudes, to a tolerable degree of exactneſs. He thought that the light of the ſun is not ſenſibly refracted at 43 degrees of altitude, that of the moon at 45, and that of the fixed ſtars at 20. He ſuppoſed all the refractions to be leſs than they really are, except the horizontal, which he made too great; for he allows for it 34′ in caſe of the ſun, 33′ the moon, and 30′ the fixed ſtars; whereas De la Hire and Caſſini make it to be 32′ for all the heavenly bodies. † M. Caſſini was the firſt who diſcovered that the refraction of the atmoſphere extends as far as the zenith, as may be ſeen in the aſtronomical tables of M. De la Hire, in which the refraction correſponding to 89 degrees is one ſecond; and

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* Montucla, Vol. 2. p. 176. † Diderot's Encyclopedie under the article Refraction.

now aſtronomers have tables of refraction calculated for every degree of altitude. *

Neither T. Brahe, nor any of his cotemporaries, had a juſt idea where, or in what manner the rays of light are refracted; not even Kepler himſelf, who wrote ſo much upon this ſubject. T. Brahe ſuppoſed, with Alhazen and Vitellio, that the cauſe of refraction was a difference between the air and the ether beyond it, as well as the denſe vapours near the ſurface of the earth; which Rothman, mathematician to the Landgrave of Heſſe, with whom he had many diſputes, would not admit of, aſſerting that there is no difference between the air and ether, and that there is no refraction except near the horizon. The controverſy between theſe philoſophers on this ſubject is very long, much being written on both ſides, but it is not worth entering into at this day. Kepler, in the concluſion of his account of it, blames both the diſputants for not proceeding upon the true meaſures of refractions, by which, he ſays, it would have appeared, that the medium in which the light of the heavenly bodies is refracted is nothing that vaniſhes gradually into ether, extending as far as the moon, which Tycho ſuppoſed, but is the very air in which we breathe, having its upper ſurface as viſible to an eye placed above it as the ſurface of air and water. † He, therefore, ſuppoſed that the whole refraction was made at once, at the top of the atmoſphere; which he imagined to be uniformly denſe, and he determines its altitude, from the obſerved quantity of refraction, to be but little more than that of the higheſt mountains. But the true conſtitution of the denſity of the atmoſphere, deduced

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* Saverien's Dictionary, Vol. 2. p. 372.

† Paralipomena, p. 168.

afterwards from the Torricellian experiment, afforded a juſter idea of theſe refractions. *

It is probable that Kepler thought that the atmoſphere was of an invariable height, as well as of an uniform denſity; for Gilbert ſays that one Bruce, whom he calls a learned man, obſerved that the height of the denſe atmoſphere is variable, and that the length of the twilight varies with it. † But though Kepler was miſtaken with reſpect to the uniform denſity of the atmoſphere, and therefore cenſures T. Brahe unjuſtly on that account, he juſtly blames Rothman for aſſerting, in the courſe of this controverſy, that light, falling obliquely on a denſer medium, may ſuffer no refraction, and that refractions are made at the bottom of a medium, and not at the ſurface. §

Kepler was of opinion that the ruddy appearance of the moon in eclipſes is occaſioned by the light of the ſun refracted by the atmoſphere of the earth, being thereby bent out of its courſe, ſo as to fall upon the moon within the ſhadow of the earth. Whereas, before his time, it had generally been thought, that this red appearance was owing to the proper light of the moon herſelf; which being faint, could only become conſpicuous when the light of the ſun was withdrawn. This opinion, however, is mentioned and combated by Plutarch. ‡ Kepler even points out a method of meaſuring the quantity of the refraction of light in the atmoſphere of the earth by the appearance of the moon in eclipſes, ſuppoſing that only one part of the ſhadow of the earth will be illuminated by that refracted light.

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* Smith's Opticks. Remarks, p. 59. † Phyſiologia, p. 212. § Paralipomena, p. 79. ‡ Ib. p. 274.

The improvements in the doctrine of viſion, and other branches of optical ſcience, which were begun by Maurolycus, and B. Porta, were carried much farther by Kepler. His was the diſcovery of the images of external objects upon the retina, formed by the foci of the pencils of rays coming from every point of them. The inverſion of theſe images he alſo obſerved, and he clearly explained every thing relating to diſtinct and indiſtinct viſion, and ſhewed in what manner imperfect viſion is corrected by convex and concave glaſſes.* Theſe particulars we find in his *Aſtronomiæ Pars optica*; and as, upon the whole, it is probable that Kepler did not borrow any thing from Maurolycus, I ſhall, in this place, give a more particular illuſtration of the method of remedying the defects of viſion by lenſes of different forms, borrowing my figures from Dr. Smith. Let fig. 29, repreſent an eye, the cornea of which is too flat, as is commonly the caſe with old perſons; the effect of which is, that the rays which enter the pupil do not converge faſt enough, but tend to ſome point beyond the retina, as *q*. If a convex lens be interpoſed, as in fig. 30, it makes the rays converge faſter, ſo as to come to a focus upon the retina.

On the other hand, if the cornea of the eye be too protuberant, as in fig. 31, by which means the rays come to a focus before they reach the retina, as at *q* (in which caſe we ſay that a perſon is *ſhort-ſighted*) the interpoſition of a concave lens between the eye and the object, as in fig. 32, will make the rays diverge more at their entrance into the eye, which will throw the focus farther within it, ſo as to fall upon the retina.

The rectification of inverted images upon the retina, Kepler ſays, is the buſineſs of the mind, which, when it perceives an impreſſion on the lower part of the retina, conſiders it as made by rays proceeding from the higher parts of objects;* tracing the rays back to the pupil, where they croſs one another. But this hypotheſis will hardly be deemed ſatisfactory. It ſeems ſufficient to ſay, that *upper* and *lower* are only relative terms; and that, as all objects are painted upon the retina in a ſimilar manner (all the upper parts in one direction, and all the lower parts in another) it is by cuſtom only, founded on experience, and the aſſociation of ideas, that we learn to diſtinguiſh them one from another; ſo as to direct our eyes, or point our hands upwards or downwards, as we have occaſion. If this be the true ſolution, it will follow, that if the images of objects had always been painted in a different manner, that is erect, as the objects themſelves are, we ſhould have acted juſt as we do now, without being ſenſible of the difference, a different aſſociation of ideas only having taken place. In what manner the mind perceives the images upon the retina, Kepler does not pretend to know, and he blames Vitellio very much for attempting prematurely to determine a queſtion of this nature, and which, indeed, he ſays, does not belong to opticks. †

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* Montucla, Vol. 1. p. 159.

In order to account for our being able to ſee objects diſtinctly at different diſtances, which ſuppoſes a power of making rays of different degrees of divergency to meet in the ſame place, Kepler imagined that the contraction of the Proceſſus Ciliares draws the ſides of the eye towards the cryſtalline; by which means the eye is lengthened, and the retina puſhed to a greater diſtance from the pupil, when we are viewing objects very near to us. But though it was in vain, as we ſhall find, that M. De la

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* Smith's Opticks, Remarks, p. 4. † Paralipomena, p. 168.

Hire, in oppoſition to this hypotheſis of Kepler, endeavoured to prove, that there is no alteration in the form of the eye, in conſequence of any attempt to view objects at different diſtances, it will appear that this philoſopher was miſtaken in his hypotheſis concerning the contraction of this ligament, ſince it can only alter the poſition of the cryſtalline within the eye. Kepler confined the judgment of diſtances by a ſingle eye, to thoſe which bear a ſenſible proportion to the breadth of the pupil. *

T. Brahe, who was always intent upon aſtronomical obſervations, firſt obſerved an apparent diminution of the diameter of the moon in ſolar eclipſes, and imagined the cauſe of it to be a real diminution of the diſk, by the force of the ſun's rays; † but Kepler ſays, that the diſk of the moon doth not appear leſs, in conſequence of being unenlightened, but rather that it appears at other times larger than it really is, in conſequence of its being enlightened. For pencils of rays from ſuch diſtant objects, generally come to their foci, before they reach the retina, and conſequently diverge, and ſpread when they reach it; ſo that the image of the whole object conſiſting not of a number of well defined points, but of ſmall circular ſpaces, will be larger than it ought to be. For this reaſon, he ſays, different perſons may imagine the diſk to be of different magnitudes, according to the different goodneſs of their eyes; and he obſerves that, of twenty two obſervations that were made of the moon, in February 1591, it twice appeared to be 31′ in diameter, ſix times it was 32′, ſeven times 33′, ſix times 34′, and once 36′. § On the ſame account, Kepler ſays, that thoſe who ſee remote objects very indiſtinctly, inſtead of one phaſe of the moon, will ſee a ſeries of ten phaſes, diſpoſed in the

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* Robins's Tracts, Vol. 2. p. 277.

† Paralipomena, p. 284.

§ Ib. p. 218.

form of a creſt. The ſame perſons, he ſays, cannot diſtinguiſh their friends ſo well, at a little diſtance from them, when they have white neckcloths on, as when they are without them.* This philoſopher did not conſider, that, for the ſame reaſon that the moon, when it is enlightened, appears larger than it is, that part of the ſun which is ſeen next to the body of the moon, in ſolar eclipſes, muſt appear larger, likewiſe, and conſequently encroach upon the image of the moon, which is then quite dark. This we ſhall find fully explained in the laſt period of my hiſtory by Dr. Jurin.

Reinhold Mæſtlin, and Gemma, the cotemporaries of Kepler and T. Brahe, taught the method of obſerving eclipſes by their images caſt upon a white paper in a darkened room.†

It was Kepler who firſt diſcovered the true reaſon of the apparent places of objects ſeen by reflecting mirrors, as it depends upon the angle which the rays of light, iſſuing from the extreme part of an object, make with one another after ſuch reflexions. In plane mirrors theſe rays are reflected with the ſame degree of inclination to one another that they had before their incidence; but he ſhews that this inclination is changed in convex and concave mirrors, as has been particularly explained, p, 12.§ Kepler alſo demonſtrated the impoſſibility of burning by means of mirrors or lenſes at any great diſtance, for which I have before referred to Dechales.‖

The ſame age that produced Kepler was ſtill more diſtinguiſhed by Galileo, who threw new light upon almoſt every ſubject of philoſophical inquiry. What he did in the buſineſs of teleſcopes has been already related; and indeed it is not for much more than this, that the ſcience of opticks is indebted to him. It muſt not, however, be forgotten, that he firſt conceived the thought of meaſuring the velocity of light, and he gives a particular deſcription of his contrivance for this purpoſe, in his treatiſe on mechanics, p. 39. He had two men with lights, one of whom was to obſerve when the other uncovered his light, and to exhibit his own the moment that he perceived it. In this method the progreſs of ſound might be meaſured, and it was not at that time ſuſpected that there was ſo great a diſproportion between their velocities. In the preceding period of this hiſtory we have ſeen that Lord Bacon ſeems to have taken it for granted, that the progreſs of light is liable to be obſtructed by winds, though not ſo much as ſound is. Galileo began, as he informs us, with exerciſing two aſſiſtants very near to one another, that they might learn their maneuvres to perfection, expoſing or covering their lights at the preciſe moment required. At that time he had tried the experiment at the diſtance of no more than one mile; but he propoſed to try it at the diſtance of ſeveral miles, by means of teleſcopes in the dark. But we do not find that he ever proſecuted his enquiry, and though it was taken up by the members of the academy del Cimento, it was carried very little farther by them.

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* Paralipomena, p. 218. † Ib. p. 39. § Ibid p. 59. ‖ Borellus de vero teleſcopii inventore, p. 14.

The meaſuring of the velocity of light was certainly a noble problem; but no menſuration upon the ſurface of the earth was adequate to the ſolution of it; and at that time they had no idea of meaſuring the time of its paſſage from one planet to another, which happy method was firſt applied by Monſ. Roemer, as we ſhall ſee hereafter. In the mean time it is amuſing to conſider the firſt rude attempts towards the ſolution of ſuch great problems.

SECTION V.

A SHORT HISTORY OF PERSPECTIVE.

IT was in the 16th century that *Perſpective,* a new branch of opticks, was revived, or rather re-invented. This, is more a buſineſs of geometry than opticks, and is, indeed, more of an *art* than a *ſcience* ; but ſince it is derived from optical principles, and as the uſe of it is to give pleaſure to the eye, by a juſt repreſentation of natural objects, I ſhould do wrong not to give a ſhort account of its riſe and progreſs.

The art of Perſpective owes its birth to painting, and particularly to that branch of it which was employed in the decorations of the theatre, where landſcapes were principally introduced, and which would have looked unnatural and horrid, if the ſize of the objects had not been pretty nearly proportioned to their diſtance from the eye. We learn from Vitruvius, that Agatharchus, inſtructed by Æſchylus, was the firſt who wrote upon this ſubject; and that afterwards the principles of this art were more diſtinctly taught by Democritus and Anaxagoras, the diſciples of Agatharchus. Of the theory of this art, as deſcribed by them, we know nothing; ſince none of their writings have eſcaped the general wreck that was made of antient literature in the dark ages of Europe. However the revival of painting in Italy was accompanied with a revival of this art.

The firſt perſon who attempted to lay down the rules of Perſpective was Pietro del Borgo, an Italian. He ſuppoſed objects to be placed beyond a tranſparent tablet, and endeavoured to trace the images, which rays of light, emitted from them, would make upon it. But we do not know what ſucceſs he had in this attempt, becauſe the book, which he wrote upon this ſubject, is not now extant. It is, however, very much commended by the famous Egnazio Dante; and, upon the principles of Borgo, Albert Durer conſtructed a machine, by which he could trace the perſpective appearance of objects.

Balthazar Peruſſi ſtudied the writings of Borgo, and endeavoured to make them more intelligible. To him we owe the diſcovery of *points of diſtance,* to which all lines that make an angle of 45 degrees with the ground line are drawn. A little time after, Guido Ulbaldi, another Italian, found that all the lines that are parallel to one another, if they be inclined to the ground line, converge to ſome point in the horizontal line; and that, through this point, alſo, a line drawn from the eye, parallel to them, will paſs. Theſe principles put together enabled him to make out a pretty compleat theory of perſpective.*

Great improvements were made in the rules of perſpective by ſubſequent geometricians, particularly by profeſſor Graveſende, and ſtill more by Dr. Brook Taylor, whoſe principles are, in a great meaſure, new, and far more general than thoſe of any perſon before him.

To the opticians of this age we are indebted for ſeveral ingenious devices to apply the knowledge they had of opticks, and particularly of this branch of it, perſpective, to the purpoſes of amuſement. And though I profeſs only to relate diſcoveries concerning the nature of light, and the ſolution of the principal phenomena of nature, by means of them; the moſt rigid philoſopher, would not excuſe me, if I did not take ſome ſlight notice of the *reformation of diſtorted images* by convex, and other ſpeculums. By this means pictures that are ſo miſhapen, as to exhibit no regular appearance of any thing to the naked eye, ſhall, when viewed by reflexion, preſent a regular and beautiful image. The inventor of this ingenious deviſe, is not known. Simon Stevinus, who was the firſt that wrote upon it, does not inform us from whom he learned it. The principles of it are laid down by S. Vauzelard in his *Perſpective Conique et Cylindrique,** and Gaſpar Schott profeſſes to copy Marius Bettinus in his deſcription of this piece of artificial magick.

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* Saverien's Hiſtoire, p. 255.

It will be ſufficient for my purpoſe to copy one of the ſimpleſt figures of this writer, as, by this means, the myſtery of this art will be ſufficiently unfolded. Upon the cylinder of paper, or paſteboard, ABCD fig. 33, draw whatever is intended to be exhibited, as the letters IHS. Then, with a needle, make perforations along the whole out line; and, placing a candle, G, behind this cylinder, mark upon the ground plane the ſhadow of them, which will be diſtorted more or leſs, according to the poſition of the candle, or the plane, &c. This being done, let the picture be an exact copy of this diſtorted image, let a metallick ſpeculum be ſubſtituted in the place of the cylinder, and let the eye of the ſpectator have the ſame poſition before the cylinder, that the candle had behind it. Then looking upon the ſpeculum, he will ſee the diſtorted image reſtored to its proper ſhape. The reformation of the image, he ſays, will not eaſily be made exact, in this method, but it will be ſufficiently ſo to anſwer the purpoſe.*

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* Saverien's Hiſtoire, p. 256. Montucla, Vol. 1. p. 627.

Other methods, more exact and geometrical than this, were found out afterwards; ſo that theſe pictures could be drawn by certain rules, without the uſe of a candle. Schott quotes one of theſe methods from Bettinus, another from Herigonius, and another from Kircher, which may be ſeen in his Magia, Vol. 1. p. 162, &c. He alſo gives an account of the methods of reforming pictures by ſpeculums of conical, and other figures.

Inſtead of copying any of theſe methods from Schott or Bettinus, I ſhall preſent my reader with that which Dr. Smith hath given us in his Opticks, Vol. 1, p. 250, as, no doubt, the beſt, and from which any perſon may eaſily make a drawing of this kind. The ſame deſcription anſwers to two mirrors, one of which, fig. 34, is convex, and the other, fig. 35, is concave.

In order to paint upon a plane a deformed copy ABCDEKIHG F of an original picture, which ſhall appear regular, when ſeen from a given point O, elevated above the plane, by rays reflected from a poliſhed cylinder, placed upon the circle, *l n p*, equal to its given baſe; from the point R, which muſt be ſuppoſed to lie perpendicularly under O, the place of the eye, draw two lines R *a*, R *e*; which ſhall either touch the baſe of the cylinder, or elſe cut off two ſmall equal ſegments from the ſides of it, according as the copy is intended to be more or leſs deformed. Then taking the eye, raiſed above R, to the given height RO, ſomewhat greater than that of the cylinder, for a luminous point, deſcribe the ſhadow *a e k f* (of a ſquare *a e x z*, fig. 36, or parallelogram ſtanding upright upon its baſe *a e*, and containing the picture required) any where behind the arch *l n p*. Let the lines drawn from R to the extremities and diviſions of the baſe *a*, *b*, *c*, *d*, *e*, cut the remoteſt part of the ſhadow in the points *f*, *g*, *h*, *i*, *k*, and the arch of the baſe in *l*, *m*, *n*, *o*, *p*; from which points draw the lines *l*AF, *m* BG, *n* CH, *o* DI, *p* EK, as if they were rays of light that came from a focus R, and were reflected from the baſe *l n p*; ſo that each couple, as *l*A, *l*R, produced, may cut off equal ſegments from the circle. Laſtly, transfer the lines *l a f*, *m b g*, &c. and all their parts, in the ſame order, upon the reſpective lines *l*AF, *m* BG, &c. and having drawn regular curves, by eſtimation, through the points A,B,C, D,E, through F,G,H,I,K, and through every intermediate order of points; the figure ACEKHF, ſo divided, will be the deformed copy of the ſquare, drawn and divided upon the original picture, and will appear ſimilar to it, when ſeen in the poliſhed cylinder, placed upon the baſe *l n p*, by the eye in its given place O.

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* Schott's Magia, Vol. 1. p. 160.

The practical methods of drawing theſe images ſeem to have been carried to the greateſt perfection by J. Leopold, who, in the Acta Lipſienſia, for the year 1712, has deſcribed two machines, one for the images to be viewed with a cylindrical, and the other with a conical mirror. The perſon poſſeſſed of this inſtrument has nothing to do but to take any print he pleaſes, and while he goes over the out lines of it with one pen, another traces the anamorphoſis.

In the ſame age, alſo, people of ingenuity, wealth, and leiſure, cut groves of trees, ſo as to repreſent the appearance of men, horſes, and other objects from ſome one point of view, which were not at all diſcernible in any other. This might eaſily be effected by one perſon placing himſelf in any particular ſituation, and giving directions to other perſons what trees to lop, and in what manner. In the ſame method they contrived that buildings, of circular and other forms, and alſo whole groupes of buildings, conſiſting of walls at different diſtances, and with different poſitions to one another, ſhould be painted ſo as to exhibit the exact repreſentation of particular objects, which could only be perceived in one ſituation. Bettinus has illuſtrated this method by drawings in his *Apiaria.*

PERIOD III.

THE DISCOVERIES OF DESCARTES AND HIS CONTEMPORARIES.

THE name of DESCARTES was the firſt which ſucceeded that of Ariſtotle, as an authority in the ſchools of philoſophy. For though many perſons ſince the revival of literature had begun to think for themſelves, and did not chuſe to acquieſce in the *ipſe dixit* of the Stagirite; as Teleſius in Italy, Lord Bacon in England, and others in different places, no perſon had been ſo fortunate as to have acquired a degree of reputation, ſufficient to have his name ſet up in oppoſition to him. Indeed nothing but ſome new and general ſyſtem of philoſophy could have anſwered the purpoſe. But the Carteſian hypotheſis, after ſome oppoſition (which every thing that is new is ſure to meet with) became almoſt as popular as the Ariſtotelian had been; ſo that there was danger, leſt the world, inſtead of being emancipated from ſervitude, ſhould only have changed one maſter for another.

In fact, however, Deſcartes was more indebted for the reputation he ſo long enjoyed to the fertility and boldneſs of his imagination, which led him into many miſtakes, than to the accuracy of his judgment, and the real diſcoveries that he made. In no branch of philoſophy do we owe more to him than in opticks; and yet his *vortices* did infinitely more towards eſtabliſhing him in the rank which he ſo long held in the philoſophical world. The Carteſian hypotheſis was not effectually ſupplanted but by the Newtonian, which bids fair to ſtand as long as that ſyſtem, the great laws of which it ſo ſatisfactory unfolds.

Though Deſcartes is truly great in the ſcience of opticks, he does not ſtand ſingle in this period. Beſides SNELLIUS, who led the way to the diſcovery of the great law of refraction, which particularly diſtinguiſhes this period of my hiſtory, he had other illuſtrious cotemporaries; particularly the famous SCHEINER, a Jeſuit, and confeſſor to the Arch-duke of Auſtria, who died in the ſame year with Deſcartes, but was an older man than he. He it was, as we have ſeen, who carried into execution the ſchemes of Kepler, for conſtructing teleſcopes upon plans different from that of the original one, or Galilean; and who has, perhaps, equal merit with Galileo himſelf, with reſpect to the diſcovery of the ſpots of the ſun. The whole of his treatiſe intitled *Oculus*, is exceedingly valuable, and abounds with ingenious and important illuſtrations of the nature of viſion.

Cotemporary with Deſcartes was the great GASSENDI, who was alſo his rival as a philoſophical authority; and though his reputation gave way to that of his antagoniſt, there were many perſons in thoſe days who were called *Gaſſendians*, in oppoſition to the *Carteſians*. Gaſſendi wrote pretty largely on the ſubject of light, and the natural phenomena which depend upon it; but he adhered cloſely to the ſyſtem of Epicurus, in oppoſition to that of Ariſtotle, maintaining the materiality of light, and aſſerting that bodies are viſible by means of particles continually detaching themſelves from their ſurfaces. His arguments in ſupport of this ſtrange hypotheſis, and his anſwers to the objections that were made to it are very copious, and diſcover great acuteneſs; but it will hardly be expected that I ſhould give them a place in this hiſtory. This philoſopher made no experiments that produced any diſcovery in opticks, and, upon the whole, though he was a great man, he cannot be allowed to have much merit with reſpect to the ſubject of this part of my work. In fact, he is to be ranked rather among the ſchoolmen than philoſophers.

In oppoſition to the philoſophical ſyſtems both of Deſcartes and Epicurus, revived by Gaſſendi, the old Ariſtotelian doctrine of light did not want a defender in DU HAMEL, who was choſen the firſt ſecretary to the royal academy of ſciences at Paris. This writer, in his *Aſtronomia Phyſica*, publiſhed in 1681, examines the opinions of Epicurus, as defended by Gaſſendi, and alſo the Carteſian hypotheſis; and after a laboured refutation of them both, pleads ſtrongly in favour of the Ariſtotelian doctrine, in which light is conſidered as a *property* of bodies, and takes great pains to anſwer many objections that were made to it.

Cotemporary, alſo, with Deſcartes, was AGUILONIUS, a Jeſuit of Bruſſels, who publiſhed a large treatiſe on opticks in 1613. This is a very ſyſtematical and comprehenſive work, eſpecially with reſpect to thoſe branches of the ſcience in which mathematicks are concerned; but it does not contain much that is of a philoſophical nature, and nothing that is material of his own.

In this enumeration of the heroes of this period, I muſt not overlook the laborious and ingenious ATHANASIUS KIRCHER, who was one of the greateſt philoſophers, and ableſt mathematicians of the time in which he lived. He was of about the ſame age with Deſcartes, but outlived him thirty years.

His large and magnificient work, intitled *Ars magna lucis et umbræ,* muſt have been conſidered as a very capital performance, at the time in which it was written; and though this author neither diſcovered any new property of light, nor explained any capital phenomenon of nature better than it had been done before, it will be ſeen, in the hiſtory of this period, that the ſcience of opticks is under conſiderable obligations to him.

SECTION I.

OBSERVATIONS AND DISCOVERIES CONCERNING REFRACTION.

THE great problem concerning the meaſuring of refractions had been attempted both by Alhazen and Vitellio, and they made ſeveral experiments for this purpoſe; but, as muſt be acknowledged to have been very natural, they attempted to meaſure the *angle* itſelf, and not the *ſine,* or *ſecant* of it, and therefore they failed in their attempts. Even Kepler, who gave a great deal of attention to this ſubject, and who wrote in a later period, did not think of any other method; and notwithſtanding all the pains that he, and other philoſophers took, to determine the proportion between the angle of refraction and that of incidence, they came no nearer the truth than to obſerve, that, when the angle of incidence makes an angle of leſs than 30 degrees with the perpendicular, the angle of refraction into glaſs is about one third of it. This did not give ſatisfaction to the philoſophers of that age, in which geometry was very much cultivated, and conſequently exactneſs attempted in every thing. Scheiner alſo took a great deal of pains about the buſineſs of refractions, and accurately meaſured the proportion of the angles of refraction to thoſe of incidence, from air into water to every degree. The reſult of theſe obſervations he reduced into a table, which may be ſeen in Kircher's Ars Magna, p. 607.

With great care and pains Kircher drew, from actual experiments, a table of the angles of refraction, correſponding to any given angle of incidence, even to a minute of a degree, with reſpect to air and water; and he obſerved the degrees with reſpect to wine, oil, and glaſs. Theſe inveſtigations muſt have been of great uſe, before it was known that the ſines of the angles of incidence and refraction bear an invariable proportion to one another; whereby the one being given, in any caſe, the other may be known. See theſe tables, and the manner in which they were conſtructed, Ars Magna, p. 599. As this work was publiſhed not long after the Dioptrics of Deſcartes, there is little doubt but theſe experiments of Kircher were made ſome time before the diſcovery of the law of refraction, but not long enough to have been of any real uſe.

As it may amuſe ſome of my readers, I ſhall deſcribe the method which Kercher uſed to meaſure theſe refractions; eſpecially as it was, probably, the ſame, or nearly the ſame, with that which was uſed by Alhazen, Vitellio, Scheiner, and other old opticians for the ſame purpoſe. CDE, fig. 37, repreſents a hemiſpherical glaſs bowl, to the ſide of which was fixed a quadrant CO, exactly graduated, and a ſlide upon it, G; and from the top of the quadrant was let fall a perpendicular line, OE, moveable on the center B, ſo that while one end of it juſt touched the ſide of the veſſel ED, which was alſo graduated, the other moved along the quadrant. To try the refractive power of any fluid with this inſtrument, he firſt filled the veſſel exactly up to the center, B, then placing the rod OE in any angle required, a perſon looking along it obſerved (or was aſſiſted by a ſpectator in obſerving) both where it actually touched the oppoſite ſide of the veſſel, and alſo the place where it ſeemed to touch it by refraction; or, without looking through the ſights, or making uſe of the rod OE, they might obſerve the progreſs of a ray of the ſun's light, or that of a candle, in the fluid, when it was tranſmitted through the ſight upon the quadrant, and fell upon the middle of the veſſel B.*

Kircher's tables of refractions from air into water and glaſs, differ a little from thoſe of Vitellio, which he publiſhes along with his own, that his readers might compare them together. †

At length this capital diſcovery in opticks, viz. the true law of refraction was made, in part, by Willebrod Snellius, profeſſor of mathematicks at Leiden. This philoſopher did not, indeed, perfectly underſtand his own diſcovery, if it may be called his, and he did not live to publiſh any account of it himſelf; but his manuſcript papers were communicated to ſeveral perſons; and Voſſius informs us, that profeſſor Hortenſius explained this diſcovery of Snellius, both publickly and privately, before it appeared in the writings of Deſcartes; who publiſhed it under a different form, without making any acknowledgment of his obligations to Snellius, whoſe papers, Huygens aſſures us, that, to his knowledge, Deſcartes had ſeen.

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* Ars Magna, p. 608.

† Ib. p. 609.

Snellius was led to this diſcovery not by any reaſoning a priori, but by experiments. He obſerved, that ſuppoſing GC, fig. 38, to be a ray of light falling at C upon the ſurface of a denſer medium, and AB to be a perpendicular at the point of incidence; if the ray, after being refracted, was continued in CE, till it was intercepted by a plane HD, parallel to AB, there was always the ſame proportion between CE, the refracted ray, and CF the line which it would have deſcribed if it had ſuffered no refraction; and that if another ray, *g*C, be refracted at the ſame place into C*e*; CE will be to CF, as C*e* is to C*f*; that is, that the ſecants of the complements, or co-ſecants, of the angles of incidence and refraction are always in the ſame ratio. If the medium be air and water, the ratio is 4 to 3, if air and glaſs, it is 3 to 2 nearly. The miſtake of Snellius conſiſted in this, that he aſſerted this conſtant proportion not to belong to the true lengths of the lines CE and CF, but to their apparent lengths, with reſpect to which, alſo, he was under a miſtake.*

Deſcartes gives no account of any experiments that he made, in order to diſcover this law, but publiſhes it as the reſult of his inquiries into the nature of reflexion and refraction. The form in which he gives this law, is different from that of Snellius, and in general more commodious; but it might eaſily have been deduced from it. According to Deſcartes, the *ſine* of the angle of refraction always bears the ſame proportion to to the ſine of the angle of incidence.

I ſhall explain this diſcovery of Deſcartes very nearly in his own words, and by the help of his own diagrams. The proportion of the refraction of the ray AB, fig. 39, is to be meaſured, he ſays, by CB, or AH, the ſine of the angle of incidence, and by EB, or IG, the ſine of the angle of refraction, and not by the angles themſelves, ABH and GBI, and much leſs by the angle DBI, the difference between them. For the proportion of theſe angles changes with every degree of inclination; whereas that of the lines, AH and IG, is the ſame, at every degree of inclination in the ſame medium; ſo that, if two rays, KB and AB, fig. 40, be incident on the ſame point of a refracting medium, B, and the former be refracted to L, and the latter to I, KM will be to AH, as ~~M~~NL is to SI. This proportion being once found, for any one angle of incidence, it will be unneceſſary to try any more, though it will be ſafer to make the trial with ſeveral angles of incidence, in order to aſcertain the ſame thing with the greater certainty. It will be abſolutely neceſſary, however, to make theſe experiments with every different medium, becauſe the ratio of the ſine of the angle of incidence, to that of refraction is hardly the ſame in any two.[*]

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* Huygens's Dioptrics, p. 2

It does not appear that, before Deſcartes, any perſon attempted to explain the *cauſe* of refraction, which he undertook to do by the reſolution of forces, on the principles of mechanics; in conſequence of which he was obliged to ſuppoſe that light paſſes with more eaſe through a denſe medium than a rare one. Thus the ray AB, fig. 41, falling obliquely on a denſer medium at B, is ſuppoſed to be acted upon by two forces, one of them impelling it in the direction AC, and the other in AD, which only can be affected by the change of medium; and ſince, after the ray has entered the denſer medium, it approaches the perpendicular BH, it is plain that this force muſt have received an in-

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[*] Deſcartes's Dioptrics, p. 62.

increaſe, while the other continued the ſame, for if BE be taken equal to BD, or CA, the conſequence of the angle IBH being leſs than ABC, will be, that BH muſt be longer than BC, or AD.

The firſt perſon who queſtioned the truth of this explanation of the cauſe of refraction was M. Fermat, counſellor to the parliament of Thoulouſe, and an able mathematician. He aſſerted, contrary to Deſcartes, that light ſuffers more reſiſtance in water than in air, and more in glaſs than in water; and he maintained that the reſiſtance of different mediums, with reſpect to light, is in proportion to their denſities. M. Leibnitz adopted the ſame general idea; and theſe gentlemen argued upon the ſubject in the following manner.

Nature, they ſay, accompliſhes her ends by the ſhorteſt methods. Light, therefore, ought to paſs from one point to another, either by the ſhorteſt road, or that in which the leaſt time is required. But it is plain that the line in which light paſſes, when it falls obliquely upon a denſer medium, is not the moſt direct, or the ſhorteſt; ſo that it muſt be that in which the leaſt time is ſpent. And, whereas it is demonſtrable, that light, falling obliquely upon a denſer medium (in order to take up the leaſt time poſſible, in paſſing from a point in one medium, to a point in the other) muſt be refracted in ſuch a manner, that the ſines of the angles of incidence and refraction muſt be to one another, as the different facilities with which light is tranſmitted in thoſe mediums, it follows that, ſince light approaches the perpendicular when it paſſes obliquely from air into water (ſo that the ſine of the angle of refraction is leſs than that of the angle of incidence) the facility with which water ſuffers light to paſs through it is leſs than that of the air; ſo that the light meets with more reſiſtance in water than in air.

This method of arguing, from *final cauſes*, could not ſatisfy philoſophers. M. Fermat himſelf was not thoroughly pleaſed with it, and had recourſe to M. De la Chambre; but he was not capable of affording him any ſupport.

I ſhall take this opportunity of mentioning the hypotheſis of Dechales, to explain the law of refraction. He ſuppoſes that every ray of light, as BCAD, fig. 42, is compoſed of ſeveral ſmaller rays, which adhere to one another; and that they are refracted towards the perpendicular, in paſſing into a denſer medium, becauſe one part of the ray, as B, meets with more reſiſtance than another part, as A; ſo that B traverſes a ſmaller ſpace than A; in conſequence of which the ray muſt neceſſarily bend a little towards the perpendicular. The famous Dr. Barrow adopted this hypotheſis, if, indeed, he was not the author of it, but Dechales himſelf was not ſatisfied with it. It is evident that, on this hypotheſis, mediums of a greater refractive power muſt give greater reſiſtance to the paſſage of the rays of light than mediums of a leſs refractive power, which is contrary to fact. *

* Saverien's Dictionarie, Vol. 2, p. 368.

SECTION II.

Discoveries concerning the RAINBOW.

ONE of the moſt conſiderable of the real improvements that Deſcartes made in optical knowledge is his explication of the rainbow, which he took up after Antonio De Dominis, who gave no tolerable ſolution of the phenomenon of the external bow, and no reaſon at all for the preciſe angle which each of them ſubtends at the eye of the ſpectator ; concerning both of which circumſtances, Deſcartes has given us perfect ſatisfaction, though he failed in his attempt to account for the colours of this remarkable object.

According to this philoſopher, the exterior, or ſecondary bow is formed by two reflexions, and two refractions in a drop of water, as at B, fig. 43, in which S repreſents the ſun, and O the eye of the ſpectator, receiving a ray of light, after it has been thus twice reflected within the drop, and refracted once at its entrance near the lower part of it, and again at its exit, near the upper part ; whereas in the interior, or primary bow, the ray enters at the upper part of the drop, is only once reflected within it, and then again refracted at the lower part, from which it is tranſmitted to the eye ; as at A, in the ſame figure.

To account for drops in ſome particular ſituations, and in no others, tranſmitting light in this manner, ſo as to make the two bows always to appear under the ſame angle ; he obſerves, that

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it requires a conſiderable quantity, or denſity of light, to affect the eye in a ſenſible manner; and he found by calculation, that, of all the rays which proceed from the ſun, none are tranſmitted to the eye, after ſuffering the above mentioned reflexions and refractions, but thoſe which are diſtant from the central ray between 85 and 86 hundredth parts of the radius of the ſphere. No other than theſe, therefore, can ſenſibly affect the eye. But theſe form, with a line drawn from the ſun to a point in the heavens oppoſite to it, an angle of 41°. 30′, ſuppoſing the ſine of the angle of incidence be to that of the angle of refraction from air into water as 257 to 180. By a ſimilar proceſs he demonſtrates that, of all thoſe rays that are tranſmitted to the eye after two reflexions within the drop, none are ſufficiently denſe, but thoſe which iſſue at an angle of 51° 57′ from the point oppoſite to the ſun; and that no drops in the interval between A and B can tranſmit any rays that will ſenſibly affect the eye. *

Deſcartes's own explanation of the reaſon why the two rainbows are ſeen at theſe preciſe angles may be ſeen in his Dioptrics, p. 220. He there informs us, that he was at a loſs to know why the two rainbows appeared under theſe particular angles, till he took his pen, and calculated the effect of refraction on rays falling on every point of a drop of water, in order to find under what angles they could come to our eyes after two refractions, and one or two intermediate reflexions; and he was ſatisfied that, after one reflexion and two refractions, many more rays could be ſeen under an angle betwixt 41 and 42 degrees, than under any leſs angle, and none at all under a greater. Alſo, that, after two reflexions and two refractions, many more rays

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* Montucla, Vol. 2, p. 203.

came to the eye under an angle of 51 or 52 degrees, than under any greater, and that none of them could come to the eye under a leſs angle.

To make my younger readers underſtand what rays thoſe are that are ſo refracted and reflected in a drop of rain, as to exhibit the colours of the rainbow, let XY, fig. 44, repreſent a drop of rain, and let SA, SB, SC, SD, &c. be rays of light falling upon it from the ſun, which may, therefore, be conſidered as, in fact, parallel to one another. The ray SA, falling perpendicularly upon the drop, will paſs through it, without being diverted out of its courſe; but every other ray, falling more obliquely, will converge towards the ray SA, and will tend to meet it at a greater or leſs diſtance, according to its greater or leſs degree of obliquity. The conſequence of this muſt be, that there will be ſome point, P, on the oppoſite ſurface, which will be the fartheſt from the line AM that any ray, as SD, can fall. All the rays above this, as SE, &c. being ſo oblique to the ſurface, that, if they be produced, after refraction, they would tend to meet the perpendicular ray in a place nearer than that in which SD would have met it, and conſequently would ſtrike the oppoſite ſurface nearer to M than P. If rays be taken at equal diſtances from this extreme ray SD, on each ſide of it, provided theſe diſtances be but ſmall, as SE, and SC, they will fall, without ſenſible error, on the ſame point of the oppoſite ſurface, as at O; and, for the ſame reaſon, SF and SB will meet at N. It is, likewiſe, demonſtrable, that (ſince the angles of incidence and reflexion are equal) any two rays that meet upon the ſame point of the oppoſite ſurface, as at N, will iſſue parallel to one another, when they go out of the drop, as in the lines VR and TQ. But ſince all the rays that fall between theſe diſtant ones, SF and SB, will be reflected in very different angles, they will come alone to the eye, and therefore will not make a ſenſible impreſſion upon it; whereas thoſe that fall near the ray SD, will not only iſſue parallel to one another, but alſo very nearly parallel to all thoſe that fall between them. Conſequently, they are the rays that fall with the obliquity of the ray SD, on which the colours of the rainbow depend; and ſince the proportion between the angles of incidence and refraction from air into water is known, this angle may be calculated, and therefore the angle which the arch of the bow itſelf makes at the eye, which depends upon it. In a ſimilar manner, may the diameter of the exterior bow be calculated likewiſe.

Though Deſcartes could not give any ſatisfactory account of the colours of the rainbow, he conſidered the ſmall portion of the globule of water, at which the ray iſſues, as having the effect of a priſm; which was known to have the property of rendering the light that was tranſmitted through it coloured. He alſo obſerved, that the different ſituation of theſe ſmall priſms, with reſpect to the eye of the ſpectator, muſt be the reaſon why the colours appear in a contrary order in the two bows. If this philoſopher had been aſked in what manner the priſm produces colours, he could only have anſwered that it modifies the light, and that colour is a modification of light.

Deſcartes ſatisfied himſelf that he had hit upon the true ſolution of the ſecondary, as well as of the primary rainbow, by experiments with a glaſs globe filled with water, and expoſed to the rays of the ſun. If IG, fig. 45, repreſent the globe, and AB and FG be rays of the ſun falling upon it, while his eye was at E;

he obſerved that when he looked towards D, that part of the globe appeared red; and that was the only colour exhibited by it, while his eye was ſo ſituated, that the ray ED made the ſame angle DEM, viz. of 42°, with the line ZM paſſing through his eye and the ſun. If he placed his eye ſo that this angle became a little larger, the red diſappeared; and if he made the angle leſs, the red gave place to the lighter colours of the bow. Then looking towards the part K, ſo as to make the angle KEM about 52°, he obſerved that part of the globe to look red, but not ſo bright as D had done. Enlarging this angle, he obſerved the fainter colours; but if he made it leſs, all the colours diſappeared in that place. To make this order of colours, the rays of light had entered at the lower part of the globe G, had been reflected at H and I, and finally refracted at K; as, in the former caſe, the rays had entered the upper part of the globe B, had been reflected at C, and finally refracted at D.

SECTION III.

OBSERVATIONS CONCERNING VISION.

THE moſt important obſervations relating to viſion within this period, were made by Scheiner. To him we are indebted for compleating the diſcovery concerning viſion, as performed by means of the images of external objects upon the retina. For, in cutting away the coats of the back part of the eyes of ſheep and oxen, and preſenting ſeveral objects before them, within the uſual diſtance of viſion, he ſaw their images diſtinctly and beautifully painted upon the naked retina. He did the ſame with the human eye. This curious experiment he exhibited at Rome in the year 1625. *

Scheiner takes particular notice of the correſpondence between the eye and the Camera Obſcura, and explains a variety of methods to make the images of objects erect. † As to the images of objects being inverted in the eye, he acquieſces in the reaſon given for it by Kepler; viz. that the mind traces the progreſs of the rays to the pupil, and refers them to that part of the object, from which they, at that place, ſeemed to have proceeded. §

That the pupil of the eye is enlarged, in order to view remote objects, and that it is contracted while we are viewing thoſe that are near, is a fact with which Scheiner was well acquainted, and

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* Schott's Magia Univerſalis, p. 87. † Oculus, p. 176. § Ib. p. 192.

what he proved by experiment, and illuſtrated by figures. When a needle, or any ſmall object, is brought near to the eye of any perſon, who looks attentively at it, the pupil, he ſays, is plainly ſeen to contract; and it as conſtantly expands whenever it is withdrawn. *

He ſhews that the viſual rays proceeding from any object, and paſſing through a ſmall hole in a thin board or card, &c. before they reach the eye croſs one another; for if the edge of a knife be applied cloſe to the board, on the ſide next to the eye, and be moved along it, till it come to the hole, it will firſt conceal from the ſight, that part of the object which is ſituated on the oppoſite ſide of the hole. † It is not neceſſary, in this experiment, that the knife ſhould be cloſe to the board. It will ſucceed equally well wherever the knife is placed, between the board and the eye. The reaſon of this appearance is, that all the pencils paſſing through this ſmall hole muſt neceſſarily croſs one another in that place; and, being much ſmaller than the pupil of the eye, the pencils belonging to the object next to the left hand muſt fall upon the right ſide of the pupil, and *vice verſa*; ſo that a knife paſſing between the eye and the hole muſt neceſſarily intercept the rays that come from the oppoſite ſide firſt.

Making a number of perforations with a ſmall needle in a piece of paſteboard, not farther diſtant from one another than the diameter of the pupil of the eye, he ſays, that, if it be held cloſe to one eye, while the other is ſhut, as many images of a diſtant object will be ſeen as there are holes in the paſteboard, and even more diſtinctly than if nothing was interpoſed between them and the eye. § And yet he obſerves, that, at a certain diſtance, objects do not appear multiplied when they are viewed in this manner.* An explanation of this curious phenomenon will be found among the diſcoveries relating to viſion in the laſt period of this hiſtory.

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* Oculus, p. 31. † Ib. p. 32. § Ib. p. 37.

If an object be ſuſpended in a ſmall hole made in a thin board, and an eye, ſituated in the dark, look through the hole at a number of torches, or other luminous bodies, he obſerves that as many of the ſmall objects will be ſeen, as there are torches, &c.† In fact, it is the ſhadow of the object, made by each of the luminous bodies, that is caſt upon the eye.

Scheiner took a good deal of pains to aſcertain the denſity and refractive power of all the humours of the eye, by comparing their magnifying power with that of water or glaſs, in the ſame form and circumſtances; and concludes, that the aqueous humour doth not differ much from water in this reſpect, nor the cryſtalline from glaſs; and that the vitreous humour is a medium between them both. He alſo very accurately, and minutely traces the progreſs of the rays of light through all the humours of the eye; and, after diſcuſſing every poſſible hypotheſis concerning the proper ſeat of viſion, demonſtrates that it is in the *retina*, and ſhews that this was the opinion of Alhazen, Vitellio, Kepler, and all the moſt eminent philoſophers. He produces many reaſons of his own for this hypotheſis, anſwers a great number of objections to it, and, by a variety of arguments, refutes the opinion of former times, that the ſeat of viſion is in the cryſtalline. §

In the writings of Deſcartes we find a variety of obſervations on the phenomena of viſion, ſeveral of which are very judicious, and worth reciting. He well explains the natural methods of judging of the magnitudes, ſituations, and diſtances of objects by the direction of the optic axes, comparing it to a blindman's judging of the ſize and diſtance of an object, by feeling at it with two ſticks of a known length, when the hands in which he holds them are at a known diſtance from each other.

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* Oculus, p. 41. † Ib. p. 49. § Ib. p. 193.

He alſo obſerves that, having been accuſtomed to judge of the ſituation of objects by their images falling on a particular part of the eye; if, by any diſtortion of the eye, they fall on a different place, we are apt to miſtake their ſituation, or imagine one object to be two; as, till we become accuſtomed to it, we imagine one ſtick to be two, when it is placed between two contiguous fingers laid acroſs one another. But he obſerves that all the methods we have of judging of the diſtances of objects are very uncertain, and extend but to narrow limits. The direction of the optic axes, he ſays, will not ſerve us beyond 15 or 20 feet, and the change of the form of the cryſtalline not more than 3 or 4 feet. * For he imagined that the eye conforms itſelf to the view of near or diſtant objects by a change in the curvature of the cryſtalline, which he ſuppoſed to be a muſcle, the tendons of it being the proceſſus ciliares.† In another place he ſays that, the change in the conformation of the eye is of no uſe to us for the purpoſe of judging of diſtances beyond four or five feet, and the angle of the optic axes not more than 100 or 200 feet. For this reaſon, he ſays, the ſun and moon are conceived to be much more nearly of the ſame ſize than they are in reality. §

White and luminous objects, he ſays, appear larger than others, and alſo the parts contiguous to thoſe on which the rays

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* De homine, p. 64. † Dioptrics, p. 66. § Ib. p. 39.

actually impinge; and, for the ſame reaſon, if the objects be ſmall, and placed at a great diſtance, they will always appear round, the figure of the angles diſappearing.*

Deſcartes thought that thoſe coloured circles which ſometimes appear round the flame of a candle, and which had been imagined to be ſimilar to halo's are to be aſcribed to ſome affection of the eye. This concluſion he was led into by a caſual obſervation reſpecting himſelf. For when, after leaning his head for a conſiderable time upon his hand, during which time his right eye had been cloſed, and with his left he had been looking towards the heavens, a candle was brought to him, and he looked upon it with both eyes, he ſaw two circles round the flame, fig. 46, conſiſting of colours as vivid as any that he had ever ſeen in the rainbow. The larger, AB, was red without and blue within, and the leſs was red without and white within; and this colour extended quite to the flame. Upon cloſing his right eye, both theſe circles vaniſhed, but upon opening it again, and cloſing his left eye, they appeared as before. From theſe obſervations he concluded, that the cauſe of this appearance was the ſtate which his right eye had been brought into, by keeping it preſſed with his hand; but in what manner it had been affected, he did not pretend to determine.†

I cannot ſay that I have much to advance in favour of any of Gaſſendi's peculiar opinions concerning viſion. I ſhall, therefore, only mention two of them, becauſe I find them referred to by other writers, ſome of whom have taken a great deal of pains to confute them. He thought that the ſun and moon appeared larger in the horizon than in the zenith, becauſe, the light being weaker, the pupil of the eye is more dilated in looking at them; which is ſurpriſing in ſo great a man, conſidering how much better a reaſon had already been aſſigned for this appearance by Ptolemy, Alhazen, and other optical writers who ſucceeded them. To account for ſingle viſion with two eyes, he aſſerted that, in fact, we do not make uſe of more than one of them at a time; the one being relaxed, and inattentive to objects, while the other is upon the ſtretch.* Something like this opinion we ſhall find advanced in the laſt period of this hiſtory by M. Du Tour, who has produced a great number of experiments in favour of it.

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* Dioptrics, p. 93. † Ib. p. 232.

It appears from Kircher, that ſome of the moſt well known circumſtances relating to viſion, were very imperfectly underſtood in his time. One Joſeph Bonacurſius, diſcourſing with him concerning the properties of light, aſſerted, that he could make it appear, that a man might ſee an object as diſtinctly in the dark, as in the light. Paradoxical as this appeared, our philoſopher tried the experiment; and to his great ſurprize, as he ſays, found that the effects of it had not been aggravated. To make it, he directs that a ſmall hole be left in the ſhutter of a room, that can be made perfectly dark, and that this hole be covered with thin paper, on which marks may be made; and he ſays, that when this paper has been looked upon very ſteadily for ſome time, by a perſon in the room; if, after that, a white paper be held before his eyes, the marks of the former paper will be viſible upon it, and their colour be perpetually varying, even though the hole in the ſhutter be perfectly cloſed. This experiment, which is nothing more than the conſtant effect of firſt looking upon any object ſtrongly illuminated, and then ſhutting the eyes, is very ſolemnly recommended by Kircher to the conſideration of all philoſophers; and from it he concludes, that theſe images of things bear the ſame relation to the eye, that the light does to the Bolognian ſtone, which retains it ſome time after it has been expoſed to it.*

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* Gaſſendi's Works, Vol. 2, p. 395.

Of this experiment Kircher ſeems, afterwards, to have made ſome farther uſe, for in his *Phyſiologia*, p. 74, it is ſaid, that, if after looking at the ſun, a perſon look upon any dark object, the image in his eye will firſt be white, then yellow, then red, then green, and then blue.

SECTION IV.

Improvements in OPTICAL INSTRUMENTS.

AFTER what Kepler did in the buſineſs of teleſcopes, and the execution of his plans by Scheiner, which were mentioned under the preceding period, though a good deal of attention continued to be given to the ſubject, it being then new and ſtriking, no improvements of conſequence were made in them, till after the time of which I am now treating.

All that I have farther to relate concerning Scheiner, on the ſubject of oprical inſtruments, is his contrivance to exhibit the appearance of the diſk of the ſun with all its ſpots, in a camera obſcura, by means of a teleſcope. The camera obſcura itſelf, with the manner of exhibiting the appearance of external objects in it, we owe to B. Porta; but this gentleman had no teleſcope; and if he had had one, it is poſſible he might not have thought of making this ingenious uſe of it. It conſiſts of nothing but placing the teleſcope in the direction of the rays of the ſun; ſo that a perſon may either look through it, and take the image upon his eye, or, with a little alteration of the focus, receive it upon a white ſheet of paper; when a number of perſons, at the ſame time, may contemplate it at their leiſure, ſeeing the ſpots upon it, or the clouds paſſing over it, in a moſt beautiful manner.

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* Kircher's Ars Magna, p. 118.

Simple as this experiment is, it muſt have appeared very extraordinary to thoſe who firſt obſerved it, and ſaw the face of the ſun ſo diſtinctly, as in a mirror, without ſo much as turning their faces towards it, and yet without any offence to the eye. Kircher, in his deſcription of this appearance, repreſents the ſpectator as *ſummo ſtupore defixus, fixed in the utmoſt aſtoniſhment.* *

Deſcartes himſelf, who is the principal hero of this period, beſtowed great pains on the ſubject of teleſcopes; but he ſeems to have had no idea of any conſtruction beſides the Galilean; being either ſo much intent on his own ſchemes and productions, as not to have read thoſe works of Kepler, in which farther improvements were propoſed, or not having given much attention to them, or to the uſe that was made of them by Scheiner and others.

Finding that a ſpherical lens does not make the rays of light, that are parallel to its axis, converge to the ſame point, he took a great deal of pains to find a better figure for lenſes ; and he demonſtrated, geometrically, that ſome others, particularly the ellipſis, and hyperbola, would anſwer better. * He even engaged ſeveral artiſts to make lenſes of a hyperbolical figure, as the moſt perfect of all ; but it does not appear that any of them had much ſucceſs in their attempts ; and the great diſcovery of Sir Iſaac Newton, concerning the different refrangibility of the rays of light, has convinced us, that their ſucceſs would have been of little conſequence ; ſince the defect of viſion, ariſing from the ſpherical figure of the glaſs, is very ſmall, in compariſon of that which ariſes from this circumſtance. Beſides, as Montucla obſerves, though a lens in the form of an ellipſis, or an hyperbola, would make the rays that are parallel to the axis of the lens converge more nearly to a point, it would be of diſſervice with reſpect to thoſe which fall with ſome degree of obliquity upon it, ſince the curvature of a ſphere is uniform in all places, whereas every part of an ellipſis, or an hyperbola, has a different curvature. †

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* Kircher's Ars Magna, p. 2.

It was in, and a little before the age of Deſcartes, after much attention had been given to teleſcopes, and other optical inſtruments, that philoſophers began to ſpeculate concerning the poſſibility of what Archimedes is ſaid to have effected with burning mirrors, in deſtroying the Roman fleet ; and, obſerving that the focal diſtance of concave ſpeculums was much too ſmall for the purpoſe, the ſtory was generally diſcredited, and treated as a fable, particularly by Deſcartes. But Kircher, and his pupil Schott, thought the ſubject not unworthy of a more particular examination, eſpecially as Proclus is alſo ſaid to have deſtroyed a fleet

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* Dioptrics, p. 123, 136. † Montucla, Vol. 2, p. 201.

at Conſtantinople by the ſame means. Kircher, before he would give any opinion concerning the facts, did not content himſelf with reaſoning, but made a great number of experiments, in order to judge of the poſſibility of them. At firſt he was of opinion that parabolical ſpeculums, combined in the manner that has been repreſented before, would anſwer the purpoſe; but not ſucceeding with them, and having a very fruitful invention, he had recourſe to many other expedients, and at length tried the effect of a number of plane mirrors.

He erected a frame, on which he placed five of theſe mirrors, of the ſame ſize, and in ſuch a manner as to throw the rays that were reflected from them upon the ſame place, at the diſtance of more than a hundred feet; and theſe five mirrors only produced ſo great a degree of heat, that he had no doubt but that, by multiplying them, he could have ſet fire to inflammable ſubſtances at a greater diſtance.* A drawing of this machine of Kircher's may be ſeen in his *Ars Magna lucis et umbræ*, p. 772.

So intent was this indefatigable philoſopher upon this ſubject, that, after theſe trials, he made a voyage to Syracuſe, in company with Schott, on purpoſe to take a view on the place of the ſuppoſed tranſaction; and they both concluded, that the galleys of Marcellus could not have been more than thirty paces from Archimedes.†

M. Buffon, without knowing what had been done by Kircher, hit upon the ſame method of burning at a diſtance. In 1747 he conſtructed a machine which contained 400 mirrors, each half a foot ſquare, with which he could melt lead and tin at about a hundred and forty feet from it, and he could fire wood at a much greater diſtance.[*]

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* Phil. Tranſ. Vol. 48, p. 622. † Ib. p. 625.

The Marquis de Courtivron took the pains to make an exact calculation of the effect of ſuch a machine as that of M. Buffon, conſiſting of plane mirrors, compared with one that is truly ſpherical, and found that the quantity of light reflected by the latter was to the quantity reflected upon the ſame place by an equal ſurface of the former, as 314 to $247\frac{1}{2}$, that is, that the effect of the plane mirror is about $\frac{2}{9}$ leſs than that of the ſpherical.[†]

Dr. Wolfe (after giving an account of the ſtructure and effect of ſome parabolical burning mirrors, conſtructed by M. Hoeſen of Dreſden, and which are now in the poſſeſſion of M. Ehrard of the ſame place) ſo late as the year 1768, ſuppoſed that the ſpeculums of Archimedes might have been of the ſame kind; ſince a parabola, whoſe parameter is 2000 feet, may eaſily be deſcribed, and that, poſſibly, rays reflected from ſuch a ſpeculum, may be received by a lens after the focus, and tranſmitted parallel to any diſtance, but that the fuſibility of the glaſs muſt be conſidered.[§] The author of this account was not aware that Kepler and Dechales, as mentioned above, had demonſtrated the impoſſibility of this effect.

Conſidering how ſoon Kircher's tables of refractions were ſuperſeded by more perfect diſcoveries and obſervations, we are, perhaps, at this day, moſt obliged to him for his ingenious contrivance of the *Magic lantern*, and ſome other things in the optical way, that are calculated to afford us amuſement. The magic lantern, in particular, is capable of making ſo much diverſion to children, and perſons unacquainted with the principles of opticks, and even to philoſophers themſelves, in an hour of relaxation, that it certainly deſerves to be deſcribed in this place. By means of this curious inſtrument, ſmall coloured images painted upon glaſs are prodigiouſly magnified, and thrown upon an oppoſite wall, in all their natural and vivid colours, and of whatever ſize a perſon ſhall chuſe.

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[*] Montucla, Vol. 1, p. 246.

[†] Ac. Par. 1747. M. p. 659.

[§] Phil. Tranſ. Vol. 59, p. 4.

It conſiſts of two lenſes, CD and EF, fig. 47, incloſed in a dark lantern, together with a candle A, and the image, GH, is placed between the lenſes. The uſe of the lens CD, is to throw a ſtrong light upon the image, from which rays of the ſeveral pencils iſſuing, and paſſing through the lens EF, are made to converge to their foci IK, at the oppoſite wall. By having the inſtrument ſo contrived, as that the lens EF may move in a ſlide, the focus may be made, and conſequently the image appear diſtinct, at almoſt any diſtance. Sometimes a concave mirror, MN, is placed behind the candle, in order to throw ſtill more light upon the object, and to make the image upon the wall ſtill more vivid.

A view of the inſtrument itſelf may be ſeen fig. 48, in which *a* is the lens that throws the light upon the object. Thoſe who chuſe to ſee Kircher's own drawings of this ingenious inſtrument, will find ſome very fine ones in his *Ars Magna lucis et umbræ*, p. 768, 769. He will there ſee that the inventor of this inſtrument had ſliders filled with images, ſuch as we make uſe of at this day; but he will alſo perceive that Kircher, with all his ingenuity, of which it is impoſſible not to conceive a very great opinion, had not the art of making his lamp burn without ſmoke; though it is poſſible that the deſigner he employed might, in order to ſhew his ſkill in drawing, make a greater cloud of ſmoke both from the lamp, and the chimney belonging to this inſtrument than, in fact, ever iſſued from them.

Among other amuſing things that were either invented or improved by Kircher, was the method of throwing the appearance of letters, and other forms of things, into a darkened room from without, by means of a lens and a plane mirror. The figure 49 from Schott's *Magia Univerſalis*, p. 428, will make the whole method very plain. The figures or letters were written upon the face of the mirror, and inverted, and the focus of the lens was contrived to fall upon the ſcreen or wall that received their images. In this manner he ſays, that, with the light of the ſun, he could throw a plain and diſtinct image 500 feet.*

Kircher being alſo fond of the art of dialling, in which he made many ingenious improvements, contrived a great variety of dials, in which the hour was indicated by the ſun beams, thrown by reflexion into places where the ſun could not ſhine. He alſo executed a variety of other things, of a ſimilar nature, which greatly contributed to the entertainment of himſelf and his friends. Deſcriptions and drawings of them all may be ſeen in his *Ars Magna lucis et umbræ*.

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* Schott's Magia Univerſalis, p. 434.

SECTION V.

MISCELLANEOUS OBSERVATIONS AND EXPERIMENTS.

AMONG the miſcellaneous articles that fall within this period, the Carteſian theory of light requires to be mentioned, if on no other account, at leaſt for its novelty and ſingularity, being different from all the theories that had preceded it. According to Deſcartes, light is neither a *ſubſtance*, as Epicurus and ſome others had ſuppoſed, nor yet a mere *property* of bodies, as Ariſtotle maintained; but the *motion of a ſubtle fluid*, communicated by the preſſure of a luminous body. As this philoſopher ſuppoſed that the univerſe was an abſolute *plenum*, without any vacuities, it was a neceſſary conſequence of this hypotheſis, that light muſt be tranſmitted inſtantaneouſly; and to illuſtrate it, he compares the tranſmiſſion of light to motion given to one end of a rod, which is inſtantly communicated to the other, whatever be the length of it. *

Light being nothing more than motion communicated to a fluid, it did not neceſſarily follow, that the eye had not a power of exciting that motion, as well as a luminous body; and therefore, when Deſcartes ſays that, in general, viſion is performed by intromiſſion, or by light proceeding from viſible objects, he ſuppoſed that there were exceptions to this rule; and he took it for granted, that the eyes of cats had the power of emitting light. He alſo thought that ſome men might ſee in the ſame manner.*

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* Dioptrics, p. 50.

Notwithſtanding Deſcartes ſpeaks of the inſtantaneous tranſmiſſion of light through all diſtances, yet, his reaſoning about the cauſe of refraction above-mentioned, goes upon the ſuppoſition of a difference in its velocity, depending upon the denſity of the medium through which it is tranſmitted, and its being greater in a denſe medium than in a rare one. The reaſon he gives for this is ſomething remarkable. A ray of light, he ſays, meets with more reſiſtance in a rare medium, the parts of which are moveable, and do not adhere ſufficiently to one another, than in a denſe one, in which it is leſs apt to be turned out of its way on that account. This hypotheſis retained its credit many years, notwithſtanding the inſuperable objection to it, from the analogy which light would then bear to ſound, which diffuſes itſelf in all directions, and is not, like light, ſubject to be intercepted by the interpoſition of any body whatever. The followers of Deſcartes amended his hypotheſis in ſeveral reſpects, but without relieving it from this great objection.

Inſtead of the perfectly ſolid globules, in the motion of which Deſcartes thought that light conſiſted, Malebranche ſubſtituted fluid vortices, and ſuppoſed that every impreſſion communicated to any one of them is immediately tranſmitted to thoſe that are contiguous to it, ſo that ſtill the propagation of light is ſimilar to that of ſound.† The later Carteſians in general, ſuppoſed that the fluid by which light is tranſmitted is elaſtic, and M. Huygens made a ſtill farther modification of the Carteſian hypotheſis, imagining the waves of light to ſpread not circularly, but in ellipſes; and upon this ſuppoſition he endeavours to explain the phenomena of Iſland cryſtal.*

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* Dioptrics, p. 57.

† Ac. Par. 1699, H. p. 29, M. p. 41.

It is no wonder that, with ſuch an hypotheſis of *light*, Deſcartes ſhould have been embarraſſed in his ideas concerning *colours*; but it was not his diſpoſition, much more than that of the antient philoſophers, to confeſs his ignorance. Rather therefore than ſay nothing on the ſubject, he advanced that light is affected by two kinds of motion, direct and circular. When the circular motion is quicker than the direct, the colour, he ſays, is red; if the direct be quicker, it is blue; but if they be equal, it is yellow; and out of theſe three colours it was long the opinion of many philoſophers, that all the reſt were formed, by different proportions in their mixture.

Though Deſcartes was much miſtaken with reſpect to the nature of light and colours, he juſtly diſtinguiſhes between *black* and *white*; obſerving that the former ſuffocates, or extinguiſhes the rays that fall upon it; whereas the latter reflects them.† This is the firſt diſtinct account I have met with of this ſenſible hypotheſis. Kepler, who conſidered the ſame ſubject, ſaid, that the reaſon why black objects grow hot ſooner than white ones, is not properly owing to any difference in the colour, but becauſe thoſe ſubſtances which are black are of a more dry and inflammable nature, *ariditatem et aduſtionem ſapiunt*.§

Deſcartes was not equally clear in all his ideas concerning colours. Conceiving of light as the motion of a fluid medium, in which reſpect it is analogous to ſound, and obſerving how agreeable

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* Huygens's Works, Vol. 3, p. 46. † Dioptrics, p. 50. § Paralipomena, p. 220.

able *green* is to the eye; among other obſervations, he compares the effect of it to that of an octave in muſic.*

The opinions of Gaſſendi concerning the nature of light, and the reaſon which he gave why the ſun and moon appear larger in the horizon than in the meridian have been already mentioned. He maintained, however, and even pretended to prove by experiment, that, notwithſtanding the ſun appears larger when he is in the horizon, the ſhadows of objects are larger at that time than they are when the ſun is higher. As he took a good deal of pains about this experiment, I ſhall copy his deſcription of it, and the drawing that he made to explain it. IH, fig. 50, repreſents a ſtrong board, four Paris toiſes in length; and NI and RH are two ſmall boards, about half a foot high, erected at right angles to it. This machine being placed ſo that the rays of the ſun were exactly parallel to it, when he was 3 degrees high, the ſhadow of the former board, received upon the latter, meaſured 3 inches, and 3. 6 lines; at 5 degrees of altitude, the ſhadow of the ſame board meaſured 3 inches, 3 lines; at 8 degrees it was 3 inches, 2. 8 lines, and at 15 degrees it meaſured 3 inches 2. 4 lines. The increaſe of the ſhadow in the afternoon exactly anſwered to the decreaſe in the forenoon, as he found by frequent menſurations; and when the ſun was 3 degrees above the weſtern horizon, the ſhadow meaſured 3 inches 3. 6 lines, as it did when the ſun had the ſame altitude above the eaſtern horizon. He made the ſame obſervations by moon light, and repeated them very often, without any variation in the reſult; and he aſſures his friends, that it was impoſſible to make the menſurations with more care or caution. He accounts for this great

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* De Homine, p. 57.

magnitude

of ſhadows, when the ſun is near the horizon, by ſuppoſing that his diſk is contracted by the groſs vapours through which he is then ſeen, ſo that the upper part of the ſhadow is made by beams which iſſue not from the higheſt part of the diſk, but from a part which is not ſo much elevated. *

If the diſtances of the ſun and moon had borne a leſs proportion to the ſemidiameter of the earth, a more important uſe might have been made of this obſervation than Gaſſendi was aware of. For, ſince thoſe who live under the equator are nearer to thoſe heavenly bodies, by one ſemidiameter of the earth, when they are in their zenith, than when they are in the horizon, their diſtances from the earth might have been determined in this manner. But it cannot be expected that a thing ſo ill defined as a ſhadow ſhould be meaſured with ſufficient exactneſs for this purpoſe, even with reſpect to the moon, which is much nearer to us than the ſun. And ſince Gaſſendi found no difference in theſe two caſes, it is moſt likely, notwithſtanding the good opinion he had of his attention and accuracy, that there was ſome miſtake in his meaſures.

Kircher, who has contributed ſomething to almoſt every other ſection of this period, muſt not paſs without being mentioned in this. When he was at Rome, in 1639, he had an opportunity of examining what was, at that time, currently reported concerning the camileon, viz. that it could aſſume the colour of any object near to which it was placed, except thoſe that were white, or red; but this he found to be a miſtake, there being no foundation for any exception in favour of thoſe two colours; for, when the animal was placed upon a white handkerchief, it could hard-

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* Gaſſendi Opera, Vol. 3, p. 423.

ly be diſtinguiſhed from it; in the ſame manner as when it was placed upon a green cloth, it was as green as the cloth itſelf. This property he aſcribes to the power of imagination in the animal, becauſe it loſes it when it is dead.* In fact, the ſkin of the camileon ſeems to be in ſome reſpects ſimilar to a mirror, which reflects, without diſtinction, all the coloured light that falls upon it.

Kircher was the firſt who obſerved the remarkable properties of the infuſion of *lignum nephriticum*, the optical phenomena of which have been the ſubject of various diſcuſſion, from his time to that of Sir Iſaac Newton, who firſt explained them ſatisfactorily. But as the obſervations of Kircher were very imperfect, I ſhall reſerve the mention of them for an introduction to the more accurate account of the ſame appearances by Mr. Boyle.

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* Ars Magna, p. 63.

PERIOD IV.

DISCOVERIES IN OPTICKS FROM THE TIME OF DESCARTES TO THAT OF SIR ISAAC NEWTON.

NOTWITHSTANDING the attempt of Deſcartes to philoſophize after the manner of the antients, in deducing all the phenomena of nature from preconceived principles and cauſes, and the great number of his followers, which threatened to put a ſtop to the inveſtigation of natural powers by means of *experiment*, which had been ſo ſtrongly recommended by Lord Bacon, and ſo happily purſued by Galileo and others, the manifeſt ſuperiority of this method ſoon recommended it to all the lovers of true ſcience; and from this æra there have been no advocates for the Carteſian philoſophy, or any other method ſimilar to it, among philoſophers.

It is no wonder, however, that the mode of philoſophizing, which was in uſe before recourſe was had to experiment, was given up with ſo much reluctance. It was a compendious road to univerſal knowledge, and peculiarly flattering to the ambition of thoſe who wiſhed to appear as the heads of ſects, and the founders of ſyſtems. We ſhould alſo conſider, that the amazing variety which we now find in natural powers and principles, though ariſing, we have reaſon to believe, from the greateſt uniformity and ſimplicity in their primary cauſes, was a thing of which the ancients had no idea. Some *one* great key, that was able to unlock all the myſteries of nature, always appeared to them to be within their reach; and though ſo many of them had failed, it ſuggeſted no ſuſpicion that *one* key would not anſwer the purpoſe, but only led them to try others. At length, however, the more humble, patient, and laborious method of inveſtigation was univerſally purſued, by all perſons who made any pretenſions to philoſophy; and we now entertain no hope of coming at the great, general, and ſimple laws of nature, but by tracing them through an almoſt endleſs, but moſt pleaſing labyrinth, of ſubordinate and particular laws.

So long, however, was it before theſe ideas were generally adopted, that we may conſider the true æra of experimental philoſophy, as having had its commencement with the period on which we are now entering; for, in fact, very little was done in this buſineſs till after the time of Deſcartes. Before this period, we hardly meet with any production among the learned, but ſuch as might have been executed without ſtirring from their cloſets, except in what reſpected mechanicks; ſo that the proper diſtinction between the natural philoſopher and the mathematician was but little attended to. Even in chymiſtry we find little that is worth knowing before the ſame period; for, notwithſtanding the multiplicity of chymical writers, and the great parade of their proceſſes, it appears that they had ſeldom any other object beſides the production of certain ſubſtances, eſpecially thoſe that were uſed in pharmacy, and not the inveſtigation of thoſe powers in nature by which they are produced. But preſently after this remarkable period, which has continued little more than a ſingle century, experimental philoſophy began to be aſſiduouſly cultivated in all the countries of Europe; and, moreover, individuals, finding themſelves unequal to many attempts to which experimental inquiries led them, formed themſelves into ſocieties for the greater advancement of natural knowledge.

The firſt of theſe was eſtabliſhed in Italy (to which country we are indebted for the revival and cultivation of all the branches of polite and uſeful learning;) being founded by Leopold, Grand Duke of Tuſcany, in 1657. The members of this celebrated ſociety, called the academy *Del Cimento*, were very aſſiduous in making experiments in various branches of natural philoſophy, and were enabled to do them to the greateſt advantage, by the generous aſſiſtance of their patron. They were not entirely unattentive to the phenomena of *light*, though it will be ſeen that their experiments on this ſubject are barely worth reciting. This reſpectable ſociety was ſhort-lived, the firſt and laſt publication of their experiments being in 1667.

The next philoſophical ſociety was begun in this country, having taken its firſt riſe from the aſſemblies of a few perſons at Oxford, after the concluſion of the civil wars, and having been finally incorporated by Charles II. in 1660. The next and only remaining philoſophical ſociety of note, eſtabliſhed within this period, is the royal academy at Paris, founded by Lewis the XIVth, in 1666, and refounded and improved in 1699. Theſe two ſocieties are ſtill ſubſiſting, and they never fail to make annual publications of their obſervations and diſcoveries, which at preſent are very numerous, and contain a great treaſure of mathematical and philoſophical knowledge.

Thoſe of my readers, who have a general acquaintance with the hiſtory of philoſophy, will have a better idea of what they may expect in this period, from a brief enumeration of the principal names that will occur in it. But before I mention any of them, I muſt obſerve that I have found it moſt convenient to make this period of an indeterminate extent; as the hiſtory of it will comprize many obſervations and diſcoveries that were made in the time of Sir Iſaac Newton, if they were independent of his great diſcovery, concerning the different refrangibility of the rays of light, and if the authors of them were, upon the whole, prior to Newton; as, for the ſame reaſon, I propoſe to include in the laſt period of my hiſtory, thoſe diſcoveries of Newton's cotemporaries, that were made in purſuance of any of his diſcoveries, or which ſuppoſe, and refer to them.

Hitherto not ſo much as the name of an Engliſhman has occured in this hiſtory, except thoſe of the two Bacons; but about this period we find ſeveral of our countrymen putting in their claim to diſtinction, by the attention they gave to optical ſubjects; nor is their merit in this reſpect inconſiderable, though it was eclipſed by the incomparably ſuperior diſcoveries of Sir Iſaac Newton; whoſe fame in this ſcience is ſo great, as deſervedly to regulate the principal diviſions and periods of this hiſtory.

The firſt among Engliſh philoſophers within this period, was Mr. BOYLE; and his is a name that muſt be mentioned with honour in the hiſtory of almoſt every branch of experimental philoſophy. He was a gentleman equally diſtinguiſhed for his regard to religion and ſcience, and was happy in circumſtances that enabled him to ſpare no expence in purſuing his reſearches into nature, to every part of which he extended his enquiries, by numerous, well conducted, and generally ſucceſsful experiments; and he has recorded his diſcoveries with the greateſt perſpicuity and modeſty. His *Experiments and obſervations upon colours* was one of the earlieſt of his productions, being publiſhed in 1663, and it abounds with facts that were new to all his cotemporaries. A great part of them more properly belong to other branches of experimental philoſophy; but ſeveral of his uſeful remarks fall within the ſubject of this volume.

The next Engliſhman whoſe labours in this field claim our particular notice is JAMES GREGORY, whoſe *Optica promota* was alſo publiſhed in 1695. This gentleman examined with attention the cauſe of the diſtinctneſs of viſion, and the magnifying power of teleſcopes, and demonſtrated ſeveral propoſitions relating to theſe ſubjects, which had eſcaped the notice of former opticians. He alſo publiſhed in 1695 his *Elements of Dioptrics and Catoptrics* which, being a mathematical treatiſe, it is ſufficient barely to mention in this hiſtory. But the greateſt obligation we have to him is for the conſtruction of a reflecting teleſcope.

To Dr. ISAAC BARROW, tutor to Sir Iſaac Newton, and who greatly diſtinguiſhed himſelf by his ſkill in geometry, we owe ſeveral optical inveſtigations of a mathematical nature, and ſome new hints on the ſubject of viſion. Mr. Molyneux, alſo, the friend of Mr. Locke, publiſhed a valuable treatiſe on Dioptrics in 1693; but the contents of this work, as well as thoſe of the greater part of Dr. Barrow's, do not come within the ſubject of this hiſtory, being purely of a mathematical, and not of a philoſophical nature.

A very great number of articles, and ſome of conſiderable value, within this period, were contributed by that indefatigable and excellent experimenter Dr. HOOKE, ſecretary to the royal ſociety, and who, for many years, kept up the ſpirit and credit of that whole body. He himſelf, indeed, ſet much too high a value on his obſervations and improvements in opticks; but we ought not, for that reaſon, to neglect and undervalue them.

The Italian philoſophers made a principal figure in the former periods of this hiſtory, and they are not without their ſhare of fame in this period. Among them we find Father GRIMALDI, the companion of Riccioli, in his aſtronomical labours, very buſy and ſucceſsful in optical experiments; but except what was done by him, the Italians contributed little to the advancement of optical knowledge in this period, and my reader will be diſappointed if he expect to hear much more from them.

Among the French we find many illuſtrious names within this period, particularly that of M. DE LA HIRE, who took as much pains with the ſubject of philoſophy in general, as almoſt any man of his time. Thoſe of his obſervations which relate to the ſubject of this part of my work chiefly reſpect the theory of viſion; and his *Accidens de la vue* will always be conſidered as a valuable addition to the ſtock of knowledge upon that ſubject. To M. MARRIOTTE, of the ſame country, notwithſtanding his oppoſition to the doctrine of Newton, we are indebted for a curious diſcovery, which was the occaſion of ſome very important circumſtances relating to the theory of viſion being well diſcuſſed by ſeveral philoſophers of his time; among whom was M. PERRAULT, who alſo publiſhed ſome diſſertations on the tranſparency of bodies, and the cauſe of reflexion and refraction;

but they ſeem to contain mere theories, unſupported by experiment, and therefore I ſhall give no particular account of them.

No ſingle perſon took ſo much pains with the buſineſs of opticks within this period, or was ſo ſucceſsful in his enquiries, as M. HUYGENS. He began to compoſe his treatiſe of *Dioptrics* in the early part of his life, though it was not publiſhed till after his death. This treatiſe is ſo excellent, that Newton himſelf always expreſſed the greateſt eſteem for it, on account of the geometrical method of demonſtration obſerved in it, ſo like that of the ancients, of which he was a great admirer. M. Huygens was alſo very aſſiduous in his endeavours to improve teleſcopes, as will be ſeen in the laſt ſection of this period; nor is all his merit that of a geometrical or practical optician. He took great pains in a new and curious field of enquiry; I mean in his explication of the phenomena of halo's and mock-ſuns.

The celebrated OTTO GUERICKE, will not paſs wholly unnoticed in this part of my work, though it is not that in which he will make the moſt diſtinguiſhed figure.

I muſt not cloſe this ſection without obſerving, that within this period Father CHERUBIN, the ſame who took ſo much pains with the Binocular teleſcope, abovementioned, publiſhed his *Dioptric Oculaeri*, and *Viſion parfaite*, though they do not furniſh any materials for this philoſophical hiſtory.

SECTION I.

OBSERVATIONS AND DISCOVERIES CONCERNING LIGHT AND COLOURS IN GENERAL.

THE greateſt diſcovery that was made within this period, and, *a priori*, the moſt aſtoniſhing diſcovery that has yet been made, relating to the general theory of light, was finding the *velocity* with which it is tranſmitted. Nor was this great problem ſolved in conſequence of any direct attempt to do it; but, like moſt other great diſcoveries, was deduced from obſervations that were made with other views. Indeed, mankind had no idea of any velocity ſo great as that of light appears to be. Ariſtotle, we have ſeen, concluded, that if light was progreſſive at all, it could not but be perceived in its paſſage from the eaſt to the weſt; meaning, probably, that the progreſs of day light, along the ſurface of the earth, would have been viſible to the eye; and Galileo, who was the firſt perſon that actually attempted to meaſure the velocity of light, after the tranſmiſſion of it had for ages been generally thought to be inſtantaneous, was not without hopes of ſucceeding in the method that has been already deſcribed, with two men and torches.

Not diſcouraged by the diſappointment of Galileo, the members of the academy Del Cimento reſumed his experiments, placing their obſervers two miles from one another, which was at twice the diſtance at which Galileo had made the trial; but that velocity which could not be meaſured for the ſpace of one mile, was incapable of being meaſured in two; * and when we conſider what the thing which they attempted to meaſure afterwards appeared to be, we are far from being ſurprized at their ill ſucceſs, and are rather diſpoſed to ſmile at their attempts.

At length this great diſcovery was made, in conſequence of ſome obſervations of the eclipſes of Jupiter's ſatellites, taken at the obſervatory belonging to the royal academy of ſciences at Paris, from 1670 to 1675; but all the concluſion that was at firſt drawn from them, was, that thoſe ſatellites were ſubject to a ſpecies of irregularity in their revolutions, which had not been noted before. The principal fact was, that the firſt ſatellite ſometimes emerged exactly at the time calculated by the tables, and ſometimes not; in ſo much that the greateſt variation was about 14 minutes. The particular obſervation that ſtruck theſe philoſophers the moſt, was the emerſion of this ſatellite obſerved at Paris, November 9, 1676, 10 minutes later than it had been obſerved in the month of Auguſt, when the earth was much nearer to Jupiter.†

Meſſrs. Caſſini and Roemer, examining this circumſtance with attention, both concluded that it depended upon the diſtance of Jupiter with reſpect to the earth; and that, in order to account for it, they muſt ſuppoſe that the light was about 14 minutes in croſſing the earth's orbit. Caſſini, who publiſhed his firſt thoughts on this ſubject in Auguſt 1675, preſently abandoned this hypotheſis; but M. Roemer ſtrenuouſly maintained it. § M. Maraldi

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* Muſchenbroeck's Tentamina, part 2, p. 183.

† Phil. Tranſ. ab. Vol. 1, p. 409.

§ Ac. Par. 1707. H. p. 96.

raldi afterwards, in a memoir preſented to the academy in 1707, p. 32, endeavoured to ſhew, that though the hypotheſis of the ſucceſſive propagation of light would account for the appearances of the firſt ſatellite of Jupiter, it did not agree with that of others, or with the eclipſes of thoſe of Saturn. But this oppoſition was ineffectual. M. Roemer had already brought the greateſt part of aſtronomers to his opinion; and later, and more accurate obſervations have ſhewed it to be perfectly agreeable to all celeſtial appearances.

To give an idea of the nature of this proof of the ſucceſſive propagation of light, let S, fig. 51, repreſent the ſun, and J Jupiter, moving in his orbit. If, when the earth is at *t*, an eclipſe of any of the ſatellites of Jupiter, or any other appearance in Jupiter, ſhould conſtantly happen 14 minutes later than it is obſerved to do when the earth is at T, it is evident that the light, by which it is viſible to us, muſt have ſpent 14 minutes in paſſing from T to *t*; which being the whole diameter of the earth's orbit, half of that time, or ſeven minutes, muſt be allowed for its paſſage from the ſun to us; a velocity exceeding any thing that we know in nature, for ſince the late tranſit of Venus, this diſtance is now ſuppoſed to exceed 90 millions of miles.*

The greateſt part of the obſervations and experiments of Mr Boyle relate to the ſubject of this ſection, being intended to diſcover the firſt principles and cauſes of colours, and the conſtitution of bodies on which they depend. This inquiry he was naturally led into by the taſte he had for chymiſtry, in which branch of natural philoſophy experiments were firſt made, and about which no perſon was even more aſſiduous than himſelf.

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* The beſt account we have yet of the ſun's mean parallax, from the laſt tranſit, makes it 8′, 86″, and conſequently his diſtance about 92,000000.

A variety of obſervations led Mr. Boyle to conclude that diverſity of colours did not always denote any great difference in the internal ſtructure of bodies; yet he was induced to think that it was often an indication of conſiderable alterations in the diſpoſition of their parts, as appears, he ſays, from the extraction of tinctures, wherein the change of colour is the chief, and ſometimes the only thing by which the artiſt regulates his proceſs in their preparation. Inſtances of this are alſo obvious in ſeveral ſorts of fruit, wherein, according as the vegetable ſap is ripened, by paſſing from one degree of maturation to another, the external part of the fruit changes from one colour to another. A leſs obvious inſtance of this kind is afforded us, he ſays, by the method of tempering ſteel for gravers, drills, ſprings, &c. which is this. Firſt the ſteel to be tempered is hardened, by heating it in glowing coals, and not quenched as ſoon as taken from the fire, but held over a baſon of water, till it paſs from a white heat to a red one, when it is immediately quenched in cold water. The ſteel thus hardened will, if it be good, look whitiſh, and being brightened at the end, and held in the flame of a candle, that the bright end may lie about half an inch diſtant from the flame, it will ſwiftly paſs from one colour to another; as from a bright yellow to a deeper and reddiſh yellow, from that firſt to a fainter, and then to a deeper blue; each of which ſucceeding colours argues ſuch a change made in the texture of the ſteel, that, if it be taken from the flame, and immediately quenched in tallow, whilſt it is yellow, it will be of ſuch an hardneſs as fits it for drills; but if kept for a few minutes longer in the flame, till it turns blue, it becomes much ſofter, and proper to make ſprings for watches; which are, therefore, commonly of that colour. Laſtly, if the ſteel be kept in the flame after the deep blue has appeared, it will grow too ſoft even for penknives. Any perſon, he ſays, may eaſily ſatisfy himſelf with reſpect to the different hardneſs of ſteel of different colours, either by the file, or by breaking ſome ſlender wires thus tempered, and obſerving how they vary in point of brittleneſs. * Theſe colours, though very vivid, yet, he ſays, if the metal be broken, will appear to be ſuperficial; not only the innermoſt parts of the ſteel, but thoſe within an hair's breadth of its ſurface, having none of theſe colours. †

Similar to theſe ſuperficial colours on ſteel, are thoſe which our author obſerved in melting a large quantity of pure lead with a ſtrong fire. For, pouring it immediately into a clean iron veſſel, and carefully and nimbly taking off the ſcum, he perceived the ſmooth gloſſy ſurface of the melted metal to be adorned with a very delightful colour, which almoſt immediately gave place to another vivid one, that was as quickly ſucceeded by a third, and again by a fourth. Thus, ſays he, theſe wonderfully vivid colours ſucceſſively appeared and vaniſhed, till the metal began to cool, when thoſe colours which happened to come up laſt remained upon it, but quite ſuperficial, like thoſe on the ſteel. However, the ſame colours did neither always, nor regularly ſucceed one another, as is uſual in ſteel; but in the following irregular order, which paſſed ſo ſuddenly, that he was hardly able to commit it to paper; blue, yellow, purple, blue, green, purple, blue, yellow, red, purple, blue, yellow, and blue, yellow, blue, purple, green, mixed yellow, red, blue, green, yellow, red, purple, green. §

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* Boyle's Works by Shaw, Vol. 2, p. 1. † Ib. p. 5. § Ib. p. 5.

Bodies of very different kinds he found to exhibit the ſame colours, in certain circumſtances. Viewing a ſphere of rock cryſtal, which was ſawn aſunder by a lapidary; and expoſing the flat ſurfaces to the ſun beams, the little particles, which, notwithſtanding their ſeeming ſoothneſs in the ſhade, rendered their ſurfaces rough, ſo refracted the light, as to exceed the vivid colours of the rainbow. He alſo obſerved the ſame colours on the ſurface of a fine grained touchſtone; ſo that, notwithſtanding the great tranſparency of the cryſtal, and the great opacity of the touchſtone, their ſuperficial corpuſcles exhibited the ſame vivid colours.*

The difference between *white* and *black* was, as we have ſeen, pretty well underſtood by Deſcartes; but as Mr. Boyle made ſeveral obſervations and experiments, which demonſtrate his theory in a very ſatisfactory manner, I think that an account of them ought not to be omitted in this place. Many learned men, he obſerves, ſuppoſed that ſnow affects the eyes, not by a *borrowed*, but by a *native* light; but having placed a quantity of ſnow in a room, from which all foreign light was carefully excluded, neither he nor any other perſon could perceive it.†

To try whether white bodies reflect more light than others, he held a ſheet of white paper in a ſun beam admitted into a darkened room; and obſerved that it reflected a far greater light than a paper of any other colour, a conſiderable part of the room being enlightened by it.

Farther, to ſhew that white bodies reflect the rays outwards, he adds, that common burning glaſſes will not, of a long time, burn, or diſcolour white paper. When he was a boy, he ſays, and took great pleaſure in making experiments with theſe glaſſes, he was much ſurpriſed at this remarkable circumſtance; and it ſet him very early upon gueſſing at the nature of whiteneſs, eſpecially as he obſerved that the image of the ſun was not ſo well defined upon white paper as upon black one; and as, when he put ink upon the paper, the moiſture would be quickly dried up, and the paper which he could not burn before, would preſently take fire. He alſo found that by expoſing his hand to the ſun, with a thin black glove upon it, it would be ſuddenly, and more conſiderably heated, than if he held his naked hand to the rays, or put on a glove of thin white leather. *

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* Boyle's Works by Shaw, Vol. 2, p. 6. † Ib. p. 29.

To prove that black is the reverſe of white, with reſpect to its property of reflecting the rays of the ſun, he procured a large piece of black marble; and having got it ground into the form of a large ſpherical concave ſpeculum, he found that the image of the ſun reflected from it was far from offending or dazzling his eyes, as it would have done from another ſpeculum; and though this was large, he could not in a long time, ſet a piece of wood on fire with it; though a far leſs ſpeculum, of the ſame form, and of a more reflecting ſubſtance, would preſently have made it flame. †

To ſatisfy himſelf ſtill farther with reſpect to this ſubject, he took a broad and large tile; and having made one half of its ſurface white, and the other black, he expoſed it to the ſummer ſun. And having let it lie there ſometime, he found that, while the whited part remained cool, the part that was black was grown very hot. For his farther ſatisfaction, he ſometimes left part of the tile of its native red; and, after expoſing the whole

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* Boyle's Works by Shaw, p. 59. † Ib. p. 34.

to the ſun, obſerved that this part grew hotter than the white, but was not ſo hot as the black part. He alſo obſerves that rooms hung with black are not only darker, than they would otherwiſe be, but *warmer* too; and he knew ſeveral perſons, who found great inconvenience from rooms hung with black. As another proof of his hypotheſis, he informs us that a Virtuoſo, of unſuſpected credit, acquainted him, that, in a hot climate, he had ſeen eggs well roaſted in a ſhort time, by firſt blacking the ſhells, and then expoſing them to the ſun.*

Mr. Boyle obſerved a manifeſt difference in ſome kinds of coloured bodies viewed by day light, and afterwards by the light of the moon. Yellow paper appeared much fainter by moon light than in the day, inclining to a pale ſtraw colour. Red paper was but little changed. It only reflected the light more ſtrongly than the other colours. A fair deep green, looked upon by itſelf, ſeemed to be of a dark blue; but viewed together with a dark blue appeared greeniſh, and compared with yellow appeared more blue than at firſt. The blue looked more like a deep purple than it had done in the day light. The purple ſeemed very little altered. Laſtly, the red, compared with the yellow, made the latter appear almoſt like brown cap paper.†

An obſervation of M. Homberg, concerning the *intenſity* of different kinds of light, whereby ſome more eaſily pervade ſemi-tranſparent ſubſtances than others, is worth mentioning. He ſhut himſelf up in a room, into which no light could be admitted, but through one aperture, which was cloſed with a piece of glaſs, at firſt quite rough, and nearly opaque, through which he could perceive the general ſhape of objects, but not their particular colours; but poliſhing the ſurface of this piece of glaſs by degrees; he was firſt able to ſee *white* objects through it. Poliſhing it a little more, he could diſtinguiſh thoſe that were *yellow*, then thoſe that were *green*, *red*, and *blue*, in the order here mentioned, and laſtly thoſe that were *black*. It is rather probable that theſe obſervations were not very accurately made; or I ſhould think that *red* would have been perceived before green. However, according to the obſervations of Newton and others, yellow, or rather orange, is more offenſive to the eye than red, and may therefore have been viſible before it.*

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* Boyle's Works, p. 36. † Ib. p. 58.

We ſhall be diſappointed if we expect more conſiſtent, or better founded ſyſtems, concerning the nature of *colour*, in this period, than in the laſt. The moſt extravagant opinion that was, perhaps, ever formed upon this ſubject, was that of Dr. Hooke. For, whereas before this period, it had generally been ſuppoſed that there are *three* primary colours, he maintained that there are but *two*, blue and red, and that all the reſt are compoſed of them. Blue, ſays he, is an impreſſion on the retina of an oblique and confuſed pulſe of light, whoſe weaker part precedes, and whoſe ſtronger follows; and red is an impreſſion on the retina, of an oblique and confuſed pulſe of light, whoſe ſtronger part goes before, and whoſe weaker part follows.†

The fantaſm of colour, Dr. Hooke ſays, is cauſed by the ſenſation of the oblique, or uneven pulſe of light, which is capable of no more varieties than two, which ariſe from the two ſides of the oblique pulſe, though they be capable of infinite gradations; each of them beginning from white and ending,

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* Du Hamel's Hiſt. p. 535. † Micrographia, p. 64.

ing, one of them in the deepeſt ſcarlet or yellow, and the other in the deepeſt blue.*

Imagining that blue and red contained the principles of all the other colours, he thought to have verified this hypotheſis by experiment; and, for this purpoſe, he filled one priſmatic glaſs veſſel with a deep tincture of copper, which gave a beautiful blue, and another with a deep tincture of aloes, which exhibited a beautiful red. Now ſince, at the edges of theſe priſms, the colours were fainter, and where there was a greater quantity of the fluid, the colour was deeper, he thought to have produced from them all the variety of colours imaginable, by placing the two ſides together and the edges contrary ways, and looking through them in different places. But though he did not meet with what he was looking for, he ſays he found ſomething more admirable; which was that he could not ſee through them at all, when they were both placed together; though ſeparately they were ſufficiently tranſparent, and he could ſee through twice the thickneſs of either of them.† This experiment was afterwards explained by Sir Iſaac Newton, who obſerves, that one kind of rays being ſtopped by the red liquor, and the other by the blue, none were tranſmitted through both.

M. De la Hire, whoſe miſtakes did not ariſe from want of attention to the ſubject, makes all difference of colour to depend upon the degree in which the light (by which, no doubt, he meant the ſame light) agitates or affects the optic nerve; and that whatever tends to weaken this impreſſion makes a change in the colour. Thus, he ſays, that the red blood appears blue, on account of the interpoſition of the ſkin, and that the air, which is made white by the rays of the ſun, appears blue on account of the black ground of the unenlightened ſpace beyond it. He alſo makes other obſervations on the ſubject as trifling as theſe, and yet the paper which contains them was written a conſiderable time after Newton's hypotheſis, of the different colours depending upon the different refrangibility of the rays of light, had been publiſhed, and had engaged a general attention. *

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* Micrographia, p. 67. † Ib. p. 74.

SECTION II.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE REFLEXION OF LIGHT.

I SHALL introduce this ſection, which is appropriated to obſervations on the *reflexion* of light, with an account of ſome experiments made by the members of the academy del Cimento, intended to verify, or refute an odd opinion of Kepler, viz. that lenſes form images of bodies by means of the air, which, adhering to the ſurface of the glaſs, is of the ſame uſe in reflecting the light, as a piece of leaf tin. To try this, they cemented a lens to the mouth of a glaſs veſſel, which ended in a tube, the length of a barometer; and having filled it with mercury, and inverted it (which was the only method they then had of making a vacuum) they obſerved that, when this apparatus was placed in a darkened room, the image of a candle, formed by rays tranſmitted through the lens, was the ſame when there was no air contiguous to the lens, as when there was. Leſt there ſhould be any objection to the accuracy of their experiment, from particles of mercury, adhering to the glaſs, they introduced a quantity of ſpirit of wine through the quickſilver, that it might lie next the glaſs, in order to waſh it, and, by its quick evaporation to wipe it, as it were. *

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* Ac. Par. 1711. M. 100.

To this ſection belongs the account of the remarkable properties of the infuſion of *lignum nephriticum*, with reſpect to light tranſmitted through it, or reflected by it; the capital facts concerning which were fully aſcertained by Mr. Boyle, though ſomething extraordinary had been obſerved in it by Kircher, in the preceding period. He deſcribes this lignum nephriticum to be a whitiſh kind of wood, that was brought from Mexico, which the natives call *coatl* or *tlapazatli*, and which had been thought to tinge water of a green colour only; but he ſays that he found it to communicate all kinds of colours. If, ſays he, an infuſion of this wood be put into a glaſs globe, and expoſed to a ſtrong light, it will be as colourleſs as pure water; but if it be carried into a place a little ſhaded, it will be a moſt beautiful green. In a place ſtill more ſhaded it will incline to red, and in a very ſhady place, or in an opaque veſſel, it will be green again. †

A cup of this remarkable wood was ſent to Kircher by the procurator of his ſociety at Mexico, and was preſented by him to the Emperor, as a great curioſity. It is called *lignum nephriticum*, becauſe the infuſion of it was imagined to be of ſervice in diſeaſes of the kidneys and bladder, and the natives of the country where it grows do made uſe of it for that purpoſe.

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* Muſchenbroeck's Tentamina, p. 66. † Phyſiologia, p. 57.

The changeableneſs in colour of this infuſion, our philoſopher acknowledges, puzzled him exceedingly at firſt; as he did not know whether, according to the ſyſtem then in vogue, thoſe colours were *real*, exiſting in the wood, or only *apparent*, ariſing from the light variouſly affected. At length, however, he ſays, that he found the cauſe of this appearance, and promiſes to explain it elſewhere; but, as far as I can find, without ever making good his promiſe.[†]

Mr. Boyle corrected ſeveral of the haſty obſervations of Kircher concerning the colours that appear in the infuſion of lignum nephriticum, and he diverſified the experiments with it in a very pleaſing manner. Several of his obſervations are ſo curious, that I cannot help reciting them, though I am ſenſible they will carry me farther than I am under neceſſity of going within the bounds of chymiſtry.

He firſt diſtinctly noted the two very different colours which this remarkable tincture exhibits, by *tranſmitted* and *reflected* light. If, ſays he, it be held directly between the light and the eye, it will appear tinged (excepting the very top of it, where a ſky coloured circle ſometimes appears) almoſt of a golden colour, except the infuſion be too ſtrong, in which caſe it will be dark or reddiſh, and requires to be diluted with water. But if it be held from the light, ſo that the eye be between the light and the phial, it will appear of a deep lovely blue colour, as will alſo the drops, if any lie on the outſide of the glaſs.

When a little of this tincture was poured upon a ſheet of white paper, and placed in a window, where the ſun could ſhine upon it, he obſerved that, if he turned his back upon the ſun, the ſhadow of his pen, or any ſuch ſlender ſubſtance, projected upon the liquor, would not be all dark, like other ſhadows; but that part of it would be curiouſly coloured, the edge of it next the body being almoſt of a lively golden colour, and the more remote part blue. Theſe, and other experiments, of a ſimilar nature, many of his friends, he ſays, beheld with wonder; and he remembered an excellent occuliſt, who accidentally meeting with a phial full of this liquor, and being unacquainted with this remarkable property of it, imagined, after he had viewed it a long time, that ſome new and ſtrange diſtemper had ſeized his eyes; and Mr. Boyle himſelf acknowledges, that the oddneſs of the phenomena made him very deſirous to find out the cauſe of it; and his inquires were not altogether unſucceſsful.

[†] Ars Magna, p. 57.

Obſerving that this tincture, if it were too deep, was not tinged in ſo beautiful a manner, and that the impregnating virtue of the wood did, by being frequently infuſed in freſh water, gradually decay, he conjectured that the tincture contained much of the eſſential ſalt of the wood; and to try whether the ſubtle parts, on which the colour depended, were volatile enough to be diſtilled, without diſſolving their texture, he applied ſome of it to the gentle heat of a lamp furnace; but he found all that came over was as limpid and colourleſs as rock water, while that which remained behind was of ſo deep a blue, that it was only in a very ſtrong light that it appeared of any colour.

Suſpecting that the tinging particles abounded with ſalts, whoſe texture, and the colour thence ariſing would probably be altered by acids, he poured into a ſmall quantity of it a very little ſpirit of vinegar, and found that the blue colour immediately vaniſhed, while the golden one remained, on which ever ſide it was viewed with reſpect to the light.

Upon this he imagined that as the acid ſalts of the vinegar, had been able to deprive the liquor of its blue colour, a ſulphureous ſalt, which is of a contrary nature, would deſtroy their effects; and having placed himſelf betwixt the window and the phial, and let fall into the ſame liquor a few drops of oil of tartar *per deliquium*, he found that it was immediately reſtored to its former blue colour, and exhibited all the ſame phenomena which it had done at the firſt. *

Having ſometimes brought a round long necked phial, filled with this tincture, into a darkened room, into which a beam of the ſun was admitted, by a ſmall aperture; and holding the phial ſometimes near the ſun beams, and ſometimes partly in them and partly out of them, changing alſo the poſition of the glaſs, and viewing it from ſeveral parts of the room, it exhibited a much greater variety of colours than it did in an enlightened room. Beſides the uſual colours, it was red in ſome places, and green in others, and within were intermediate colours produced by the different degrees and odd mixtures of light and ſhade. †

It was not only in this tincture of lignum nephriticum that Mr. Boyle obſerved the difference between reflected and tranſmitted light. He obſerved it even in gold, though no perſon explained the cauſe of theſe effects before Sir Iſaac Newton. He took a piece of leaf gold, and holding it betwixt his eye and the light, obſerved that it did not appear of a golden colour, but of a greeniſh blue. He alſo obſerved the ſame change of colour by candle light; but the experiment did not ſucceed with a leaf of ſilver. §

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* Boyle's Works by Shaw, Vol. 2, p. 60. † Ib. p. 62. § Ib. p. 59.

The principal phenomena of this infuſion of lignum nephriticum are eaſily explained on the Newtonian doctrine of the different refrangibility and reflexibility of the rays of light, and the diſpoſition of particular bodies to reflect ſome kinds of rays and tranſmit others; ſo that if the reflected light fall upon the eye, they ſhall appear to be of one colour, when by the tranſmitted light they appear to be of another. The conſtitution of the atmoſphere and of the ſea, we ſhall find, by obſervations made in later periods, to be ſimilar to that of this infuſion; for the blue rays, and others of a faint colour, do not penetrate ſo far into them as the red, and others of a ſtronger colour; but what this conſtitution is, which is common to them all, deſerves to be inquired into. For almoſt all other tinctures, and this of lignum nephriticum too, after ſome change made in it by Mr. Boyle, as well as all other ſemi-tranſparent coloured ſubſtances, as glaſs, appear of the ſame hue in all poſitions of the eye. To increaſe or diminiſh the quantity makes no difference, but to make the colour deeper or more dilute.

The firſt diſtinct account of the colours exhibited by thin plates, of various ſubſtances, I have met with among the obſervations of Mr. Boyle. To ſhew the chymiſts that colours may be made to appear or vaniſh, where there is no acceſſion or change either of the ſulphureous, the ſaline, or the mercurial principle of bodies, he obſerves that all chymical eſſential oils, as alſo good ſpirit of wine, being ſhaken, till they riſe in bubbles, appear of various colours; which immediately vaniſh when the bubbles burſt, ſo that a colourleſs liquor may be immediately made to exhibit a variety of colours, and loſe them in a moment, without any change in its eſſential principles. He then mentions the colours that appear in bubbles of ſoap and water, and alſo in turpentine. He ſometimes got glaſs blown ſo thin as to exhibit ſimilar colours, and obſerves that a feather, of a proper ſhape and ſize, and alſo a black ribbon, held at a proper diſtance, between his eye and the ſun, ſhewed a variety of little rainbows, as he calls them, with very vivid colours, none of which were conſtantly to be ſeen in the ſame objects. *

Much more pains were taken with this ſubject, and a much greater number of obſervations reſpecting it were made by Dr. Hooke. As he loved to give ſurprize by his diſcoveries, he promiſed, at a meeting of the ſociety on the 7th of March 1672, to exhibit, at their next meeting, ſomething which had neither reflexion nor refraction, and yet was diaphanous. Accordingly, at the time appointed, he produced the famous coloured bubble of ſoap and water, of which ſuch admirable uſe was afterwards made by Sir Iſaac Newton, but which Dr. Hooke and his cotemporaries ſeem to have overlooked in Mr. Boyle's treatiſe on colours, though it was publiſhed nine years before. It is no wonder that ſo curious an appearance excited the attention of that inquiſitive body, and that they ſhould deſire him to bring an account of it in writing at their next meeting. This he accordingly did, and, in order that my reader may compare this firſt account of it with the more accurate deſcription that he will hereafter meet with from Sir Iſaac Newton, I ſhall here introduce it.

By the help of a ſmall glaſs pipe there were blown ſeveral ſmall bubbles, out of a mixture of ſoap and water; where it was obſervable, that, at firſt, they appeared white and clear; but that, after ſome time, the film of water growing thinner,

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* Boyle's Works by Shaw, Vol. 2, p. 70.

there appeared upon it all the colours of the rainbow. Firſt a pale yellow, then orange, red, purple, blue, green, &c. with the ſame ſeries of colours repeated; in which it was farther obſervable, that the firſt and laſt ſeries were very faint, and that the middlemoſt order or ſeries was very bright. After theſe colours had paſſed over the changes above mentioned, the film of the bubble began to appear white again, and preſently, in ſeveral parts of this ſecond white film, there appeared ſeveral holes; which by degrees grew very big, ſeveral of them running into one another. After reciting other obſervations, which are not of much conſequence, he ſays it is ſtrange, that though both the *encompaſſing* and *encompaſſed* air have ſurfaces, yet he could not obſerve that they afforded either reflexion or refraction, which all the other parts of the encompaſſed air did. This experiment, he ſays, at firſt ſight, may appear very trivial, yet, as to the finding out the nature and cauſe of reflexion, refraction, colours, congruity and incongruity, and ſeveral other properties of bodies, he looked upon it as one of the moſt inſtructive. And he promiſed to conſider it more afterwards,* but I do not find that he ever did; nor indeed is it to be much regretted, as we ſhall ſoon find this buſineſs in better hands. He adds, that that which gives one colour by reflexion, gives another by *trajection*; not much unlike the tincture of lignum nephriticum, and he promiſes an explication of theſe phenomina, but without fulfilling his promiſe; which, for the *reaſon* given above, it is probable we have not much *reaſon* to regret.†

Dr. Hooke was the firſt to obſerve, if not to deſcribe the beautiful colours that appear in thin plates of muſcovy glaſs. Theſe, he ſays, are very beautiful to the naked eye, but much more when they are viewed with a microſcope. With this inſtrument he could perceive that theſe colours were ranged in rings ſurrounding the white ſpecks, or flaws, in this thin ſubſtance, that the order of the colours was the very ſame as in the rainbow, and that they were often repeated ten times. But the colours, he ſays, were diſpoſed as in the outer bow, and not the inner. Some of them, alſo, were much brighter than others, and ſome of them very much broader. He alſo obſerved that, if there was a place where the colours were very broad, and conſpicuous to the naked eye, they might be made, by preſſing the place with the finger, to change places, and move from one part to another. Laſtly, he obſerved that, if great care be uſed, this ſubſtance may be ſplit into plates of $\frac{1}{8}$ or $\frac{1}{6}$ of an inch in diameter, each of which will appear, through a microſcope, to be uniformly adorned with ſome one vivid colour, and that theſe plates will be found upon examination to be of the ſame thickneſs throughout.*

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* Birch's hiſtory, Vol. 3, p. 29. † Ib. Vol. 3, p. 54.

As a fact ſimilar to this, but obſerved previous to it, I ſhall here mention that Lord Brereton, at a meeting of the royal ſociety in 1666, produced ſome pieces of glaſs taken out of a window of a church, both on the north and on the ſouth ſide of it; obſerving, that they were all eaten in by the air, but that the piece taken from the ſouth ſide had ſome colours, like thoſe of the rainbow upon it; which the others, on the north ſide, had not.† This phenomena has been frequently obſerved ſince, and in other circumſtances. It is not to be doubted, but that in all theſe caſes, the glaſs is divided into thin plates, which exhibit colours, upon the ſame principle with thoſe which Dr. Hooke obſerved in the bubble of ſoap and water, and in the thin plate of air, which we ſhall find more fully explained by Sir Iſaac Newton. With care the thin plates of the glaſs may be ſeparated, and the theory verified.

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* Micrographia, p. 48. † Ib. Vol. 2, p. 104.

Among ſome of the experiments which Dr. Hooke rehearſed before the royal ſociety, in order to refute Newton's theory of colours, which had juſt then been communicated to them, there is one which I muſt not omit to recite in this place. Dr. Hooke having, at leaſt, an equal right to the honour of the firſt obſervation with that more celebrated philoſopher, though he did not make the ſame uſe of it. It reſpects the colours that are exhibited by a plate of air, and, like the preceding articles, may give my readers an opportunity of comparing the earlier and more rude, with the later and more accurate obſervations of the ſame remarkable phenomenon.

He took two thin pieces of glaſs, ground plane and poliſhed, and putting them one upon another, he preſſed them till there began to appear a red coloured ſpot in the middle; and preſſing them cloſer, he obſerved ſeveral rings of colours encompaſſing the firſt place, till, at laſt, all the colours diſappeared out of the middle of the circles, and the central ſpot appeared white; and yet, when he continued to preſs the plates together, the white would, in ſeveral places, turn to black. The firſt colour that appeared was red, then yellow, then green, then blue, then purple, then red again; yellow, green, blue, and purple; and again in the ſame order, ſo that he ſometimes counted 9 or 10 of theſe circles, the red immediately next to the purple; and the laſt colour that appeared before the white was blue; ſo that it began with red, and ended with purple. Theſe rings, he ſays, would change their places, by changing the poſition of the eye, ſo that, the glaſſes remaining the ſame, that part, which was red in one poſition of the eye, was blue in a ſecond, green in a third, &c. *

I ſhall cloſe this ſection with mentioning an obſervation made by Otto Guericke, by which he well explains the reaſon why ſtars are viſible at the bottom of a deep well. It is, ſays he, becauſe the light that proceeds from them is not overpowered by the rays of the ſun, which are loſt in the number of reflexions which they muſt undergo in the pit, ſo that they can never reach the eye of a ſpectator at the bottom of it. §

SECTION III.

OBSERVATIONS RELATING TO THE REFRACTION OF LIGHT.

IN this period we find ſeveral members of the royal ſociety buſy in making experiments on the refractive powers of different mediums, but they did ſo little, that it is hardly worth reciting. At a meeting of the ſociety Auguſt 31, 1664, an experiment for meaſuring the refraction of common water was made with a new inſtrument, which they had prepared for that purpoſe, and the angle of incidence being 40 degrees the angle of refraction was 30.†

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* Birch's hiſtory, Vol. 3, p. 54.

§ Experimenta Magdeburgica, p. 141.

† Birch's hiſtory, p. 463.

About this time it is that I find the firſt mention of mediums not refracting the light in an exact proportion to their denſities. For Mr. Boyle, in a letter to Mr. Oldenburgh, dated November 3, 1664, obſerves that, in ſpirit of wine, the proportion of the ſines of the angles of incidence, to the ſines of the angles of refraction were nearly the ſame; viz. as 4 to 3, and that as ſpirit of wine occaſions a greater refraction than common water, ſo oil of turpentine, which is lighter than ſpirit of wine, produces not only a greater refraction than common water, but a much greater than ſalt water.* And at a meeting held November 9, the ſame year, Dr. Hooke, who had been ordered to proſecute the experiment, brought in an account of one that he had made with pure and clear ſallad oil, which was found to have produced a much greater refraction than any liquor which he had then tried; the angle of refraction that anſwered to an angle of incidence of 30° being found no leſs than 40°, 30′, and the angle of refraction that anſwered to an angle of incidence of 20° being 29°, 47′.†

M. De la Hire, alſo, made ſeveral experiments to aſcertain the refractive power of oil with reſpect to that of water and air, and found the ſine of the angle of incidence to that of refraction to be as 60 to 42, which he obſerves is a little nearer to that of glaſs than to that of water, though oil is much lighter than water, and glaſs much heavier. §

The members of the royal ſociety, finding that the refraction of ſalt-water exceeded that of freſh, purſued the experiment a little farther, with ſolutions of vitriol, ſalt petre, and alum in water; when they found the refraction of the ſolution of vitriol and ſalt petre a little more, but that of alum, a little leſs than common water. *

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* Birch's hiſtory, Vol. 1, p. 480. † Ib. p. 483. § Memoires Anciennes, Vol. 9, p. 382.

Dr. Hooke made an experiment before the royal ſociety, February 11, 1663, which clearly proves that ice refracts the light leſs than water, which he took to be a good argument, that the lightneſs of ice, which cauſes it to ſwim in water, is not cauſed only by the ſmall bubbles which are viſible in it; but that it ariſes from the uniform conſtitution, or general texture of the whole maſs. † M. De la Hire alſo took a good deal of pains to determine whether, as was then the common opinion, the refractive power of ice and water were the ſame, and he found, as Dr. Hooke had done before, that ice refracts leſs than water. §

By a moſt accurate and elaborate experiment, made in the year 1698, in which a ray of light was tranſmitted through a Torricellian vaccuum, Mr. Lowthorp found that the refractive power of air is to that of water as 36 to 34400. He concludes his account of the experiment with obſerving, that the refractive power of bodies is not proportioned to the denſity, at leaſt not to the gravity of the refracting medium. For the refractive power of glaſs to that of water, is as 55 to 34, whereas its gravity is as 87 to 34; that is the ſquares of their refractive powers are (very nearly) as their reſpective gravities. And there are ſome fluids, which, though they are lighter than water, yet have a greater power of refraction. Thus the refractive power of ſpirit of wine, according to Dr. Hooke's experiment, is to that of water as 36 to 33, and its gravity reciprocally as 33 to 36, or 36½.

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* Birch's hiſtory, Vol. 1, p. 501. † Hooke's experiments by Derham, p. 26.
§ Ac. Par. 1693, Mem. p. 25.

But the refractive powers of air and water ſeem to obſerve the ſimple proportion of their gravities directly. And if this ſhould be confirmed by ſucceeding experiments, it is probable, he ſays, that the refractive powers of the atmoſphere are every where, and at all heights above the earth, proportioned to its denſity and expanſion; and then it would be no difficult matter to trace the light through it, ſo as to terminate the ſhadow of the earth; and, together with proper expedients for meaſuring the quantity of light illuminating an opaque body, to examine at what diſtances the moon muſt be from the earth to ſuffer eclipſes of the obſerved durations. Some farther account of the refraction of the atmoſphere written by Mr. Lowthorp, may be ſeen in Dr. Hooke's experiments publiſhed by Derham, p. 338.

Caſſini the younger, happened to be preſent when Mr. Lowthorp made the above-mentioned experiment before the royal ſociety, and upon his return home, having made a report of it to the members of the royal academy of ſciences, thoſe gentlemen endeavoured to repeat the experiment in 1700; but they did not ſucceed. For, as they ſaid, beams of light paſſed through the vacuum without ſuffering any refraction. The royal ſociety, being informed of this, were deſirous that it might be put paſt diſpute, by repeated and well atteſted trials, and ordered Mr. Haukſbee to make an inſtrument for the purpoſe, by the direction of Dr. Halley. It conſiſted of a ſtrong braſs priſm, two ſides of which had ſockets to receive two plane glaſſes; whereby the air in the priſm might either be exhauſted or condenſed. The priſm had alſo a mercurial gage fixed to it, to diſcover the denſity of the contained air, and was contrived to turn upon its axis, in order to make the refractions equal on each ſide, when it was fixed to the end of a teleſcope. The refracting angle was near 64°, and the length of the teleſcope was about 10 feet, having a fine hair in its focus. The event of this accurate experiment was as follows:

Having choſen a proper and very diſtinct erect object, whoſe diſtance was 2588 feet, June: 15: old ſtyle 1708, in the morning, the barometer being then at 29. 7. ½, and the thermometer at 60, they firſt exhauſted the priſm, and then applying it to the teleſcope, the horizontal hair in the focus covered a mark on the object diſtinctly ſeen through the vacuum, the two glaſſes being equally inclined to the viſual ray. Then admitting the air into the priſm, the object was ſeen to riſe above the hair gradually, as the air entered, and in the end the hair was obſerved to hide a mark 10¼ inches below the former mark. This they often repeated, and with the ſame ſucceſs.

After this they applied the condenſing engine to the priſm, and having forced in another atmoſphere, ſo that the denſity of the included air was double to that of the outward, they again placed it before the teleſcope, and letting out the air, the object which before ſeemed to riſe, appeared gradually to deſcend, and the hair at length reſted on an object higher than before by the ſame interval of 10¼ inches. This experiment they likewiſe frequently repeated, without any variation in the event.

They then forced in another atmoſphere, and upon diſcharging the condenſed air, the object was ſeen near 21 inches lower than before.

Now the radius in this caſe being 2588 feet, 10 ¼ inches will ſubtend an angle of one minute and 8 ſeconds, and the angle of incidence of the viſual ray being 32 degrees (becauſe the angle of the glaſs planes was 64) it follows from the known laws of refraction; that as the ſine of 39° is to that of 31°, 59′, 26″; differing from 32° by 34″ the half of 1′. 8″ ſo is the ſine of any other incidence, to the ſine of its angle of refraction; and ſo is radius, or 1000000, to 999736; which, therefore, is the proportion between the ſine of incidence in vacuo and the ſine of refraction from thence into common air.

It appears, by theſe experiments, that the refractive power of the air is proportionable to its denſity. And ſince the denſity of the atmoſphere is as its weight directly, and its heat inverſly, the ratio of its denſity, at any given time, may be had by comparing the heights of the barometer and thermometer; and thence he concludes that this will alſo be the ratio of the refraction of the air.* But Dr. Smith obſerves, that, before we can depend upon the accuracy of this concluſion, we ought to examine whether heat and cold alone may not alter the refractive power of air, while its denſity continues the ſame. This, he ſays, may be tried, by heating the condenſed or rarified air, ſhut up in the priſm, juſt before it is fixed to the teleſcope, and by obſerving whether the hair in its focus will continue to cover the ſame mark all the while that the air is cooling.†

The French Academicians, being informed of the reſult of the above mentioned experiment, employed M. Deliſle, the younger, to repeat their former experiment with more care; and he preſently found that their operators had never made any vacuum at all, there being chinks in their inſtrument, through which the air had inſinuated itſelf. He therefore annexed a gage to his inſtrument, by which means he was ſure of his vacuum ; and then the reſult of the experiment was the ſame with that in England. The refraction was always in proportion to the denſity of the air, excepting when the mercury was very low, and conſequently the air very rare; in which caſe the whole quantity being very ſmall, he could not perceive much difference in them. Comparing, however, the refractive power of the atmoſphere, obſerved at Paris, with the reſult of his experiment, he found that the beſt vacuum he could make was far ſhort of that of the etherial regions above the atmoſphere.[*]

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* Hauksbee's Experiments, p. 225. † Smith's Opticks. Remarks, p. 69.

Mr. Haukſbee took a great deal of pains with the buſineſs of refractions, carefully aſcertaining the refractive powers of different fluids, by means of a priſm; and as his obſervations are of conſiderable importance, I ſhall ſubjoin the table that he drew up from them, and recite ſome of his general remarks.

	Specifick Gravities in compariſon with a bulk of water equal to 820 grains.	Ang. of Obſervation.	Ratio of Refraction.
		d. ′	as 10000 is to
		16.50	7485.3
Oil of *Saſafras*	898	29.20	6475.8
Turpentine	713.5	25.25	6741.8
Bees-Wax	662	23.30	6885.4
Carrawayes	752	26.13	6696.5
Oranges	711	25.20	6741.2
Hyſop	769.5	25.10	6757.6
Roſemary	747	24.40	6794.7
Savin	789	25.30	6730.9
Origanum	752	25.00	6770.2
Pennyroyal	783	25.30	6730.9
Mint	780.5	26.00	6706.4
Spike	749	24.30	6807.3

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[*] Ac. Par. 1719. H. 88. M. 436.

Fennel

Fennel	798	27.10		6616.5
Juniper	729	25.10		6757.6
Cummin	766.5	27.00		6627.7
Tanſey	757	23.46		6865.1
Dill	795.5	27.40		6582.7
Amber	783	26.30		6662.3
Cinnamon	828	28.40		6517.7
Cloves	827	27.20		6606.8
Nutmegs	759	25.40		6721.4
Spirit of *Wine*	703.5	18.50		7287.9
Hartſhorn	786	17.00		7468.3
Vinegar	824.5	17.00	*As Hartſhorn.*	
Sal Ammoniack	794.5	16.56		7475.2
Acids, Spirit of *Amber*	825	16.56	*As Sal Ammoniack*	
Oil of *Vitriol*	1580	21.56		7011.5
Spirit of *Nitre*	1166	20.50		7104.
Aqua Regis	987	19.50		7195.
Aqua Fortis	1157	20.40		7120.5
Aqua Regis from *Aqua Fortis* and *Sal Ammoniack*	1034	20.10		7161.5
Butter of *Antimony*	1976	40.00		5941.3
Spirit of *Raw Silk*	916	20.30		7135
Spirit of *Honey*	716	16.50	*As Water.*	
Tinct. of *Antimony*	693	18.46		7294.3
Jeſuits Bark	720	18.46	*As Tincture of Antimony.*	
Balſamum Tolu	717	19.34		7219.3
Gum Ammoniacum	719	19.10		7257.3
Metals	713	18.54		7281.7
Vitreous Humour of an Ox's Eye		16.50	*As Water*	
Cryſtalline Humour of an Ox's Eye		24.10		6832.7
White of an Hen's Egg		17.40		7401.3
Jelly of Hartſhorn		17.50		7384.7

Human

Human Saliva	16.50	*As Water.*
Human Urine	17.05	7451.9
French Brandy	18.20	7338.6*

In order to meaſure the refractive power of the cryſtalline humour of the ox's eye, mentioned in the above table, he preſſed it into the angle of the priſm. It is remarkable, that he could not ſee the common object through it, but was forced to make uſe of a candle for that purpoſe, the flame of which appeared very broad, at leaſt 5 or 6 inches, nearly in the form of an half moon, the reaſon of which he could not determine.

Of all the fluids which he tried, he found nothing to refract the light leſs than water. He obſerved oil of bees-wax to be the lighteſt fluid, and butter of antimony per deliquium to be much the heavieſt; as their ſpecific gravity is nearly one to three; but their refractive power as about 1 to 1. 16.

Upon the whole, Mr. Haukſbee obſerved, that bodies do not refract the light in proportion to their ſpecific gravity, but from ſome quality peculiar to themſelves; but whether it was from their inflammability, or ſome peculiarity of their internal ſtructure he could not tell.

Oil of turpentine, ſtrongly tinged green with filings of braſs, did not at all alter its refractive power. §

Dr. Hooke firſt ſuggeſted the thought of making allowance for the effect of the refraction of light, in paſſing from the higher and rarer, to the lower and denſer region of the atmoſphere, in the computed height of mountains. To this he aſcribes the different opinions of authors concerning the height of ſeveof ſeveral very high hills. He could not account for the appearance of the Pike of Tenerif, and ſeveral very high mountains, at ſo great a diſtance as that at which they are actually ſeen, but upon the ſuppoſition of the curvature of the viſual ray, that is made by its paſſing obliquely through a medium of ſuch different denſity, from the top of them to the eye, very far diſtant in the horizon. All calculations of the height of mountains that are made upon the ſuppoſition that the rays of light come from the tops of them, to our eyes, in ſtraight lines muſt, he ſays, be very erroneous.*

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* A conſiderable part of this table will be repeated, and reduced into a more commodious form, along with ſome of Mr. Euler's, in the laſt period of this work.

§ Haukſbee's Experiments, p. 293.

Dr. Hooke gives a very good account of the *twinkling of the ſtars*, aſcribing it to the irregular and unequal refraction of the rays of light, which is alſo the reaſon why the limbs of the ſun, moon, and planets appear to wave or dance. And that there is ſuch an unequal diſtribution of the parts of the atmoſphere, he ſays, is manifeſt from the different degrees of heat and cold in the air. This, he ſays, will be evident by looking upon diſtant objects, over a piece of hot glaſs, which cannot be ſuppoſed to throw out any kind of exhalation from itſelf, as well as through aſcending ſteams of water.†

In this period I find the firſt obſervation of a remarkable effect of the different refrangibility of the rays of light, tranſmitted through a priſm, though the cauſe of it was not found before Sir Iſaac Newton. The effect, however, was obſerved by Grimaldi, who made the conſiderable diſcovery relating to the inflexion of light which will be mentioned in the next ſection. He perceived that when a beam of the ſun's light was tranſmitted through a priſm, inſtead of coming out round, for inſtance, as it entered, it made a very conſiderable oblong image of the ſun, on the oppoſite wall.

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* Micrographia, p. 236.

† Ib. p. 231.

This curious obſervation he made in the courſe of ſome experiments, by which he proves that colours are, in ſome caſes, produced by the refraction of light, without any reflexion of it. His firſt experiment to prove this, was that of Vitellio, viz. by making a beam of light in a darkened room fall obliquely on a glaſs veſſel full of water, the bottom of which was covered with ſome white ſubſtance, on which the colours appeared. His ſecond experiment is that of the priſm above mentioned, in which a beam of light is more dilated, by means of the double refraction, one at the entrance of the rays into the priſm, and the other at their coming out of it; and by figures he very well repreſents the progreſs of the rays in this caſe.* But though the light be coloured by an oblique refraction into a piece of glaſs, this author ſhews that, if the two ſurfaces of the glaſs be parallel, the rays will iſſue parallel to their direction before incidence, and be colourleſs.†

Though this writer ſpeaks of part of a beam of light being more refracted than another part of it, he was far from ſuſpecting that it was occaſioned by any difference in the refrangibility of the rays of which it conſiſted. He only imagined that the light was more condenſed on the ſide that was leaſt refracted, or red, and rarer, or more dilated, where it was moſt refracted.§

That priſms, or broken pieces of glaſs, tranſmit the light coloured, had been known as early, perhaps, as almoſt any obſervations concerning light and colours; and we have ſeen that Deſcartes availed himſelf of it in accounting for the colours of the rainbow. The *colorific* property, as I may call it, of priſms, makes them of great value in the Eaſt, as we learn from Father Trigautius, in his account of the miſſion to China; who ſays that five hundred pieces of gold had been given for one of them, by a perſon who got it upon thoſe terms with great difficulty; it being thought to be fit for ſovereign princes only. Kircher gives the ſame account in his China Illuſtrata.*

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* De Lumine, p. 235. † Ib. p. 272. § Ib. p. 272.

But, notwithſtanding the property of priſms, to exhibit colours, had been long known, that the *form of the beam* was altered in the ſame circumſtances was not obſerved before it occurred in the experiments of Grimaldi. This Italian philoſopher, however, and others after him, only imagined that the light was ſome way or other irregularly ſcattered by the parts of the priſm. This, indeed, was ſaying nothing; but it does not appear that he, or any one elſe, thought much about it, not imagining that the fact was of any conſequence. Thus the jewel, in the fable, is paſſed unnoticed by the cock, and lies unheeded; till a jeweller who knows its value, happens to come that way.

I ſhall conclude this ſection, relating to the phenomena of *refraction*, with ſome obſervations concerning the effects of *burning glaſſes*, particularly thoſe that were made by the members of the academy Del Cimento; and I am ſorry to ſay, that it is the laſt of the tranſactions of thoſe gentlemen that I have to relate, upon the ſubject of this part of my work. They were intended to try what kind of ſubſtances are moſt affected with the heat of burning glaſſes; and they proved, contrary to the opinion which had prevailed till their times, that *white* ſubſtances, ſuch as fine writing paper, and fine linen, could be ſet on fire by this means, as alſo many other ſubſtances, which they mention, particularly gunpowder; but they were not able to fire ſpirit of wine.

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* Acta Ac. Cœſ. Vol. 8, p. 223.

This laſt obſervation is a very remarkable one, but was not attended to, till M. Nollet, in the laſt period of my hiſtory, found that he could not, by the moſt powerful burning mirrors, or lenſes, ſet fire to any inflammable liquors whatever.

One cannot help ſmiling, though perhaps very unjuſtly, at ſeveral experiments that are made in the infancy of any ſcience. This was with me the effect of an optical experiment of Dr. Hooke's within this period, and with an account of which I ſhall cloſe this ſection. He related to the royal ſociety, whoſe curator he then was, at one of their meetings in 1664, that having taken a piece of ice, and ſhaped it into a lens, he found that, though it caſt the figure of the ſun upon his hand, it yielded no ſenſible heat. In the infant ſtate of this ſcience, this account was by no means treated with ridicule. He was deſired by the members preſent to try it again, and at the ſame time a lens of glaſs, to obſerve if that would give any heat in the ſame weather. The reſult of the experiment, if it ever was made, is not related. Indeed it might perhaps have been as well if Dr. Birch had not recorded ſo much as he has done of this affair, but ſince he has thought proper to mention it, I alſo have thought proper to do it after him.

We are by no means to judge of Dr. Hooke as a philoſopher, or an optician, from this experiment. Had he, or any of the members of the royal ſociety, been ſufficiently acquainted with the writings of Deſcartes, they would have found it needleſs to try the effects of lenſes made of ice. For this writer ſays, that James Metius, of Alcmar, in Holland, the ſame perſon to whom he aſcribes the invention of teleſcopes, and who took great pleaſure in making lenſes and burning glaſſes, ſometimes made them of *ice*, and did not find that ſubſtance unfit for his purpoſe.*

SECTION IV.

Experiments concerning the INFLECTION of LIGHT.

IN the title of this ſection there occurs a new term in the doctrine of light, correſponding to a new property of the rays, diſcovered within this period; and though, in fact, it ſeems to be nothing more than an effect of the ſame power which is the cauſe of reflexion and refraction, peculiarly circumſtanced, yet it is ſuch an effect as could hardly have been ſuſpected without actual obſervation. Had Deſcartes, or any other philoſopher before this period, been aſked, what would have become of a ray of light, which ſhould paſs as near as poſſible to any body, but without actually impinging upon it; he would have replied, without heſitation, that it would proceed in a ſtraight line, without being affected by its approach. But it was now found, that light coming within a certain diſtance of any body, will either be bent from it, or towards it, which is a kind of imperfect reflexion or refraction. This property was firſt called *diffraction*, but has now generally obtained the name of *inflection*, which is the term adopted by Sir Iſaac Newton, whoſe experiments on this ſubject, though not completed, are peculiarly ſtriking and important.

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* Dioptrics, p. 49.

The firſt who made this diſcovery concerning the diffraction, or inflection of light, was Father Grimaldi. At leaſt, he firſt publiſhed an account of it, in his treatiſe *De lumine, coloribus, et iride,* printed in 1666; for I do not find that any other perſon lays claim to the diſcovery, except Dr. Hooke, who communicated his obſervations on this ſubject to the royal ſociety in 1672, ſome months after the firſt communication of Sir Iſaac Newton's diſcovery, concerning the different refrangibility of the rays of light; and, as it ſhould ſeem, in order to fulfil the promiſe which he thereupon made, to bring ſome objections to Sir Iſaac Newton's doctrine, at the next meeting of the ſociety. It appears, however, that Dr. Hooke had not heard of the diſcoveries of Grimaldi; for he ſpeaks of his own, as the diſcovery of a "new "property of light not mentioned by any optical writer before "him." * Indeed, the circumſtances of Dr. Hooke's experiments are ſo different from thoſe of Grimaldi, that his account is in itſelf extremely probable.

Conſidering the obſervations of Dr. Hooke, as being made independent of thoſe of Grimaldi, and perhaps about the ſame time, and being far leſs perfect than his, I ſhall begin with them, as proper to introduce the more accurate and important obſervations of the Italian philoſopher; which, indeed, do well deſerve to be recited at ſome length, no writer that I have met with having done them juſtice.

Dr. Hooke, having made his room completely dark, he admitted into it a beam of the ſun's light, by a very ſmall hole in a braſs plate, fixed in the window ſhutter. This beam, ſpreading itſelf, formed a cone, the apex of which was in the hole, and the baſe was on a paper, ſo placed, as to receive it at ſome diſtance. In this image of the ſun, thus painted on the paper, he obſerv- that the middle was much brighter than the edges, and that there was a kind of dark penumbra about it, of about a 16th part of the diameter of the circle; which penumbra, he ſays, muſt be aſcribed to a property of light, which he promiſed to explain. Having obſerved this, at about the diſtance of two inches from the former, he let in another cone of light; and receiving the baſes of them, at ſuch a diſtance from the holes, as that the circles interſected each other, he obſerved that there was not only a penumbra, or darker ring, encompaſſing the lighter circle, but a manifeſt dark line, or circle, which appeared even where the limb of the one interfered with that of the other, This appearance is diſtinctly repreſented, fig. 52.

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* Birch's Hiſtory, Vol. 3, p. 10.

Comparing the diameter of this baſe, with its diſtance from the hole, he found it to be by no means the ſame as it would have been, if it had been formed by ſtraight lines, drawn from the extremities of the ſun's diſk, but varied with the ſize of the holes, and the diſtance of the paper.

Struck with this appearance, he proceeded to make farther experiments concerning the nature of light thus tranſmitted. To give a juſt idea of which, he held an opaque body, BB, fig. 53, ſo as to intercept the light that entered at a hole in the window-ſhutter O, and was received on the ſcreen AP. In theſe circumſtances, he obſerved, that the ſhadow of the opaque body (which was a round piece of wood, not bright or poliſhed) was all over ſomewhat enlightened; but more eſpecially towards the edge. Some perſons, who were preſent, imagining that this light within the ſhadow might be produced by ſome kind of reflexion from the ſide of this opaque body, on account of its roundneſs; and others ſuppoſing it might proceed from ſome reflexion from the ſides of the hole, in the piece of braſs through which the light was admitted into the room; to obviate both theſe objections, he admitted the light through a hole burnt in a piece of paſteboard, and intercepted it with a raſor, which had a very ſharp edge; but ſtill the appearances were the very ſame as before; ſo that, upon the whole, he concluded that they were occaſioned by a new property of light, different from any that had been obſerved by preceding writers.

He farther diverſified this experiment, by placing the raſor ſo as to divide the cone of light into two parts, the hole in the ſhutter remaining as before, and placing the paper ſo as that none of the enlightened part of the circle fell upon it, but only the ſhadow of the raſor; and, to his great ſurpriſe, he obſerved what he calls a *very briſk and viſible radiation*, ſtriking down upon the paper, of the ſame breadth with the diameter of the lucid circle; and this radiation always ſtruck perpendicularly from the line of ſhadow, and, like the tail of a comet, extended more than 10 times, and probably more than 100 times the breadth of the remaining part of the circle; nay, as far as he could find, by many trials, the light from the edge ſtruck downwards into the ſhadow very near to a quadrant, though the greater were the deflexions of this new light from the direct radiations of the cone, the more faint they were.

Obſerving this appearance with more attention, he found wherever there was a part of the interpoſed body higher than the reſt, that, oppoſite to it, the radiation of light into the ſhadow was brighter, as in the figure; and wherever there was a notch or gap in it, there would be a dark ſtroke in the half enlightened ſhadow. From all theſe appearances he concluded, that they were to be aſcribed to a new property of light, whereby it is deflected from ſtraight lines, contrary to what had been before aſſerted by optical writers.

It does not appear, however, that our philoſopher ever proſecuted this experiment to any purpoſe; as all that we find of his on the ſubject of light, after this time, are ſome crude thoughts, which he read at a meeting of the royal ſociety, on the 18th of March 1675, which however, as they are only ſhort hints, I ſhall copy.

They conſiſt of eight articles, and, as he thought, contained an account of ſeveral properties of light, that had not been noticed before. 1. There is a *deflection* of light, differing both from reflexion and refraction, and ſeeming to depend on the unequal denſity of the conſtituent parts of the ray, whereby the light is diſperſed from the place of condenſation, and rarified, or gradually diverged into a quadrant. 2. This deflection is made towards the ſuperficies of the opaque body perpendicularly. 3. Thoſe parts of the diverged radiations which are deflected by the greateſt angle from the ſtraight or direct radiations are the fainteſt, and thoſe that are deflected by the leaſt angles are the ſtrongeſt. 4. Rays cutting each other in one common foramen do not make the angles at the vertex equal. 5. Colours may be made without refraction. 6. The diameter of the ſun cannot be truly taken with common ſights. 7. The ſame rays of light, falling upon the ſame point of an object, will turn into all ſorts of colours, by the various inclination of the object. 8. Colours begin to appear when two pulſes of light are blended ed ſo well, and ſo near together, that the ſenſe takes them for one. *

After this view of the ingenious, but imperfect experiments of Dr. Hooke, I proceed to relate the more valuable diſcoveries of Father Grimaldi. Having introduced a ray of light, through a very ſmall hole, AB, fig. 54, into a darkened room, he obſerved that the light was diffuſed into the form of a cone, the baſe of which was CD; and that if any opaque body, FE, was placed in this cone of light, at a conſiderable diſtance from the hole, and the ſhadow was received upon a piece of white paper, the boundaries of it were not confined within GH, or the penumbra IL, occaſioned by the light proceeding from different parts of the aperture, and of the diſk of the ſun, but extended to MN, at which he was very much ſurpriſed, ſuſpecting, and finding by calculation, that it was conſiderably broader than it could have been made by rays paſſing in right lines by the edges of the object.

But the moſt remarkable circumſtance in this appearance was that, upon the lucid part of the baſe, CM and ND, ſtreaks of coloured light were plainly diſtinguiſhed, each being terminated by blue on the ſide next to the ſhadow, and by red on the other; and though theſe coloured ſtreaks depended, in ſome meaſure, on the ſize of the aperture AB, becauſe they could not be made to appear if it was large, yet he found that they were not limited either by it, or by the diameter of the ſun's diſk.

He farther obſerved, that theſe coloured ſtreaks were not all of the ſame breath, but grew narrower as they receded from the ſhadow, and were each of them broader, the farther the ſhadow

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* Hooke's Poſthumous Works, p. 190.

was received from the opaque body, and alſo the more obliquely the paper on which they were received was held with reſpect to it. He never obſerved more than three of theſe ſtreaks.

To give a clearer idea of theſe coloured ſtreakes, he drew the repreſentation of them, exhibited in fig. 55, in which NMO repreſents the broadeſt and moſt luminous ſtreak, next to the dark ſhadow X. In the ſpace in which M is placed there was no diſtinction of colour, but the ſpace NN was blue, and the ſpace OO, on the other ſide of it, was red. The ſecond ſtreak, QPR, was narrower than the former; and of the three parts of which it conſiſted, the ſpace P had no particular colour, but QQ was a faint blue, and RR a faint red. The third ſtreak, TSV, was exactly ſimilar to the two others, but narrower than either of them, and the colours ſtill fainter. *

Theſe coloured ſtreaks he obſerved to lie parallel to the ſhadow of the opaque body; but when it was of an angular form, they did not make the ſame acute angles, but were bent into a curve, the outermoſt being rounder than thoſe that were next the ſhadow, as is repreſented fig. 56. If it was an inward angle, as DCH, the coloured ſtreaks, parallel to each of the two ſides, croſſed without obliterating one another; only the colours were thereby rendered either more intenſe, or mixed.

The light that formed theſe coloured ſtreaks, my reader will perceive, muſt have been bent *from* the body; but this attentive obſerver has likewiſe given an account of other appearances, which muſt have been produced by the light bending *towards* the body. For within the ſhadow itſelf he ſometimes perceived coloured ſtreaks, ſimilar to thoſe above mentioned on the outſide of the

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[*] Grimaldi de Lumine, p. 2.

ſhadow. Sometimes he ſaw more of them, and ſometimes fewer; but for this purpoſe a very ſtrong light was requiſite, and the opaque body was obliged to be long, and of a moderate breadth; which, he ſays, is eaſily found by experience. A hair, for inſtance, or a fine needle, did not anſwer ſo well as a thin and narrow plate; and the ſtreaks were moſt diſtinguiſhable when the ſhadow was taken at the greateſt diſtance; but then the light grew fainter in the ſame proportion.

The number of theſe ſtreaks within the ſhadow was greater in proportion to the breadth of the plate. They were, at leaſt, two, and ſometimes four, if a thicker rod were made uſe of. But, with the ſame plate or rod, more or fewer ſtreaks appeared, in proportion to the diſtance at which the ſhadow was received; but they were broader when they were few, and narrower when there were more of them; and they were all much more diſtinct when the paper was held obliquely.

Theſe coloured ſtreaks within the ſhadow, like thoſe on the outſide of it, were bent in an arch, round the acute angles of the ſhadow, as they are repreſented, fig. 57. At this angle alſo, as at D, other ſhorter lucid ſtreaks were viſible, bent in the form of a plume, as they are drawn betwixt D and C, each bending round and meeting again in D. Theſe angular ſtreaks appeared, though the plate or rod was not wholly immerſed in the beam of light, but the angle of it only; and there were more or fewer in number, in proportion to the breadth of the rod or plate. If the plate or rod was very thin, the coloured ſtreaks within the ſhadow might be ſeen to bend round from the oppoſite ſides, and meet one another, as at B. A only repreſents a ſection of the figure, and not a proper termination of the ſhadow, and the ſtreaks within each ſide of it. The coloured ſtreaks without the ſhadow, he alſo obſerves, bend round it, in the ſame manner.

Our author acknowledges, that he omits ſeveral obſervations of leſs conſequence, which cannot but occur to any perſon who ſhall make the experiment; and he ſays, that he was not able to give a perfectly clear idea of what he has attempted to deſcribe, nor does he think it in the power of words to do it.

In order to obtain the more ſatisfactory proof that rays of light do not always proceed in ſtraight lines, but really bend, in paſſing by the edges of bodies, he diverſified the firſt of the above mentioned experiments in the following manner. He admitted a beam of light, by a very ſmall aperture, into a darkened room, as before; and at a great diſtance from it, he fixed a plate EF, fig. 58, with a ſmall aperture, GH, which admitted only a part of the beam of light, and found, that when the light tranſmitted through this plate, was received at ſome diſtance, upon a white paper, the baſe IK was conſiderably larger than it could poſſibily have been made by rays iſſuing in right lines through the two apertures, as the other ſtraight lines drawn cloſe to their edges plainly demonſtrate. *

That thoſe who chuſe to repeat theſe experiments may not be diſappointed in their expectations from them, our author gives the following more particular inſtructions. The ſun's light muſt be very intenſe, and the apertures through which it is tranſmitted very narrow, particularly the firſt, CD, and the white paper, IK, on which it is received, muſt be at a conſiderable diſtance from the hole GH; otherwiſe it will not much exceed

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* Grimaldi de Lumine, p. 9.

NO, which would be the breadth of the beam of light proceeding in ſtraight lines. He generally made the aperture CD $\frac{4}{300}$ or $\frac{5}{300}$ parts of an antient Roman foot, and the ſecond aperture, GH, $\frac{25}{300}$ or $\frac{30}{300}$; and the diſtances DG and GN were, at leaſt, 12 ſuch feet. The obſervation was made in the ſummer time, when the atmoſphere was free from all vapours, and about mid-day. *

F. Grimaldi alſo made the ſame experiment that has been recited from Dr. Hooke, in which two beams of light, entering a darkened room by two ſmall apertures near to one another, projected cones of light; which, at a certain diſtance, in part coincided; and he particularly obſerved that the dark boundaries of each of them were viſible within the lucid ground of the other. †

To theſe diſcoveries of Grimaldi, I ſhall ſubjoin an additional obſervation of Dechales; who took notice, that if ſmall ſcratches be made in any piece of poliſhed metal, and it be expoſed to the beams of the ſun, in a darkened room, it will reflect the rays ſtreaked with colours, in the direction of the ſcratches; as will appear if the reflected light be received upon a piece of white paper. § That theſe colours are not produced by refraction, he ſays, is manifeſt; for that, if the ſcratches be made upon glaſs, the effect will be the ſame; and in this caſe, if the light had been refracted at the ſurface of the glaſs, it would have been tranſmitted through it. ‡ From theſe, and many other obſervations, he concludes that colour does not depend upon the refraction of light only, nor upon a variety of other circumſtances, which he particularly enumerates, and the effects of which he diſcuſſes, but upon the intenſity of the light only. ||

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* Grimaldi de Lumine, p. 10. † Ib. p. 187. § Curſus, Vol. 3, p. 726. ‡ Ib. p. 738. || Ib. p. 739.

The very imperfect views and concluſions of the philoſophers of this period exhibit an amuſing and inſtructive proſpect; as they demonſtrate that it is by no means neceſſary to have juſt views, and a true hypotheſis, *a priori*, in order to make real diſcoveries. Very lame and imperfect theories are ſufficient to ſuggeſt uſeful experiments, which ſerve to correct thoſe theories, and give birth to others more perfect. Theſe, then, occaſion farther experiments, which bring us ſtill nearer to the truth; and in this method of *approximation*, we muſt be content to proceed, and we ought to think ourſelves happy, if, in this ſlow method, we make any real progreſs. As an hiſtorian, I am particularly pleaſed, that the philoſophers of the period of which I am now treating, publiſhed their works before their theories were exploded. Had the caſe been otherwiſe, we ſhould probably never have heard of them; whereas, at preſent, we are better able to enter into their views, and to obſerve the connection between their hypotheſes and their experiments.

The profeſſed object of Father Grimaldi's whole book, is to determine the great queſtion of thoſe times, viz. whether light be a ſubſtance, or a quality; and after diſcuſſing it very largely, in a cloſe printed quarto, conſiſting of 535 pages, he concludes, with the Ariſtotelians, that light is no real ſubſtance, but only a mode or property of bodies; or, rather in his own words, it is not a *ſubſtantial*, but an *accidental quality*. But it is not my buſineſs to note the miſtakes of great men, but to record their uſeful labours.

I ſhall cloſe this ſection with an account of a phenomenon of viſion obſerved by M. De la Hire, becauſe the ſubject of this ſection, viz. the *inflection of light*, ſeems to ſupply the true ſolution of it, though the author himſelf thought otherwiſe. It is obſervable, he ſays, that when we look at a candle, or any luminous body, with our eyes nearly ſhut, rays of light are extended from it, in ſeveral directions, to a conſiderable diſtance, like the tails of comets. This appearance exerciſed the ſagacity of Deſcartes, and Rohault, as well as of our author; but all three ſeem to have been miſtaken with reſpect to it. Deſcartes aſcribed this effect to certain wrinkles in the ſurfaces of the humours of the eye. Rohault ſays that, when the eye-lids are nearly cloſed, the edges of them act like convex lenſes; but our author ſays, that the moiſture on the ſurface of the eye, adhering partly to the eye itſelf, and partly to the edge of the eye lid, makes a concave mirror, and ſo diſperſes the rays at their entrance into the eye. But the true reaſon ſeems to be, that the light, paſſing among the eye laſhes, in this ſituation of the eye, is inflected by its near approach to them, and therefore enters the eye in a great variety of directions. The two former of theſe opinions are particularly ſtated and objected to by our author. *

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* Grimaldi de Lumine, p. 392.

SECTION V.

Observations and diſcoveries relating to VISION.

THE obſervations and diſcoveries concerning viſion in this period are ſo numerous, and of ſuch various kinds, that I find it convenient to divide my account of them into ſeveral chapters. The number and variety of articles relating to this ſubject is the more extraordinary, as they are almoſt all ſupplied by one country, viz. France, which, in this period, produced ſeveral ingenious perſons, who made the ſubject of viſion their ſtudy, particularly M. De la Hire, M. Marriotte, and M. Petit.

CHAPTER I.

Discoveries relating to the STRUCTURE OF THE EYE.

NO perſon ever gave more attention to the anatomy of the eye than M. Petit, and as ſeveral of his obſervations and diſcoveries are of importance to the theory of viſion, I ſhall begin this ſection with the recital of them.

It had long been obſerved that the *choroides* in men is black, but no obſervations had been made on the change it ſuffers by age before M. Petit, who perceived that it appears quite brown under the retina in children, and grows conſiderably brighter as they advance in age. *

All anatomiſts after Galen had thought that the *uvea* in men was convex, except Veſalius; but our author, by a variety of accurate obſervations, concludes that it is perfectly flat. †

M. Petit's obſervations on the *cryſtalline humour*, both in men and other animals, are very numerous, and particularly valuable. In ſerpents and fiſhes he obſerved that the cryſtalline is nearly ſpherical, whereas in all other animals that he had examined it was lenticular, the anterior ſurface being leſs convex than the poſterior. §

In all animals, he obſerved that the cryſtalline grows firmer with age. In new born children it reſembles cold jelly. At 15 or 20 it has an uniform firmneſs of texture, but though it increaſes continually after that period of life, it becomes harder at the center than at the circumference. The cryſtalline in men is not ſo hard a ſubſtance as it is in birds, quadrupeds, and fiſhes, in which it is harder in the order in which they are here mentioned. In fiſhes the interior part is almoſt as hard as horn, but the exterior part is ſofter than it is in any other animals, being nothing more than a mucilage.

The reaſon why the cryſtalline is hardeſt in the center is well explained by Dr. Porterfield, in a later period. It is certain, ſays he, that the rays of light which fall upon the extremities of the cryſtalline, by reaſon of their greater obliquity, muſt be more refracted than thoſe which fall upon its middle, near its axis. By this means they will be made to meet at different diſtances behind the cryſtalline humour; thoſe towards its extremity at a nearer, and thoſe near its axis at a greater diſtance; ſo that it is impoſſible for all of them to be united exactly upon the retina, for rendering the ſight diſtinct; and therefore, to prevent this inconvenience, the center of the cryſtalline is more denſe and ſolid; that the rays of light which fall near its axis may, in paſſing this nucleus, have their refraction increaſed, and by that means may be made to converge, and meet at the ſame point with thoſe that paſs the cryſtalline towards its edge.

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* Ac. Par. 1726, H. p. 32.

† Ib. 1728, H. p. 22.

§ Ib. 1730, H. p. 45.

This is the reaſon why the cryſtalline in all animals is more ſolid in its center than externally, and why in fiſhes this difference is ſo remarkable. For in them, this humour being ſpherical, the rays that fall upon it at ſome diſtance from its axis, would be made to meet, by reaſon of their great obliquity, at a greater diſtance from the point of union of the other rays that paſs near its center, than in land animals, which have this humour lenticular; and therefore, to prevent this inconvenience, which would have rendered the ſight exceedingly indiſtinct, nature has provided them with that ſmall ſolid cryſtalline, in the center of the other, whoſe denſity far exceeds that of the nucleus of land animals.*

It had been known that the cryſtalline in men loſes of its *convexity* with age, but M. Petit obſerved that it changes its *colour* on the ſame account. From the birth to the age of above 25, he obſerved it to be perfectly tranſparent; but from that time it begins to have a yellowiſh tinge in the center, which grows deeper continually, and extends farther and farther towards the circumference. He had ſeen two cryſtallines of a perſon who was 81 years of age, which reſembled two pieces of beautiful yellow amber. The harder any cryſtalline is, the yellower he obſerved it to be. *

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* Porterfield on the eye, Vol. 1, p. 439.

Leeuwenhoeck diſcovered that when the cryſtalline humour is dry, it appears to be compoſed of many thin concentric laminæ, or ſcales, lying one upon onother, of which he ſuppoſes there may be 2000 in one cryſtalline, from the outermoſt to the center; and every one of theſe ſcales, he ſays, he hath diſcovered to be made up of one ſingle fibre, or fine thread, wound in a moſt ſtupendous manner this way and that way, ſo as to run ſeveral courſes, and meet in as many centers, and yet not to interfere or croſs one another in any one place. In oxen, ſheep, hogs, dogs, and cats, the thread ſpreads in three ſeveral courſes, and makes as many centers; in whales, it has five, but in hares and rabbits only two. In the whole ſurface of an ox's cryſtalline, he reckons they are more than 12000 fibres.†

M. Petit verified what M. Leeuwenhoeck had advanced concerning the internal ſtructure of the cryſtalline, as conſiſting of concentric laminæ. This he found to be the caſe by a careful diſſection, but principally by the ſeparation of theſe laminæ in acid liquors. §

In the courſe of theſe inquiries, particularly in diſſecting the eyes of young animals, he generally found the cryſtalline white and opaque, though there was nothing of that appearance while they were alive; but at length he perceived that one of theſe opaque cryſtallines became tranſparent by the warmth of his hand, and he made it alternately opaque and tranſparent, by cooling or heating it, as long as he pleaſed. ||

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* Ac. Par. 1728. H. p. 47. † Porterfield on the eye, p. 442. § Ac. Par. 1728. H. p. 48. || Ib. p. 16.

M. Petit always found the *capſula* of the cryſtalline tranſparent, even in thoſe perſons who had cataracts; and though the cryſtalline becomes opaque in the ſolutions of ſeveral ſorts of ſalts, the capſula never does. Our author imagined that he had diſcovered the cryſtalline to have no connection with its capſula, but to be perfectly inſulated in the fluid it contains;* but this the more accurate Albinus found to be a miſtake. He diſcovered that the cryſtalline has a connection with its capſula, by means of ſeveral veſſels, which paſſing through ſome ſmall perforations in the capſula, are inſerted at the extremities of it, and ſpread along the back part of it. He obſerves, beſides, that the central artery paſſing through the vitreous humour, is divided into many branches in the back part of the capſula, and ſends ſeveral of them to the interior parts of the cryſtalline. By theſe veſſels it is both ſuſpended, and receives its nouriſhment.†

It is well known that, for ſome time after children are born, they ſee very imperfectly, and M. Petit, after taking a great deal of pains to inveſtigate the cauſe of it, found it to be owing, in part, to the thickneſs of their cornea, and the ſmall quantity of their aqueous humour. Not that the mere thickneſs of the cornea could have this effect; but becauſe that thickneſs is owing to its not being well ſtretched, and conſequently having wrinkles and inequalities on its ſurface, which occaſion an irregular refraction of the light. On the ſame account, alſo, the cornea has not a ſufficient degree of convexity, to bring the pencils of rays to a focus ſoon enough. All theſe defects, he ſhews, are remedied by the increaſe of the aqueous humour.§

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* Ac. Par. 1728. H. p. 50.

† Muſchenbroeck's Introductreo, Vol. 2, p. 754.

§ Ac. Par. 1727. H. p. 14.

M. Petit accounts for this imperfection of ſight in infants, to their eyes being compreſſed by the fluid in which they are immerſed in the womb. He had alſo the curioſity to inquire into this circumſtance reſpecting various new born animals, as dogs, cats, rabbits, calves, and hogs; and he found in all of them, that the cornea was thick and flaccid, and the aqueous humour not ſufficiently copious.

It is poſſible that, beſides the ſmall quantity, and want of tranſparency, which ſome writers alſo mention, in the aqueous humour, viſion in new born infants may be obſtructed by the remains of the *membrana pupillaris*, which is a production of the *uvea*, and cloſes the pupil in the foetus. This membrane was diſcovered by the perſon whom Dr. Hunter calls Dr. F----s S------s, and alſo by M. Wachendorf, Haller, and Albinus.* Haller ſays, that the time when this membrane diſappears is uncertain, but he is confident that it is never found in a full grown foetus.§ An account of Albinus's diſcovery of this membrane may be ſeen in Muſchenbroeck's Introductio, Vol. 2, p. 779.

To the account of theſe attempts to explain the ſtructure of the eye, and the nature of viſion, I muſt add, that M. Huygens firſt propoſed, if he did not actually execute, the conſtruction of an *artificial eye*, to give the cleareſt idea of the principal phenomena of viſion, and eſpecially the cauſe of ſhort-ſightedneſs or the contrary. A figure and deſcription of this eye may be ſeen in his Dioptrics, p. 112.

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* Hunter's medical commentaries, part 1, p. 63.

§ Opuſcula, p. 342.

CHAPTER II.

An account of the controversy concerning the proper SEAT OF VISION.

NO ſubject relating to the theory of viſion was more canvaſſed within this period, or indeed in any period, than that concerning *the proper ſeat, or inſtrument of viſion.* In early times, we have ſeen, that the *cryſtalline* was thought to be the beſt qualified for this office; but this ſubſtance, though ſituated in the middle of the eye, which B. Porta thought to be the proper center of obſervation, had univerſally given place to the better founded pretentions of the *retina*; and for many years its claim to this function, was by all philoſophers ſuppoſed to be indiſputable. Leaſt of all was it imagined that the *choroides;* which is ſituated beyond it, would ever have been thought of for the purpoſe; nor indeed is it probable that it ever would have been conſidered in that light, had it not been for a ſingular experiment of M. Marriotte, who firſt eſpouſed the ſentiment. He was led to form this hypotheſis, by finding that one part of the retina, viz. at the inſertion of the optic nerve, is inſenſible to the impreſſion of light; for when the image of any object falls upon this part of the bottom of either eye, it cannot be perceived if the other be ſhut.

Having often obſerved, in the diſſections of men, as well as of brutes, that the optic nerve is not inſerted exactly oppoſite to the pupil, that is, in the place where the picture of the objects, upon which we look directly, is made; and that in man it is ſomewhat higher, and on the ſide towards the noſe, he had the curioſity to examine into the reaſon of this ſtructure, by throwing the image of an object upon this part of the eye. In order to this, he faſtened on a dark wall, about the height of his eye, a ſmall round paper, to ſerve him for a fixed point of ſight; and he faſtened ſuch another paper on the right hand, at the diſtance of about two feet, but rather lower than the former, that the light iſſuing from it, might ſtrike the optic nerve of his right eye, while his left eye was kept ſhut. He then placed himſelf over againſt the former paper, and drew back by degrees, keeping his right eye fixed, and very ſteady upon it; and when he had retired about 10 feet, he found that the ſecond paper intirely diſappeared.

This, he ſays, could not be imputed to the oblique poſition of the ſecond paper with reſpect to his eye, becauſe he could ſee more remote objects on the ſame ſide. The effect was ſuch, as if the ſecond paper had been conveyed away by ſlight of hand. This experiment he repeated, by varying the diſtances of the papers and of his eye. He alſo made it with his left eye, while the right eye was kept ſhut, the ſecond paper being faſtened on the left ſide of the point of ſight; ſo that, by the ſituation of the parts of the eye, it could not be doubted, but that this defect of viſion is in the optick nerve, where only the choroides is deficient. The ſame experiment ſucceeds in other perſons, though not exactly at the ſame diſtances.*

* Smith's Opticks. Remarks, p. 6. Marriotte's Works, p. 496.

An improvement was afterwards made upon this experiment by M. Picard, who contrived that an object ſhould diſappear when both the eyes were kept open. He faſtened upon a wall a round white paper, an inch or two in diameter; and by the ſide of it he fixed two marks, one on the right hand, and the other on the left, each at about 2 feet diſtance from the paper, and ſomewhat higher. He then placed himſelf directly before the paper, at the diſtance of 9 or 10 feet, and putting the end of his finger over againſt both his eyes, ſo that the left hand mark might be hid from the right eye, and the right hand mark from the left eye. Remaining firm in this poſture, and looking ſteadily, with both eyes, on the end of his finger, the paper which was not at all covered by it would totally diſappear. This, he ſays, is the more ſurpriſing, becauſe, without this particular encounter of the optic nerves, where no viſion is made, the paper will appear double, as is the caſe when the finger is not rightly placed.[*]

M. Marriotte obſerves, that this improvement on his experiment, by M. Picard, is ingenious, but difficult to execute, ſince the eyes muſt be conſiderably ſtrained in looking at any object ſo near to them as 4 inches, and propoſes another not leſs ſurpriſing, and more eaſy. Place, ſays he, on a dark ground, two round pieces of white paper, at the ſame height, and three feet from one another; then place yourſelf oppoſite to them, at the diſtance of 12 or 13 feet, and hold your thumb before your eyes, at the diſtance of about 8 inches, ſo that it may conceal from the right eye the paper that is to the left hand, and from the left eye the paper to the right hand. Then if you look at your thumb ſteadily, with both eyes, you will loſe ſight of both the papers; the eyes being ſo diſpoſed, that each of them receives the image of one of the papers upon the baſe of the optic nerve, while the other is intercepted by the thumb.*

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[*] Marriotte's Works, p. 506.

M. Le Cat purſued this curious experiment a little farther than M. Marriotte had done. In the place of the ſecond paper, he fixed a large white board, and obſerved that, at a proper diſtance, he loſt ſight of a circular ſpace in the center of it. He alſo obſerved the ſize of the paper which is thus concealed from the ſight, correſponding to ſeveral diſtances, which enabled him to aſcertain ſeveral circumſtances relating to this part of the ſtructure of the eye more exactly than had been done before.

Keeping his eye fixed upon the point A. fig. 59, and withdrawing it, in the line AP, he carefully meaſured the ſizes and diſtances of the circles *a*, *b*, *c*, which diſappeared when he was at certain diſtances from the point A; and upon the whole he found, that to make this ſecond paper diſappear, this ſeries of dark circles form a dark cone, BAC, with an angle of near 24 degrees, the upper ſide AB being near 5 degrees below the horizontal line, and the axis AD paſſing through the centers of all theſe circles, being about 7 degrees below it. Theſe obſervations furniſhed him with *data* ſufficient to determine the place of the inſertion of the optic nerve in any particular eye. By this means he found that, in himſelf, it was $\frac{1}{5}$ nearer to the axis of the eye than it was in M. Marriotte. From the ſame data he computed the ſize of this place in which there is no viſion, and found it to be no larger than the head of a ſmall pin, viz. $\frac{1}{3}$ or $\frac{1}{4}$ of a line. §

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* Marriotte's Works, p. 516. § Traité des Sens, p. 171.

The manner in which this curious experiment is now generally made, and which is both the eaſieſt with reſpect to the eye, and the moſt indiſputable with reſpect to the fact, is the following. Let three pieces of paper, A,B, and C, fig. 60, be faſtened upon the ſide of a room, about 2 feet aſunder, and let a perſon place himſelf oppoſite to the middle paper, and, beginning pretty near to it, retire gradually backwards, all the while keeping one of his eyes ſhut, and the other turned obliquely towards that outſide paper which is toward the covered eye, and he will find a ſituation (which is generally at about five times the diſtance at which the papers are placed from one another) where the middle paper will intirely diſappear, while the two outermoſt continue plainly viſible; becauſe, as is evident from the figure, the rays which come from B will fall upon the retina at D, where is the inſertion of the optic nerve.

It will not ſurprize any perſon, even thoſe who are the ſtrongeſt advocates for the retina being the place at which the pencils of rays are terminated, and conſequently the proper ſeat of viſion, that M. Marriotte was led, by this remarkable obſervation, to ſuſpect the contrary. He not only did ſo, but, in conſequence of attentively conſidering the ſubject, a variety of other arguments, in favour of the choroides, occurred to him, particularly his obſervation, that the retina is tranſparent, as well as the cryſtalline, and other humours of the eye, which he thought could only enable it to tranſmit the rays farther; and he could not perſuade himſelf that any ſubſtance could be conſidered as being the termination of the pencils, and the proper *ſeat of viſion,* at which the rays are not ſtopped in their progreſs.

He was farther confirmed in his opinion, of the ſmall degree of ſenſibility in the retina, and of the greater ſenſibility of the choroides, by obſerving that the pupil dilates itſelf in the ſhade, and contracts itſelf in a great light; which involuntary motion, he thought, was a clear proof, that the fibres of the iris are extremely ſenſible to the action of light; and this part of the eye is only a continuation of the choroides.* He alſo thought that the *dark colour* of the choroides was intended to make it more ſuſceptible of the impreſſion of light.

M. Pecquet, in anſwer to M. Marriotte's obſervation concerning the tranſparency of the retina, ſays, that it is very imperfectly ſo, reſembling only oiled paper, or the horn that is uſed for lanterns; and beſides, that its whiteneſs demonſtrates it to be ſufficiently opaque for ſtopping the rays of light, as much as is neceſſary for the purpoſe of viſion; whereas, if viſion be performed by means of thoſe rays which are tranſmitted through ſuch a ſubſtance as the retina, it muſt be very indiſtinct.

As to the blackneſs of the choroides, which M. Marriotte thought to be neceſſary for the purpoſe of viſion, M. Pecquet obſerves, that it is not the ſame in all eyes, and that there are very different ſhades of it among the individuals of mankind, as alſo among birds, and ſome other animals, whoſe choroides is generally black, and that in the eyes of lions, camels, bears, oxen, ſtags, ſheep, dogs, cats, and many other animals, that part of the choroides which is the moſt expoſed to light, very often exhibits colours as vivid as thoſe of mother of pearl, or of the iris.†

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* Smith's Opticks. Remarks, p. 7.

† M. Muſſchenbroeck ſays, that in many animals, as in the lion, camel, bear, ox, ſtag, ſheep, dog, cat, and many birds, the choroides is not black, but blue green, yellow, or ſome other colour. Introductio, Vol. 2, p. 748.

He admits that there is a defect of viſion at the inſertion of the optic nerve, but he thought that it was owing to the blood veſſels of the retina, the trunks of which are ſo large in that place, as to obſtruct all viſion.[*]

To M. Pecquet's objection, founded on the opacity of the retina, M. Marriotte obſerves, that there muſt be a great difference betwixt the ſtate of that ſubſtance in living and dead ſubjects, and as a farther proof of the tranſparency of the retina, and the power of the choroides, beyond it, to reflect light, he ſays that, if a lighted candle be held near to a perſon's eyes, and a dog, at the diſtance of eight or ten ſteps, be made to look at him, he would ſee a bright light in the dog's eyes, which he thought to proceed from the reflection of the light of the candle, from the choroides of the dog, ſince the ſame appearance cannot be produced in the eyes of men, or other animals, whoſe choroides is black.[†]

To M. Pecquet's remark concerning the blood veſſels of the retina, M. Marriotte obſerves, that they are not large enough to prevent viſion in every part of the baſe of the nerve, ſince the diameter of each of the two veſſels occupy no more than $\frac{1}{8}$ part of it.[§] Beſides, if this were the cauſe of this want of viſion, it would vaniſh gradually, and the ſpace to which it is confined, would not be ſo exactly terminated as it appears to be.

I muſt add, that M. Pecquet alſo obſerved; that, notwithſtanding the inſenſibility of the retina at the inſertion of the optic nerve when the light is only moderate; yet that luminous objects, ſuch as a bright candle, placed at the diſtance of 4 or 5 paces, do not abſolutely diſappear, in the ſame circumſtances in

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[*] Marriotte's Works, p. 504. [†] Ib. p. 509. [§] Ib. p. 514.

which a white paper would; for that this ſtrong light may be perceived though the picture fall on the baſe of the nerve. I cannot help ſuſpecting, however, that M. Pecquet did not make this obſervation with ſufficient care. A large candle makes no impreſſion on that part of my eye, though it is by no means able to bear a ſtrong light.

The common opinion was alſo favoured by the anatomical deſcription of ſeveral animals by the members of the French academy, and particularly their account of the ſea-calf, and porcupine; in both of which the optic nerve is inſerted in the very axis of the eye, exactly oppoſite to the pupil, which was thought to leave no room to doubt but that, in theſe animals, the retina is perfectly ſenſible to the impreſſion of light at the inſertion of the nerve. * But this obſervation may deſerve to be reconſidered.

M. D la Hire, by whom nothing relating to viſion could paſs unnoticed, took part with M. Pecquet, arguing in favour of the retina from the analogy of the ſenſes, in all of which the nerves are the proper ſeat of ſenſation. † This philoſopher, however ſuppoſed that the choroides receives the impreſſions of images, in order to tranſmit them to the retina.§

M. Perrault alſo took the part of M. Pecquet againſt M. Marriotte, and in M. Perrault's works we have ſeveral letters that paſſed between theſe two gentlemen upon this ſubject.

This diſpute about the immediate inſtrument of viſion was revived upon the occaſion of an odd experiment of M. Mery, recorded in the memoirs of the French academy for 1704. He

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* Porterfield on the eye, Vol. 2, p. 255.

† Smith's Opticks, Remarks, p. 7.

§ Ac. Par. for 1711, H. p. 102.

plunged a cat in water, and, expoſing her eye to the ſtrong light of the ſun, obſerved that the pupil was not at all contracted by it; from which he concluded, that the contraction of the iris is not produced by the action of the light, but by ſome other circumſtance. For he contended that the eye receives more light in this ſituation than in the open air. At the ſame time he thought he obſerved that the retina of the cat's eye was tranſparent, and that he could ſee the opaque choroides beyond it; from which he concludes, that the choroides is the ſubſtance intended to receive the rays of light, and to be the chief inſtrument of viſion. But M. De la Hire replies to this argument of M. Mery, in a memoir for the year 1709, p. 119; in which he endeavours to ſhew that fewer rays enter the eye under water, and that, in thoſe circumſtances, it is not ſo liable to be affected by them. Beſides, it is obvious to be remarked, that the cat muſt be in great terror in this ſituation; and being an animal that has a very great voluntary power over the muſcles of the iris, and being now extremely attentive to every thing about her, ſhe might keep her eye open, notwithſtanding the action of the light upon it, and though it might be very painful to her. I am informed that, when a cat is placed in a window, through which the ſun is ſhining, and conſequently her iris nearly cloſed, if ſhe hear a ruſtling, like that which is made by a mouſe, on the outſide of the window, ſhe will immediately open her eyes to their greateſt extent, without, in the leaſt, turning her face from the light.

M. Le Cat took part with M. Marriotte in this controverſy, it being peculiarly agreeable to this general hypotheſis, viz. that the pia mater, of which the choroides is a production, and not the nerves themſelves, is the proper inſtrument of ſenſation.[*] He thought that the change which takes place in the eyes of old people (the choroides growing leſs black with age) favoured his hypotheſis, as they do not ſee with that diſtinctneſs with which young perſons do.[||] M. Le Cat ſuppoſed that the retina anſwers a purpoſe ſimilar to that of the ſcarf ſkin, covering the papillæ pyramidales, which are the immediate organ of feeling, or that of the porous membrane which covers the glandulous papillæ of the tongue. The retina, he ſays, receives the impreſſion of light, moderates it, and prepares it for its proper organ, but is not itſelf ſenſible of it.[†]

It muſt be obſerved, that M. Le Cat had diſcovered that the pia mater, after cloſely embracing and conſtringing the optic nerve at its entrance into the eye, divides into two branches, one of which cloſely lines the cornea, and at length is loſt in it, while the ſecond branch makes what is called the choroides, or uvea.[§] He alſo ſhewed that the ſclerotica is an expanſion of the dura mater; and he ſent diſſections of eyes to the royal academy of ſciences in 1739, to prove theſe aſſertions, and ſeveral others which he had advanced in his *Traité des Sens,* that were contrary to the opinions of the celebrated Winſlow.[‡]

To theſe arguments in favour of the choroides, alledged by thoſe gentlemen among whom the ſubject was firſt diſcuſſed, I ſhall beg leave to preſent to my reader ſome others, which eſcaped their notice, but which were ſuggeſted to me by my friend Mr. Michell.

In order that viſion be diſtinct, the pencils of rays, which iſſue from the ſeveral points of any object, muſt be collected either accurately, or, at leaſt, very nearly to correſponding points in the eye, which can only be done upon ſome uniform ſurface. But the retina, being of a conſiderable thickneſs, and the whole of it being uniformly nervous, and, at leaſt, nearly, if not perfectly tranſparent, preſents no particular ſurface; ſo that, in whatever part of it the pencils be ſuppoſed to have their foci, the rays belonging to them will be ſeparated from one another, either before or after they arrive there, and conſequently viſion would be confuſed.

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[*] Traité des Sens, p. 176. [||] Ib. p. 178. [†] Ib. p. 180. [§] Ib. p. 153. [‡] Ac. Par. 1739, H. p. 25.

If we ſuppoſe the ſeat of viſion to be at the nearer ſurface of the retina, and the images of objects be formed by direct rays, a conſiderable degree of confuſion could not but ariſe from the light reflected by the choroides, in thoſe animals in which it is white, or coloured. On the other hand, it would be impoſſible that viſion ſhould be performed at this place by light reflected from the choroides, becauſe, in many animals, it is perfectly black, and reflects no light at all; and yet ſuch animals ſee even more diſtinctly than others. And we cannot but ſuppoſe that, in whatever manner viſion is effected, it is the ſame in the eyes of all animals.

If the ſeat of viſion be at the farther ſurface of the retina, and it be performed by direct rays, a white choroides could be of no uſe; and if it were by reflected rays, a black one could not anſwer the purpoſe.

It is likewiſe an argument in favour of the choroides being the organ of viſion, that it is a ſubſtance which receives a more diſtinct impreſſion from the rays of light than any other membrane in any part of the animal ſyſtem, excepting (and perhaps not excepting) that white cuticle, which lies under the ſcales of fiſhes;

whereas the retina is a ſubſtance on which the light makes an exceedingly faint impreſſion, and perhaps no impreſſion at all; ſince light, in paſſing out of one tranſparent medium into another immediately contiguous to it, ſuffers no refraction or reflexion, nor are any of the rays abſorbed, unleſs there is ſome difference in the refracting power of the two media, which probably is not the caſe between the retina and the vitreous humour, which is in contact with it.* And wherever the light is not affected by the medium it falls upon, we can hardly ſuppoſe the medium to receive any impreſſion from the light, the action being probably always mutual and reciprocal.

Beſides, the retina is ſo ſituated, as to be expoſed to many rays beſides thoſe which terminate in it, and which, therefore, cannot be ſubſervient to viſion, if it be performed there. Now this is not the caſe with the choroides, which is in no caſe tranſparent, and has no reflecting ſubſtance beyond it.

It is, moreover, peculiarly favourable to the hypotheſis of the ſeat of viſion being in the choroides, that we can then ſee a ſufficient reaſon for the diverſity of its colour in different animals, according as they are circumſtanced with reſpect to viſion. In all terreſtrial animals, which have occaſion to make uſe of their eyes by night, the choroides is either of a bright white, or of ſome very vivid colour, which reflects the light very ſtrongly. On this account viſion may be performed with leſs light, but it cannot be with great diſtinctneſs, the reflection of the rays doubling their effect; ſince it muſt extend over ſome ſpace, all reflexion being made at a diſtance from the reflecting body. Beſides, the choroides in brutes is not in general perfectly white, but a little inclined to blue, and is therefore, probably, better adapted to ſee by the fainter coloured light, which chiefly prevails in the night; and, I would add, is, on the ſame account, more liable to be ſtrongly impreſſed by the colours to which they are chiefly expoſed.

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* This reaſoning is illuſtrated and enforced by an obſervation of Mr. Melville, of which an account will be given in the laſt period of this hiſtory, viz. that there is neither heat, nor any other ſenſible effect, at the focus of the ſtrongeſt burning mirror, in air, water, or any perfectly tranſparent medium.

On the other hand the choroides of birds in general, eſpecially eagles, hawks, and other birds of prey, is black; by which means they are able to ſee with the greateſt diſtinctneſs, but only in bright day light. The owl, however, ſeeking her food by night, has, if I miſtake, not the choroides white, like that of a cat. Laſtly, in the eyes of man, which are adapted to various uſes, the choroides is neither ſo black as that of birds, nor ſo white as that of thoſe animals who make the greateſt uſe of their eyes in the night.

As to a third hypotheſis, which is, in effect, that of M. De la Hire, which makes, both the retina and the choroides equally neceſſary to viſion, and ſuppoſe it be performed by the impreſſion of light on the choroides communicated to the retina; Mr. Michell obſerves, that the perceptions can hardly be ſuppoſed to be ſo acute, when the nerves which are the chief inſtruments of ſenſation, do not receive the impreſſions immediately, but only after they have been communicated to another ſubſtance. Beſides it muſt be more natural to ſuppoſe that, when the principal impreſſion is made upon the chorides, it is communicated to the brain by its own proper nerves, which are abundantly ſufficient for the purpoſe.

It is of little conſequence to the reader how the mind of an hiſtorian is affected by the arguments, which it is his buſineſs to lay before him; but I muſt own that, after having retained my prejudice in favour of the retina, notwithſtanding all that was advanced by M. Marriotte, and the advocates for his opinion among his countrymen, the arguments of Mr. Michell in favour of the choroides, have more weight with me than I was at firſt either able to perceive, or willing to acknowledge, in thoſe of the French philoſophers.

CHAPTER III.

MISCELLANEOUS OBSERVATIONS CONCERNING VISION.

IT had been taken for granted by all philoſophers, that the place to which the eye refers any viſible object, ſeen by reflexion or refraction, is that in which the viſual ray meets a perpendicular from the object upon the reflecting or the refracting plane. That this is the caſe with reſpect to plane mirrors, is univerſally acknowledged, and ſome experiments with mirrors of other forms ſeem to favour the ſame concluſion, and thereby give room to extend the analogy to all caſes of viſion. If a right line be held perpendicularly over a convex or concave mirror, its image ſeems to make one line with it. The ſame is the caſe with a right line held perpendicularly within water; for the part which is within the water ſeems to be a continuation of that which is without, at leaſt when it is viewed with no more than common attention, and in ſome poſitions. But Dr. Barrow, called in queſtion this method of judging of the place of an object, and thereby opened a new field of inquiry and debate in this branch of ſcience. This, and other optical inveſtigations, he publiſhed in his *Optical lectures*, that were firſt printed in the year 1674.

With reſpect to the reflected image of a perpendicular right line from a convex or a concave ſurface, he ſays, it is not eaſy for the eye to diſtinguiſh the curve that it really makes; and that, if the appearance of a perpendicular thread, part of which is plunged in water, be cloſely attended to, it will not favour the common hypotheſis. If the thread be of any ſhining metal, as ſilver, and it be viewed obliquely, he ſays that the image of the part immerſed, will appear ſenſibly to detach itſelf from that part which is without the water; ſo that it cannot be true, that every object appears to be in the place where the refracted ray meets the perpendicular; and the ſame obſervation he thinks may be extended to the caſe of reflexion.

Having, as he imagined, refuted the common hypotheſis concerning the ſuppoſed place of viſible objects, he ſubſtitutes another rule, by which, he ſays, our judgments are actually directed in this caſe. According to this writer, we refer every point of an object to the place from which the pencils of light, that give us the image of it, iſſue, or from which they would have iſſued, if no reflecting or refracting ſubſtance intervened. Purſuing this principle, our excellent geometrician proceeds to inveſtigate the place, in which the rays iſſuing from each of the points of an object, and which reach the eye after one reflexion or refraction, meet; and he found that, if the refracting ſurface was plane, and the refraction was made from a denſer medium into a rarer, thoſe rays would always meet in a place between the eye and a perpendicular to the point of incidence. If a convex mirror be uſed, the caſe will be the ſame; but if the mirror be plane, the rays will meet in the perpendicular, and beyond it if it be concave. He alſo determined, according to theſe principles, what form the image of a right line will take, when it is preſented in different manners to a ſpherical mirror, or when it is ſeen through a refracting medium. But to recite his concluſions, though curious and elegant, would carry us too far into the buſineſs of geometry. We ſee by theſe inveſtigations, how near this geometrician came to the diſcovery of *cauſtic curves*, of which an account will be given hereafter, and which, I ſhall juſt obſerve at preſent, are no other than the *locus* of all thoſe images of the ſame point, that are ſeen by reflexion or refraction from all the different places where an eye can be ſituated. It is probable that the conſideration of what Barrow had done, enabled Tſchirnhauſen to make the diſcoveries that he did in this way.

Probable, as Dr. Barrow thought the maxim which he endeavoured to eſtabliſh, concerning the ſuppoſed place of viſible objects, he has the candour to mention an objection to it, and to acknowledge that he was not able to give a ſatisfactory ſolution of it. It is this. Let an object be placed beyond the focus of a convex lens, and if the eye be cloſe to the lens, it will appear confuſed, but very near to its true place. If the eye be a little withdrawn, the confuſion will increaſe, and the object will ſeem to come nearer; and when the eye is very near the focus, the confuſion will be exceedingly great, and the object will ſeem to be cloſe to the eye. But in this experiment the eye receives no rays but thoſe that are converging, and the point from which they iſſue is ſo far from being nearer than the object, that it is beyond it; notwithſtanding which, the object is conceived to be much nearer than it is, though no very diſtinct idea can be formed of its preciſe diſtance. I would obſerve that, in reality, the rays falling upon the eye, in this caſe, in a manner quite different from that in which they fall upon it in other circumſtances, we can form no judgment about the place from which they iſſue. This ſubject was afterwards taken up by Berkley, Smith, Montucla, and others; but as the recital of their remarks would carry me too far beyond the bounds of this period, I ſhall leave the matter in ſuſpence, till I can introduce them without ſo great a violation of hiſtorial order.*

M. De la Hire made ſeveral valuable obſervations concerning the diſtance of viſible objects, and various other phenomena of viſion, which are well worth our notice. He alſo took particular pains to aſcertain the manner in which the eye conforms itſelf to the view of objects placed at different diſtances, but as he did not hit upon the right hypotheſis, I ſhall reſerve his obſervations on this ſubject for the laſt period of this hiſtory, in which this buſineſs was much better handled.

He enumerates five circumſtances, which aſſiſt us in judging of the diſtance of objects, namely their apparent magnitude, the ſtrength of the colouring, the direction of the two eyes, the parallax of the objects, and the diſtinctneſs of their ſmall parts. Painters, he ſays, can only take advantage of the two firſt mentioned circumſtances, and therefore pictures can never perfectly deceive the eye; but in the decorations of theatres, they, in ſome meaſure, make uſe of them all. The ſize of objects, and the ſtrength of their colouring are diminiſhed in proportion to the diſtance at which they are intended to appear. Parts of the ſame object which are to appear at different diſtances, as columns in an order of architecture, are drawn upon different planes, a little removed from one another, that the two eyes may be obliged to change their direction, in order to diſtinguiſh the parts of the nearer plane from thoſe of the more remote. The ſmall diſtance of the planes ſerves to make a ſmall parallax, by changing the poſition of the eye; and as we do not preſerve a diſtinct idea of the quantity of parallax, correſponding to the different diſtances of objects, it is ſufficient that we perceive there is a parallax, to be convinced that theſe planes are diſtant from one another, without determining what that diſtance is; and as to the laſt circumſtance, viz. the diſtinctneſs of the ſmall parts of objects, it is of no uſe in diſcovering the deception, on account of the falſe light that is thrown upon theſe decorations. *

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* Montucla, Vol. 2. p. 599.

To theſe obſervations concerning deceptions of ſight, I ſhall add a ſimilar one of M. Le Cat, who took notice that the reaſon why we imagine objects to be larger when they are ſeen through a miſt, is the dimneſs or obſcurity with which they are then ſeen; this circumſtance being aſſociated with the idea of great diſtance. This he ſays is confirmed by our being ſurprized to find, upon approaching ſuch objects, that they are ſo much *nearer* to us, as well as ſo much *ſmaller*, than we had imagined.§

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* Accidens de la Vue, p. 358.

§ Traité des Sens, p. 260.

Among other caſes concerning viſion, which fell under the conſideration of M. De la Hire, he mentions one which is of difficult ſolution. It is when a candle, in a dark place, and ſituated beyond the limits of diſtinct viſion is viewed through a very narrow chink in a card, in which caſe a conſiderable number of candles, ſometimes ſo many as ſix, will be ſeen along the chink. This appearance he aſcribes to ſmall irregularities in the ſurface of the humours of the eye, the effect of which is not ſenſible when rays are admitted into the eye through the whole extent of the pupil, and conſequently one principal image effaces a number of ſmall ones; whereas, in this caſe, each of them is formed ſeparately, and no one of them is ſo conſiderable, as to prevent the others from being perceived at the ſame time. *

There are few perſons, M. De la Hire obſerves, who have both their eyes perfectly equal, not only with reſpect to the limits of diſtinct viſion, but alſo with reſpect to the *colour* with which objects appear tinged, when they are viewed by them, eſpecially if one of the eyes have been expoſed to the impreſſion of a ſtrong light. To compare them together in this reſpect, he directs us to take two thin cards, and to make in each of them a round hole, of a third or a fourth of a line in diameter, and applying one of them to each of the eyes, to look through the holes on a white paper, equally illuminated; when a circle of the paper will appear to each of the eyes, and placing the cards properly, theſe two circles may be made to touch one another, and thereby the appearance of the ſame object to each of the eyes may be compared to the greateſt advantage. To make this experiment with the greater exactneſs, it is neceſſary, he ſays, that the eyes be kept ſhut ſome time before the cards be applied to them. *

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* Accidens de la Vue, p. 400.

M. De la Hire firſt endeavoured to explain the cauſe of thoſe dark ſpots which ſeem to float before the eyes, eſpecially thoſe of old people. They are moſt viſible when the eyes are turned towards an uniform white object, as the ſnow in the open fields. If they be fixed when the eye is ſo, this philoſopher ſuppoſed that they were occaſioned by extravaſated blood upon the retina. But he thought that the movable ſpots were occaſioned by opaque matter floating in the aqueous humour of the eye. He thought the vitreous humour was not ſufficiently limpid for this purpoſe. †

By the following calculation M. De la Hire gives us an idea of the extreme ſenſibility of the optic nerves. One may ſee very eaſily, at the diſtance of 4000 toiſes, the ſail of a wind-mill, 6 feet in diameter, and, the eye being ſuppoſed to be an inch in diameter, the picture of this ſail, at the bottom of the eye, will be $\frac{1}{8000}$ of an inch, which is leſs than 666th part of a line, and is about the 66th part of a common hair, or the eighth part of a ſingle thread of ſilk. So ſmall, therefore, muſt one of the fibres of the optic nerve be, which he ſays is almoſt inconceivable, ſince each of theſe fibres is a tube that contains ſpirits. If birds perceive diſtant objects as well as men, which he thought very probable, he obſerves that the fibres of their optic nerves muſt be much finer than ours. §

Gaſſendi, we have ſeen, imagined that objects appear large when they are viewed with an open pupil, and ſmall when it

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* Accidens de la Vue, p. 358.

† Smith's Opticks. Remarks, p. 4.

§ Accidens de la Vue, p. 375.

was contracted; and that upon this principle he endeavoured to account for the extraordinary apparent magnitude of the horizontal moon; but at the time of which we are now treating, this ſubject was far better underſtood. Dr. Hooke obſerved, that if a perſon look through a hole a hundred times leſs than the pupil of his eye, an object will appear the ſame that it does to the naked eye; but that with reſpect to objects whoſe light is too ſtrong, it helps the eye, by debilitating the rays, which otherwiſe make a falſe repreſentation. *

I muſt not cloſe this ſection without mentioning a curious obſervation of the ingenious Mr. Grey, who made ſo conſpicuous a figure in the early part of the hiſtory of electricity; and whoſe name my reader will have the pleaſure of meeting with in the hiſtory of ſeveral other branches of experimental philoſophy. This gentleman took a ſtiff piece of brown paper, and pricking a ſmall hole in it, he held it at a little diſtance before him; when, applying a needle to his eye, he was ſurpriſed to ſee the point of it inverted. The nearer the needle was to the hole, the more it was magnified, but the leſs diſtinct; and if it was ſo held, as that its image was near the edge of the hole, its point ſeemed crooked. From theſe appearances he concluded, that theſe ſmall holes, or ſomething in them, produce the effects of concave ſpeculums, and from this circumſtance he took the liberty to call them *aerial ſpeculums*. †

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* Birch's Hiſtory, p. 503.

† Phil. Tranſ. ab. Vol. 1, p. 172.

SECTION VI.

IMPROVEMENTS IN OPTICAL INSTRUMENTS.

NEVER was the improvement of optical inſtruments ſo much the great and profeſſed object of philoſophers, as about the beginning of this period. Aſtoniſhing diſcoveries had been made by the firſt rude attempts of this kind; and, having no idea of any natural boundary to the improvement of viſion, they went to work with the moſt ſanguine expectations, from the effects of their different ſchemes. Towards the end of this period, however, we find Dr. Hooke complaining, that, though there was ſome life left in thoſe who *made* teleſcopes, the warmth of thoſe who ſhould have *uſed* them was grown cool, and that no new diſcoveries had been made by them, except what M. Caſſini had done, in diſcovering four new ſatellites of Saturn, beſides that of M. Zulichem. The ſame was the fate of microſcopes, to which, he ſays, no body gave any attention, except M. Leeuwenhoek; all other perſons uſing them for their diverſion only, eſpecially after they were made portable inſtruments, and could be carried in the pocket. The reaſon of this neglect, he ſuppoſed to be, that thoſe who firſt promoted theſe inquiries were gone off the ſtage, and it was then generally taken for granted, that no more diſcoveries could be made by thoſe inſtruments. He alſo hints, as what might be another reaſon, that no *gain* could be made by uſing them. [*]

The firſt who diſtinguiſhed themſelves in grinding teleſcopic glaſſes were two Italians, Euſtachio Divini at Rome, and Campani at Bologna, whoſe fame was much ſuperior to that of Divini, or that of any other perſon of his time; though Divini himſelf pretended, that, in all the trials that were made with their glaſſes, his, of a great focal diſtance, performed better than thoſe of Campani, and that his rival was not willing to try them fairly, viz. with equal eye glaſſes. [†] It is generally ſuppoſed, however, that Campani really excelled Divini, both in the goodneſs and the focal length of his object glaſſes. It was with teleſcopes made by Campani that Caſſini diſcovered the neareſt ſatellites of Saturn. They were made by the expreſs order of Lewis XIV, and were of 86, 100, and 136 Pariſian feet focal length. [§]

Campani ſold his lenſes for a great price, and he took every poſſible method to keep his art of making them a ſecret. His laboratory was inacceſſible to all the world, till after his death; when it was purchaſed by Pope Benedict XIV, who made a preſent of it to the academy called the *Inſtitute*, eſtabliſhed in that city; and by the account which M. Fougeroux has given of what he could diſcover from it, we learn that (except a machine, which M. Campani conſtructed, to work the baſons on which he ground his glaſſes) the goodneſs of his lenſes depended upon

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[*] Hooke's Experiments by Derham, p. 262.

[†] Phil. Tranſ. ab. Vol. 1, p. 193.

[§] Montucla, Vol. 2, p. 609.

the clearneſs of his glaſs, his Venetian tripoli, the paper with which he poliſhed his glaſſes, and his great ſkill and addreſs as a workman. It was alſo the general opinion at Bologna, that he owed a great part of his reputation to the ſecrecy, and air of myſtery which he affected; and that he made a great number of object glaſſes which he rejected, ſhewing only thoſe that were very good. He made few lenſes of a very great focal diſtance; and having the misfortune to break one of 141 feet in two pieces, he took incredible pains to join the two parts together, which he did at length effectually, ſo that it was uſed as if it had been entire; but it is not probable that he would have taken ſo much pains about it, if, as he pretended, he could very eaſily have made another as good. *

Dr. Hooke ſeems not to have had a very good opinion of the glaſſes that were made either by Divini or Campani, for ſpeaking of ſome which they made of 36 and 50 feet focal diſtance, he ſays that, judging by what was obſerved with them, concerning the true figure of Saturn, he conceived that they made but ordinary inſtruments, and did not exceed teleſcopes of 12 or 15 feet made in England.† But great allowance, no doubt, muſt be made for the prejudices of the Engliſhman in this caſe.

Sir Paul Neille, Dr. Hooke ſays, made teleſcopes of 36 feet, pretty good, and one of 50, but not of proportional goodneſs. Afterwards Mr. Reive firſt, and then Mr. Cox, who were the moſt celebrated in England, as grinders of optic glaſſes, made ſome good ones of fifty and ſixty feet focal diſtance, and Mr. Cox made one of a 100, but how good Dr. Hooke could not aſſert. §

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* Ac. Par. 1764. H. p. 282.

† Hooke's Experiments by Derham, p. 261.

§ Ib. p. 261.

Borelli, alſo, in France made object glaſſes of a great focal length, one of which he preſented to the Royal Society; [*] but I do not find any particular account of their goodneſs.

With reſpect to the focal length of teleſcopes, theſe and all others were far exceeded by M. Auzout, who made one object glaſs of 600 feet focus; but he was never able to manage it, ſo as to make any uſe of it. Hartſocker is even ſaid to have made ſome of a ſtill greater focal length[†]; but this ingenious mechanick, finding it impoſſible to make uſe of object glaſſes, the focal diſtance of which was much leſs than this, when they were incloſed in a tube, contrived a method of uſing them without a tube, by fixing them at the top of a tree, a high wall, or the roof of a houſe. [§]

M. Huygens, who was alſo an excellent mechanick, made conſiderable improvements in the method of uſing an object glaſs without a tube. He placed it at the top of a very long pole, having previouſly incloſed it in a ſhort tube, which was made to turn in all directions, by means of a ball and ſocket. The axis of this tube he could command with a fine ſilken ſtring, ſo as to bring it into a line with the axis of another ſhort tube, which he held in his hand, and which contained the eye glaſs. In this method he could make uſe of object glaſſes of the greateſt magnifying power, at whatever altitude his object was, and even in the zenith, provided his pole was as long as his teleſcope; and, to adapt it to the view of objects of different altitudes, he had a contrivance, by which he could raiſe or depreſs a ſtage that ſupported his object glaſs at pleaſure.

M. De la Hire made ſome improvement in this method of managing the object glaſs, fixing it in the center of a board, and not in a tube; but as it is not probable that this method will ever be made uſe of, ſince the diſcovery of both reflecting and achromatic teleſcopes, which are now brought to great perfection, and have even micrometers adapted to them, I ſhall not deſcribe this apparatus minutely; but ſhall content myſelf with giving a drawing of M. Huygens's pole, which, with a very ſhort explanation, will be ſufficient for my purpoſe. In fig. 61, *a* repreſents a pulley, by the help of which a ſtage *c*, *d*, *e*, *f* (that ſupports the object glaſs, *k*, and the apparatus belonging to it) may be raiſed higher or lower at pleaſure, the whole being counterpoiſed by the weight *h*, faſtened to a ſtring *g*. [*n*] is a weight, by means of which the center of gravity of the apparatus belonging to the object glaſs is kept in the ball and ſocket, ſo that it may be eaſily managed by the ſtring *l*, *u*, and its axis brought into a line with the eye glaſs at *o*. When it was very dark, M. Huygens was obliged to make his object glaſs viſible by a lantern, *y*, ſo conſtructed, as to throw the rays of light in a parallel direction up to it.

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[*] Hooke's Experiments, by Derham, p. 261.

[†] Montucla, Vol. 2, p. 609.

[§] Miſcell. Berol, Vol. 1, p. 261.

The recollection of the incredible pains which philoſophers of the laſt age took in making obſervations, and the great expences they were obliged to be at for that purpoſe, ſhould make us ſenſible of the obligations we are under to ſuch men as Gregory, Newton, and Dollond, who have enabled us to get clearer and more ſatisfactory views of the remote parts of our ſyſtem, with much leſs labour and expence; and ſhould likewiſe make us more diligent and ſolicitous to derive all the advantage we poſſibly can from ſuch capital improvements.

The reaſon why it is neceſſary to make the common dioptric teleſcopes ſo very long is, that the length of them muſt be increaſed in no leſs a proportion than the duplicate of the increaſe of their magnifying power; ſo that, in order to magnify twice as much as before, with the ſame light and diſtinctneſs, the teleſcope muſt be lengthened four times, and to magnify thrice as much, nine times, and ſo on. *

Before I mention the *reflecting teleſcope*, I muſt obſerve that M. Auzout, in a paper delivered to the Royal Society, obſerved that the apertures which the object glaſſes of refracting teleſcopes can bear with diſtinctneſs are in about a ſub-duplicate proportion to their lengths, and upon this ſuppoſition he drew up a table of the apertures proper for object glaſſes of a great variety of focal lengths, from 4 inches to 400 feet. † Upon this occaſion, however, Dr. Hooke obſerved, that the ſame glaſs will bear a greater or leſs aperture, according to the leſs or greater light of the object. If for inſtance, he was viewing the ſun, or Venus, or any of the fixed ſtars, he uſed ſmaller apertures, but if he wanted to view the moon by day light, or Saturn, Jupiter, or Mars by night, he uſed a larger aperture.§

But the merit of all theſe improvements was, in a manner, cancelled by the diſcovery of the much more commodious *reflecting teleſcope*. For a refracting teleſcope, even of a 1000 feet focus, ſuppoſing it poſſible to be made uſe of, could not be made to magnify with diſtinctneſs more than 1000 times; whereas a reflecting teleſcope, not exceeding 9 or 10 feet, will magnify 1200 times. ||

The reflecting teleſcope was the happy thought of our countryman James Gregory, who was led to this important conſtruction, in contriving how to obviate an inconvenience attending the uſe of ſpherical lenſes, which, however, is of trifling conſequence; and the method he took to accompliſh his own ſcheme was ſuch, that it could never be brought to perfection, or made of any uſe.

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* Smith's Optics, Vol. 2, p. 354. † Phil. Tranſ. ab. Vol. 1, p. 191. § Ib. p. 192.
|| Montucla, Vol. 2, p. 609.

He obſerved that a ſpherical lens does not give the image of a plane object, perpendicular to its axis, in a plane, but that it will be a curve, and concave on the ſide of the object glaſs; whereas he found that an image of the ſame object from glaſſes, ground to the form of ſome of the conic ſections, would be in a plane. With this idea he would have made uſe of elliptical, or parabolic glaſſes; but he recollected the little ſucceſs that former opticians had had in their attempts to grind ſuch glaſſes, and imagined, though without ground, that it would be more eaſy to make a metallic ſpeculum in ſome of thoſe forms, and by poliſhing it afterwards, to get an image by reflected light. Accordingly he propoſed the conſtruction of a teleſcope with two concave metallic mirrors. One of them was in the form of a parabola, and was placed at the bottom of a tube, being intended to make the rays which proceeded from any object converge to a point in the axis of the parabola produced. In this axis was placed another ſmaller and elliptic mirror, which, receiving the rays, was to form an image within a ſmall diſtance of the larger parabolic mirror. This was perforated in the center, to receive an eye glaſs, of a ſize proper to view this image, as in a common teleſcope.

But though the theory of the curvature of the images of plane objects by ſpherical mirrors be juſt, the object glaſſes of teleſcopes are too ſmall a portion of a ſphere to make the defect ariſing from it ſenſible. Beſides, it was not found more eaſy to make a truly parabolic or elliptic mirror of metal, than it had been to make them of glaſs; and, in fact, our author never brought his teleſcope to any degree of perfection, not pretending to any ſkill in mechanicks, and only propoſing it for others to execute. He had, indeed, a ſpherical object ſpeculum of metal, and a ſmall concave, as well as a ſmall convex ſpeculum, ground by Rieves and Cox, but becauſe the metal was not well poliſhed, he only made ſome imperfect trials, not ſo much as fitting his ſpeculums and eye glaſs in a tube; and being diſcouraged, both becauſe he could not get a parabolical ſpeculum, and becauſe that which he tried was not well poliſhed, he gave over the thoughts of bringing ſuch teleſcopes into uſe.* Sir Iſaac Newton, however, neglecting Gregory's imaginary improvements, by means of parabolic and elliptic mirrors, retained his idea of accompliſhing the end by reflexion rather than by refraction and contenting himſelf with ſpherical mirrors, produced a truly admirable inſtrument, of which, and of its ſucceſſive improvements an account will be given hereafter.†

Dr. Hooke conſtructed a reflecting teleſcope in which the great mirror was perforated, ſo that the ſpectator looked directly towards the object, and it was produced before the royal ſociety February 5, 1674. § On this occaſion it was ſaid that this conſtruction was firſt propoſed by Merſennus, and afterwards repeated by Gregory, but was thought never to have been actually executed before it was done by Dr. Hooke. A deſcription of this inſtrument may be ſeen in Hooke's experiments by Derham, p. 269.

After this account of the improvement of teleſcopes, I ſhall recite what was done in the ſame period with reſpect to *microſ-*

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* Gregory's Opticks, p. 212.

† Montucla, Vol. 2, p. 595.

§ Birch's Hiſtory, Vol. 3, p. 122.

copes. Euſtachio Divini made microſcopes with two common object glaſſes, and two plano-convex eye glaſſes joined together on their convex ſides, ſo as to meet in a point. The tube in which they were incloſed was as big as a man's leg, and the eye glaſſes almoſt as broad as the palm of a man's hand. * Mr. Oldenburg, ſecretary to the Royal Society, received an account of this inſtrument from Rome, and read it at one of their meetings, Auguſt 6, 1668.

It was within this period that Hartſocker improved ſingle microſcopes, by uſing ſmall globules of glaſs, made by melting them in the flame of a candle, inſtead of the lenſes which had before been made uſe of for that purpoſe. By this means he firſt diſcovered the *animalcula* in *ſemine maſculino,* which gave riſe to a new ſyſtem of generation. A microſcope of this kind, conſiſting of a globule of $\frac{1}{10}$ of an inch in diameter, M. Huygens demonſtrated to magnify 100 times; and ſince it is eaſy to make them of leſs than half a line in diameter, they may be made to magnify 300 times. Were it not for the difficulty of applying objects to theſe magnifiers, the want of light, and the ſmall field of diſtinct viſion, they would certainly have been the moſt perfect of all microſcopes.

But no man diſtinguiſhed himſelf ſo much by microſcopical diſcoveries as the famous M. Leeuwenhoek, though he uſed only ſingle lenſes with ſhort foci, preferring diſtinctneſs of viſion to a large magnifying power.

M. Leeuwenhoek's microſcopes were all ſingle ones, each of them conſiſting of a ſmall double convex glaſs, ſet in a ſocket, between two ſilver plates, riveted together, and pierced with a ſmall hole; and the object was placed on the point of a needle, ſo contrived, as to be placed at any diſtance from the lens. If the objects were ſolid, he faſtened them with glue, and if they were fluid, or, on other accounts, required to be ſpread upon glaſs, he placed them on a ſmall piece of Muſcovy talc, or glaſs blown very thin, which he afterwards glued to his needle. He had, however, a different apparatus for viewing the circulation of the blood, which he could fix to the ſame microſcopes.

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* Birch's Hiſtory, Vol. 4, p. 313.

The greateſt part of his microſcopes M. Leeuwenhoek bequeathed to the Royal Society. They were contained in a ſmall Indian cabinet, in the drawers of which were thirteen little boxes, or caſes, in each of which were two microſcopes, neatly fitted up in ſilver; and both the glaſs, and the apparatus were made with his own hands.

The glaſs of all theſe lenſes is exceedingly clear, but none of them magnifies ſo much as thoſe globules which are frequently uſed in other microſcopes; but Mr. Folkes, who examined them, thought that they ſhewed objects with much greater diſtinctneſs, which M. Leeuwenhoek principally valued. His diſcoveries, however, are to be aſcribed not ſo much to the goodneſs of his glaſſes, as to his great judgment, acquired by long experience, in uſing them. He alſo particularly excelled in his manner of preparing objects for being viewed to the moſt advantage. On this account it becomes no perſon haſtily to condemn any of M. Leeuwenhoek's obſervations; and he himſelf has reminded us, more than once, that thoſe who are the moſt ſkilled in the uſe of microſcopes may be miſled, if they give too ſudden a judgment on what they ſee, or till they have been aſſured, by repeated experiments. But ſo many of his ſurpriſing diſcoveries have been confirmed by the moſt judicious obſervers, that we have no reaſon to diſtruſt his accuracy with reſpect to thoſe which have not yet been ſo frequently, or ſo carefully examined. *

Mr. Baker, who alſo examined M. Leeuwenhoek's microſcopes, and made a report concerning them to the Royal Society, found that the greateſt magnifier among them enlarged the diameter of an object about a hundred and ſixty times, but that all the reſt fell much ſhort of that power, ſo that he concluded, that M. Leeuwenhoek muſt have had other microſcopes, of a much greater magnifying power, for many of his diſcoveries. And it appears, he ſays, by many circumſtances, that he had ſuch microſcopes.†

It appears from M. Leeuwenhoek's writings, that he was not unacquainted with the method of viewing opaque objects by means of a ſmall concave reflecting mirror, which was afterwards improved by M. Lieberkhun. For, after deſcribing his apparatus for viewing eels in glaſs tubes, he adds that he had an inſtrument to which he ſcrewed a microſcope ſet in braſs, upon which microſcope he faſtened a little diſh of braſs, probably, that his eye might be thereby aſſiſted to ſee objects better; for he ſays he had filed the braſs which was round his microſcope as bright as he could, that the light, while he was viewing objects, might be reflected from it as much as poſſible. This microſcope with its diſh (of which an exact copy, taken from the picture in his works, may be ſeen fig. 62) ſeems ſo like our opaque microſcope, with its ſilver ſpeculum, that it may well be ſuppoſed to have given the hint to the ingenious inventor of it, provided he ever attended to it. §

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* Phil. Tranſ. ab. Vol. 6, p. 154, &c. † Ib. Vol. 8, p. 122. § Ib. Vol. 8, p. 128.

In 1702, Mr. Wilſon made ſeveral ingenious improvements in the method of uſing ſingle magnifiers, for the purpoſe of viewing tranſparent objects, and they have been ſo generally approved, that hardly any other method is made uſe of at this day. The capital advantage attending this microſcope conſiſts in its being furniſhed with a pretty large lens, to throw light upon the objects, and alſo with a fine ſcrew and ſpring, to bring them nearer, or remove them farther from the magnifier at pleaſure. See a deſcriprion of this inſtrument Phil. Tranſ. ab. Vol. 4, p. 199. Wilſon's microſcope is alſo a neceſſary part of the ſolar microſcope, invented afterwards.

In 1710, Mr. Adams gave to the Royal Society the following account of his method of making ſmall globules for large magnifiers. He took a piece of fine window glaſs, and cut it, with a diamond, into as many lengths as he thought proper, not exceeding ⅛ of an inch in breadth; then, holding one of them between the fore finger and thumb of each hand, over a very fine flame, till the glaſs began to ſoften, he drew it out till it was as fine as a hair, and broke; then putting each of the ends into the pureſt part of the flame, he had two globules preſently, which he could make larger or leſs at pleaſure. If they were held a long time in the flame, they would have ſpots in them, ſo that he drew them out preſently after they became round. The ſtem he broke off as near to the globule as he could, and lodging the remainder between the plates, in which holes were drilled exactly round, the microſcope, he ſays, performed to admiration. Through theſe magnifiers, he ſays, that the ſame thread of very fine muſlin appeared three or four times bigger than it did in the largeſt of Mr. Wilſon's magnifiers.[†]

[*] Phil. Tranſ. ab. Vol. 4, p. 204.

The ingenious Mr. Grey, whoſe name has already occurred in the courſe of this hiſtory, hit upon a very eaſy expedient to make very good temporary microſcopes, at a very little expence. They conſiſt of nothing but very ſmall drops of water, taken up with a point of a pin, and put into a ſmall hole made in a piece of metal. Theſe globules of water do not, indeed, magnify ſo much as thoſe which are made of glaſs of the ſame ſize; becauſe the refractive power of water is not ſo great; but the ſame purpoſe will be anſwered nearly as well by making them ſomewhat ſmaller.

The ſame ingenious perſon, obſerving that ſmall heterogeneous particles incloſed in the glaſs of which microſcopes are made, were much magnified when thoſe glaſſes were looked through, thought of making his microſcopes of water that contained living animalcula, to ſee how they would look in this new ſituation; and he found his ſcheme to anſwer even beyond his utmoſt expectation, ſo that he could not even account for their being magnified ſo much as they were: for it was much more than they would have been magnified, if they had been placed beyond the globule, in the proper place for viewing objects. But Montucla obſerves, that when any object is incloſed within this ſmall tranſparent globule, the hinder part of it acts like a concave mirror, provided they be ſituated between that ſurface and the focus; and that, by this means, they are magnified above 3 ½ times more than they would have been in the uſual way.*

In this period it was that MICROMETERS were added to teleſcopes and microſcopes, in order to meaſure the diameters of objects, and all ſmall apparent diſtances, with more exactneſs than could be done by the help of any quadrant, octant, ſextant, &c. This was effected by meaſuring the image of an object in the focus of the object glaſs; and for this purpoſe a variety of mechanical contrivances were applied. Till this method was hit upon, obſervers uſed to meaſure the field of view in each of their teleſcopes, by obſerving how much of the moon they could ſee through it, the ſemidiameter being reckoned at 15 or 16 minutes*; and other diſtances were eſtimated by the eye, comparing them with the field of view.

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* Montucla, Vol. 2, p. 610.

The firſt perſon who hit upon the more exact method, mentioned above, was Mr. Gaſcoigne, before the civil wars in England. The ſame ingenious gentleman firſt applied *teleſcopic ſights* to quadrants†, and alſo had a treatiſe on opticks prepared for the preſs; but to the great regret of men of ſcience, he was killed in the ſervice of Charles I, and his manuſcript was never found. His inſtrument, however, fell into the hands of Mr. R. Townley, who ſays, that by the help of it, he could mark above 40000 diviſions in a foot.

Mr. Gaſcoigne's inſtrument being ſhewn to Dr. Hooke, he gave a drawing and deſcription of it, and propoſed ſeveral improvements in it, which may be ſeen Phil. Tranſ. ab. Vol. 1, p. 217. Mr. Gaſcoigne divided the image of an object, in the focus of the object glaſs, by the approach of two pieces of metal, ground to a very fine edge, in the place of which Dr. Hooke would ſubſtitute two fine hairs ſtretched parallel to one another. Two other methods of Dr. Hooke's, different from this, are deſcribed in his Poſthumous Works, p. 497, 498. An account of ſeveral curious obſervations that Mr. Gaſcoigne made by the help of his micrometer, particularly in the menſuration of the diameters of the moon and other planets, may be ſeen in the Philoſophical Tranſactions, Vol. 48, p. 190.

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* Miſc. Berol, Vol. 1, p. 202.

† Phil. Tranſ. Vol. 48, p. 190.

M. Huygens, as appears by his *Syſtem of Saturn*, publiſhed in 1659,* uſed to meaſure the apparent diameters of the planets, or any ſmall angles, by firſt meaſuring the quantity of the field of view in his teleſcope; which, he ſays, is beſt done by obſerving the time which a ſtar takes up in paſſing over it, and then preparing two or three long and ſlender braſs plates, of various breadths, the ſides of which were very ſtraight, and converging to a ſmall angle. In making uſe of theſe pieces of braſs, he made them ſlide in two ſlits, that were made in the ſides of the tube, oppoſite to the place of the image, and obſerved in what place it juſt covered the diameter of any planet, or any ſmall diſtance that he wanted to meaſure. It was obſerved, however, by Sir Iſaac Newton, that the diameters of planets, meaſured in this manner, will be larger than they ſhould be, as all lucid objects appear to be, when they are viewed upon dark ones. †

In the Ephemerides of the Marquis of Malvaſia, publiſhed in 1662, it appears that he had a method of meaſuring ſmall diſtances between fixed ſtars, and the diameters of the planets, and alſo of taking accurate draughts of the ſpots of the moon; and this was by a net of ſilver wire, fixed in the common focus of the object and eye glaſs. He alſo contrived to make one of two ſtars to paſs along the threads of this net, by turning it, or the teleſcope, as much as was neceſſary for that purpoſe; and he counted, by a pendulum clock, beating ſeconds, the time that elapſed in its paſſage from one wire to another, which gave him the number of the minutes and ſeconds of a degree contained between the intervals of the wires of his net, with reſpect to the focal length of his teleſcope. *

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* Ac. Par. 1717, M. p. 101.

† Smith's Opticks, Vol. 2, p. 342.

In 1666 Meſſrs Auzout and Picard publiſhed a deſcription of a micrometer, which was nearly the ſame with that of the Marquis of Malvaſia, excepting the method of dividing it, which they performed with more exactneſs, by a ſcrew. In ſome caſes, they uſed threads of ſilk, as being finer than ſilver wires. † Dechales alſo recommends a micrometer conſiſting of fine wires, or ſilken threads, the diſtances of which were exactly known, diſpoſed in the form of a net, as peculiarly convenient for taking a map of the moon. §

M. De la Hire ſays that there is no method more ſimple or commodious, for obſerving the digits of an eclipſe, than a net in the focus of the teleſcope. Theſe, he ſays, were generally made of ſilken threads, and that for this particular purpoſe ſix concentric circles had alſo been made uſe of, drawn upon oiled paper; but he adviſes to draw the circles on very thin pieces of glaſs, with the point of a diamond. He alſo gives ſeveral particular directions to aſſiſt perſons in the uſe of them. ‡ In another memoir he ſhews a method of making uſe of the ſame net for all eclipſes, by uſing a teleſcope with two object glaſſes, and placing them at different diſtances from one another. ||

M. Caſſini invented a very ingenious method of aſcertaining the right aſſenſions and declinations of ſtars, by fixing four croſs hairs in the focus of the teleſcope, and turning it about its axis,

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* Ac. Par. 1717, M. p. 100. † Ib. p. 101. § Acta Eruditorum, 1710, p. 464.

‡ Ac. Par. 1701, M. p. 154. || Ib. 17:7, M. p. 72.

ſo as to make them move in a line parallel to one of them. The difficulty there was in accompliſhing this was entirely removed by a mechanical contrivance of Dr. Bradley.* As I propoſe to dwell as little as poſſible on ſubjects of mere mechaniſm, I muſt refer my reader to Dr. Smith for his deſcription of the beſt micrometer of this kind.†

I ſhall add in this place, that J. And. Segner, in a later period, propoſed to enlarge the field of view in theſe micrometers, by making them of a conſiderable extent, and having a moveable eye glaſs, or ſeveral eye glaſſes, placed oppoſite to different parts of it. He thought however, that two would be quite ſufficient, and he gives particular directions how to make uſe of ſuch micrometers in aſtronomical obſervations.§

M. Leeuwenhoek's method of eſtimating the ſize of *ſmall objects* was by comparing them with grains of ſand, of which 100 placed in a line, took up an inch. Theſe grains he laid on the ſame plate with his objects, and viewed them at the ſame time.‡ Dr. Jurin's method was ſimilar to this of M. Leeuwenhoek, for he found the diameter of a piece of fine ſilver wire, by wrapping it as cloſe as he could about a pin, and obſerving how many rings made an inch. For he uſed this wire in the ſame manner as Leeuwenhoek uſed his ſand.

Mr. Martin, in his Opticks, recommends ſuch a micrometer to a microſcope, as had been applied to teleſcopes; for he adviſes to draw a number of parallel lines, on a piece of glaſs, with the fine point of a diamond, at the diſtance of $\frac{1}{40}$ of an inch from one another, and to place it in the focus of the eye glaſs. By this method, Dr. Smith contrived to take the exact draught of objects viewed by a double microſcope. For he adviſes to get a lattice, made with ſmall ſilver wires or ſmall ſquares, drawn upon a plain glaſs, by the ſtrokes of a diamond, and to put it into the place of the image, formed by the object glaſs. Then by transferring the parts of the object, ſeen in the ſquares of the glaſs, or lattice, upon ſimilar correſponding ſquares, drawn on paper, the picture may be exactly taken.[*] Mr. Martin alſo introduced into compound microſcopes, another micrometer, conſiſting of a ſcrew. Both the methods are deſcribed in his Opticks, p. 277.

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* Smith's Opticks, Vol. 2, p. 343. † Ib. p. 345. § Com. Gotting, Vol. 1. p. 27.

‡ Baker on the Microſcope. p. 42.

Dr. Hooke uſed to look upon the magnified object with one eye, while, at the ſame time, he viewed other objects, placed at the ſame diſtance, with the other eye. In this manner he was able, by the help of a ruler, divided into inches and ſmall parts, and laid on the pedeſtal of the microſcope, to caſt, as it were, the magnified appearance of the object upon the ruler, and thereby exactly to meaſure the diameter which it appeared to have through the glaſs; which being compared with the diameter as it appeared to the naked eye, eaſily ſhewed the degree in which it was magnified. This, ſays Mr. Baker, is a ready and good method for many objects; and he declares, from his own experience, that a little practice will render it exceedingly eaſy and pleaſant.[†]

I ſhall here introduce a few miſcellaneous articles concerning improvements in optical inſtruments made by Dr. Hooke, and then I ſhall cloſe the ſection with an account of what was done with reſpect to *burning mirrors*.

We are obliged to Dr. Hooke for an excellent method of viewing the ſun without injuring our eyes. For this purpoſe he contrived that the rays ſhould be reflected from one plane to another, till it was ſo much weakened, that the eye might receive it with great ſafety and pleaſure. This method is much preferable to that of looking at the ſun through a ſmoaky or coloured glaſs, which gives it a red and diſagreeable hue. His diſcourſe on this ſubject was read before the Royal Society June 28, 1675.*

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[*] Baker on the Microſcope, p. 48. [†] Ib. p. 45.

Dr. Hooke contrived an inſtrument, conſiſting of convex glaſſes, for the benefit of ſhort ſighted perſons; and at a meeting of the Royal Society, in 1679, he read a diſcourſe concerning it; in which he obſerved, that by the uſe of ſeeing things inverted (which was the effect of this inſtrument) it became as natural as if they were ſeen erect; and he conceived that if a perſon from his childhood were uſed to ſee things in this manner, and he ſhould afterwards come to ſee them without the help of thoſe glaſſes, he would fancy them to be inverted, as they really are at the bottom of the eye; as, he ſays, is very viſible in the eye of a young cat, which is almoſt tranſparent at the bottom.† He alſo contrived a *portable camera obſcura,* for the purpoſe of drawing objects as large as the life. §

The largeſt BURNING MIRROR that had been made before the middle of the 17th century was that of Magine, of 20 inches diameter. Afterwards we find an account of one made by Septala of Milan, which was near 3 ½ feet in diameter, and which burnt at the diſtance of 15 feet. About the ſame time M. Villette, an artificer in Lyons, diſtinguiſhed himſelf firſt by the conſtruction of one of 30 inches in diameter; but being the portion of a ſphere of 12 feet in diameter, it burnt at 3 feet diſtance; and its focus being no larger than half a louis of that time, was ſmaller in proportion to its ſurface than that of Septala, ſo that the heat it produced was conſiderably greater. With this mirror he could melt, in a few ſeconds, metals the moſt difficult of fuſion; and in the ſame ſmall ſpace of time he could even vitrify ſtones and earths, over which the common fire has no power, as crucibles, &c. This mirror was purchaſed by Louis XIV, and is now in the King of France's cabinet. Afterwards the ſame workman finiſhed another mirror of 44 inches in diameter, which was purchaſed by the Landgrave of Heſſe.

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* Birch's Hiſtory, Vol. 3, p. 179. † Ib. Vol. 3, p. 500. § Ib. Vol. 3, p. 436. Vol. 4, p. 157.

Villette's ſpeculums were, however, much inferior to one which Tſchirnhauſen conſtructed about the year 1687, an account of which may be ſeen *Acta Eruditorum* for 1687, p. 52. This was about 4 $\frac{1}{2}$ French feet in diameter, and it burnt at the diſtance of 12 feet. It was not made like thoſe of M. Villette, of a mixture of melted metals; but was a thin plate of copper, and therefore very light with reſpect to its ſize. The effects of this mirror were extraordinary indeed; as there was no ſubſtance which it could not vitrify, not excepting even the aſbeſtos, which was thought to be indeſtruetible in the fire. The inconvenience attending the uſe of reflecting mirrors, made the ſame gentleman endeavour to procure glaſs lenſes of the ſame ſize, and power; and from a glaſs-houſe in Saxony he did at length procure one, that was 3 feet in diameter, and convex on both ſides, with a focus at 12 feet diſtance from it, and 1 $\frac{1}{2}$ inches in diameter; but to increaſe the heat, the rays were drawn nearer together by another common lens. This mirror produced the ſame effects with thoſe mentioned above, but in ſtill leſs time. It was purchaſed by the Duke of Orleans, who made a preſent of it to the French Academy. When finiſhed, it weighed 160 pounds.

Attempts were afterwards made to conſtruct burning mirrors of wood, and other lighter materials, and then to give them a coating of varniſh, or leaf gold; and ſome of theſe are ſaid to have produced conſiderable effects. But it does not appear that they exceeded, or even ſo much as equalled what was performed by thoſe mentioned above. *

Some attempts were made to apply burning mirrors to *practical uſes*, particularly one of the parabolic form, conſtructed by P. F. Tertio De Lanis, for chymical uſes, a figure of which may be ſeen *Acta. Eruditorum*, 1688, p. 38. But it doth not appear that any ſuch uſes as this author imagined were ever made of mirrors.

SECTION VII.

IMPROVEMENTS IN THE MATHEMATICAL PART OF THE SCIENCE OF OPTICKS.

THOUGH I make it a rule to myſelf to keep cloſe to *philoſophical* diſcoveries in opticks, and to avoid treating of thoſe that are properly *mathematical*, as much as I poſſibly can, yet the ſubjects are naturally ſo much intermixed, and one of them is on many occaſions ſo ſubſervient to the other, that it would be deviating from nature, to attempt an intire ſeparation of them; and eſpecially not to give ſome account of the moſt ſucceſsful labours of thoſe mathematicians who have given their attention to the ſubject of opticks, though they have made no diſcoveries in the ſcience that can properly be called philoſophical. Several concluſions deduced from the principles of geometry, and others by the rules of Algebra, have been already occaſionally mentioned in this hiſtory; and in this ſection I ſhall give a general account of various other mathematical diſcoveries, which took their riſe from obſervations in the ſcience of opticks.

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* Montucla, Vol. 2, p. 612.

The moſt philoſophical of all my readers will certainly excuſe me if I ſtep ſo far within the bounds of geometry, as to give ſome account of *cauſtic curves*, becauſe optical conſiderations gave the firſt hint of them; and ſo nearly are they related to my ſubject that, a hiſtory of the progreſs of knowledge concerning light and colours would be deemed imperfect without it. The diſcovery of theſe curves belongs to Mr. Tſchirnhauſen, who is ſo famous on account of his burning mirrors.

It is well known that rays of light, reflected by a concave ſurface, converge to a point, which is called their *focus*, on account of the heat that is generally produced by them at that place. But this focus is not a ſingle point, but commonly the place to which only the greater number of the rays are reflected. There is, in fact, a kind of *continued focus*, which is eaſily made viſible. If a candle be brought near a cylindrical veſſel, highly poliſhed on the inſide, two curved lines of light will be ſeen at the bottom of it, and they will be more diſtinguiſhable, the more obliquely the candle is preſented. This line is the cauſtic of the rays reflected from this ſurface, and what is properly called the focus of burning mirrors is about the point, where the two branches of the cauſtic meet, becauſe the greater part of the rays croſs one another near this point.

To conceive more diſtinctly the generation of theſe curves, parallel rays muſt be drawn at equal diſtances, as in fig. 63, when each reflected ray will interſect that which is next to it; and from all theſe points of interſection, and the parts of the reflected rays, will be formed a polygon, which, when the rays are conſidered as infinitely near to one another, will become a curve, each point of which may be conſidered as a focus of two rays infinitely near to one another.*

It is poſſible that a clearer idea may be conceived of the nature of cauſtic curves, by conſidering them as curves formed by rays of light, reflected or refracted, after falling upon another curve. Thus let L, fig. 64, be a luminous point, emitting the rays LK, LF, LH, LI, &c. which are reflected at the curve KI, ſo that the angles of reflexion are equal to thoſe of incidence, the reflected rays being KO, FP, HQ, IR, &c. Another curve OPQR being drawn, ſo that the reflected rays may all be tangents to it, will be the cauſtic by reflexion, of the curve from which they are reflected.† In the ſame manner may a clear idea be conceived of cauſtics by refraction.

M. Tſchirnhauſen publiſhed his firſt thoughts upon this ſubject in the memoirs of the French Academy for 1682, where he gave a delineation of the cauſtic of a circle formed by parallel incident rays. Some properties of theſe curves were demonſtrated by M. De la Hire, who diſcovered ſome miſtakes that M. Tſchirnhauſen had fallen into with reſpect to them. This occaſioned ſome debate betwixt theſe two gentlemen, and M. Tſchirnhauſen did not acknowledge his miſtake till ſome years after, on the communication of a new obſervation of M. Bernoulli.

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* Montucla, Vol. 2, p. 343.

† Saverien's Dictionaire, Vol. 1, p. 136.

Other geometricians preſently improved upon the theory of M. Tſchirnhauſen, extending the cauſtic curves to converging and diverging rays, refracted as well as reflected. They who diſtinguiſhed themſelves the moſt upon this ſubject were the two illuſtrious brothers, James and John Bernoulli. *

M. Bouguer, who was as eminent for his ſkill in geometry, as for his improvements in opticks, of which an account will be given in the laſt period of this hiſtory, obſerved that there are *two cauſtics* formed at the ſame time, by convex and concave ſurfaces, and that they occaſion two different images of objects, ſeen by reflexion from them. This obſervation, he ſays, is the more important, as both the images are, in ſome caſes, equally diſtinct.

To give an idea of this double cauſtic, let AIB, fig. 65, repreſent the ſurface of a perfectly poliſhed ſphere. All the incident rays which are infinitely near to one another, and which fall upon D and *d*, will be reflected as if they had iſſued from G, but the rays that proceed laterally, with reſpect to thoſe that fall on D, that is thoſe which fall on *e*, will not come to the eye as if they had iſſued from G, but from E, in which the reflected rays interſect the axis of the globe AB, which paſſes through the luminous body L. In reality, the incident ray L *e*, and the reflected ray *en*, are in a different plane from the rays LD and DM; and theſe two planes have CL for their common ſection. We muſt therefore diſtinguiſh the two different foci of reflected rays, according as the incident ones fall above or below D, or laterally with reſpect to it.

If the aperture of our eyes was a narrow vertical cleft, we ſhould only receive the rays which have their focus at G; whereas, if our pupil was a horizontal cleft, we ſhould only be affected by thoſe rays that ſeem to come from the point E. But as the light enters our eye by a round aperture, both the images will be formed upon the retina at the ſame time; and if, notwithſtanding this, we almoſt always perceive only one of them, it is chiefly becauſe our eyes are not adapted to ſee ſmall objects with the ſame diſtinctneſs at all diſtances, and that one of the two is at a more convenient diſtance from the eye than the other. *

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* Montucla, Vol. 2, p. 344.

Beſides the diſcuſſion of the optical queſtion abovementioned, which, probably, gave the firſt hint of theſe cauſtic curves, Dr. Barrow was the firſt who gave the theory of the foci of lenſes of different convexities and concavities, combined in all poſſible methods. For, excepting a very few caſes, the foci of ſuch glaſſes had, before his time, been determined by experiment only. This geometrician alſo gave a complete and elegant ſolution of the problem, to determine univerſally the concourſe of incident rays, whether parallel, converging, or diverging, as well as other problems, in which geometry lends its aid to opticks; but as they reſpect no new property of light or colours, it is more agreeable to my plan wholly to omit the mention of them.†

M. De la Hire obſerved that, when a luminous object is viewed through a plane and poliſhed glaſs in the dark, it often appears multiplied 4 times, or more, and that the multiplied object appears more diſtinct, the more obliquely the glaſs is held with reſpect to the eye. This multiplication of objects appears ſometimes on one ſide, and ſometimes on the other ſide of the luminous object; and the images grow more and more faint in proportion to their diſtance from it; but there is an oblique poſition of the glaſs, in which the object does not appear multiplied;

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* Traité d' Optique, p. 100.

† Montucla, Vol. 2, p. 597.

plied ; and ſome glaſſes do not multiply an object in this manner, though they be ever ſo much inclined.

By tracing the progreſs of a ray of light through ſuch plates of glaſs, this philoſopher, who was alſo an excellent geometrician, found that, if the two ſides were truly parallel, this multiplication of images could not take place; becauſe every ray, being equally refracted at both the ſurfaces, muſt iſſue parallel to the direction in which it entered ; and that, therefore, no more of them can enter the pupil of the eye than if the object had been viewed without the glaſs interpoſed. He alſo ſhews that, if the rays be reflected ever ſo often within the glaſs, from one ſurface to the other, they will ſtill iſſue parallel, and therefore can never make the object appear in more places than one, as appears by fig. 66, which needs no explanation.

But he demonſtrates that, if the two ſurfaces of the glaſs be not parallel, but inclined to one another, as ED and BD, fig. 67, every ray that ſuffers two refractions, as CR, will iſſue in PO, making a larger angle with the ſurface BD than BO, which has not been reflected within the glaſs ; and conſequently theſe two rays will meet ſomewhere, as in the point O, where if the eye be ſituated, it will ſee two images of the object C, one by means of the ray BO, which will be much the ſtronger, and the other by means of PO, which will be weaker, and ſituated on the right hand of the principal image. Other rays falling beyond R, being reflected oftener within the glaſs, will, for the ſame reaſon, form other images, more diſtant from the place of the firſt ; and a great part of the light being loſt at every reflexion, they will be continually fainter and fainter. In the ſame manner he demonſtrates geometrically, that the more obliquely the glaſs is held, the greater is the diſtance between theſe images. ---

In the caſe that has now been explained, it appears, that the multiplication of images will be on the right hand of the principal image; but when the two ſurfaces are inclined the other way, as in fig. 68, he ſhews, by tracing the progreſs of the rays in a ſimilar manner, that the images will fall on the contrary ſide of the principal image; ſo that they will always appear on that ſide to which the two ſurfaces are inclined to one another.

Theſe obſervations he alſo applies to the images of luminous objects ſeen by reflexion, ſhewing that there will be ſeveral repetitions of the image, if the two ſurfaces of the glaſs mirror be inclined to one another; and that, in this caſe, alſo, the faint images will appear on that ſide of the principal image, on which they are inclined to another.

Not contenting himſelf with this curious theory, which gives ſo complete a ſolution of the phenomenon, he applies it to try the paralleliſm of the ſurfaces of poliſhed plates of glaſs, as by this means it may be done with incomparably more exactneſs than it can be meaſured by the niceſt calippers. The methods of trial that he recommends are the image of a candle in the dark, and that of a black thread in the light. If, in all poſitions of the eye and the glaſs, the image be ſingle, it may be concluded that the two ſurfaces are exactly parallel; but if two or more images appear, it may be concluded that the ſurfaces are not parallel. * Moreover, in turning the glaſs round, whenever the images of the thread coincide, the ſurfaces are parallel in the direction of the thread; and a perpendicular to that line will diſcover the place to which they are inclined.

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* This however will not be exactly true, unleſs the object be at an infinite diſtance.

The images will appear the fartheſt aſunder in the poſition of the glaſs which is perpendicular to that in which they coincided, and that ſide on which the weaker images appear may be concluded to be thinner than the other. The demonſtrations of theſe propoſitions, with a great variety of figures to explain them, may be ſeen in the memoirs of the French academy for 1699, p. 108, &c.

This excellent geometrician took a great deal of pains in demonſtrating, as he thought, that, ſuppoſing the denſity of the air be in proportion to its weight, the curve into which a ray of light will be bent by its refracting power, in its paſſage through it, will be a cycloid. If the ray be a tangent to the atmoſphere, the diameter of the generating circle of this cycloid, he ſays, will be the height of the atmoſphere; but that this diameter increaſes as the rays fall upon the atmoſphere with leſs obliquity, and that when they are perpendicular, the diameter of the generating circle will be infinite, that is, that the circle itſelf will degenerate into a right line; which agrees with the common obſervation, that a perpendicular ray ſuffers no refraction. This is upon the ſuppoſition that the ſurface of the atmoſphere is a plane; but ſince it is a curve, he obſerves that theſe cycloids will, in fact, be epicycloids. But this change makes no material alteration in their principal properties. *

Hermannus, however, has ſhewn that M. Dela Hire was miſtaken, for that this curve is infinitely extended, and has an aſymptote; † and Dr. Brook Taylor, in his *Methodus Incrementorum*, has ſhewn that this curve is one of the moſt perplexed, and intricate that can well be propoſed. §

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* Ac. Par. Mem. p. 76. † Acta Eruditorum, 1706, p. 257. § Phil. Tranſ. ab. Vol. 6, p. 156.

PERIOD

PERIOD V.

The discoveries of Sir Isaac Newton.

OF all the diſcoveries that have been made concerning the nature of light and colours, there are none that make any thing like ſo great a figure as thoſe of Sir Iſaac Newton. They are, perhaps, the moſt illuſtrious of all his own atchievements in the whole compaſs of philoſophy, for even his *Principia* itſelf has not been read with more admiration and applauſe than his *Opticks*. Indeed, the ſubject of theſe diſcoveries contributes greatly to throw a luſtre upon them; for what can be expected to engage the attention of mankind more than light, or the properties of that medium by means of which all the beauty and glory of the creation are laid open to us. In the opinion of Plato, to pry into the myſteries of light, was to encroach upon the prerogatives of divinity.

It cannot but contribute, alſo, to excite our attention to theſe diſcoveries, that they were made by a perſon who is univerſally acknowledged to have been the moſt eminent of mankind, for his profound ſkill, and various improvements in many branches of mathematicks and philoſophy, and that they were made by him in the prime of life, when his ſagacity was the quickeſt, and all his powers capable of their greateſt exertion. I believe I ſhall not be contradicted by any perſon who is acquainted with the hiſtory of philoſophy, if I add, that there is no example of any philoſophical inquiry conducted with more circumſpection, or in which the aid of mathematicks was applied with more advantage or addreſs; and no inquiry could be more ſucceſsful, or appear more extraordinary and ſurpriſing, when it was completed.

We even find that the principal of theſe optical diſcoveries appeared very extraordinary to the author himſelf, ſeven or eight years after they were made, at which time he firſt communicated an account of them to the Royal Society. This appears by a paragraph in a letter of his to Mr. Oldenburgh, Secretary to the Royal Society, dated Cambridge, Jan. 18, 1672, which, becauſe I think it will amuſe my reader, I ſhall here copy *verbatim.*

" I deſire that, in your next letter, you would inform me for
" what time the ſociety continue their weekly meetings; be-
" cauſe, if they continue them for any time, I am purpoſing
" them to be conſidered and examined, on account of a philoſo-
" phical diſcovery which induced me to the making of the ſaid
" teleſcope; and I doubt not but will prove much more grate-
" ful than the communication of that inſtrument, being, in my
" judgment, the oddeſt, if not the moſt conſiderable *detection,*
" which hath hitherto been made in the operations of nature.*

If this look like vanity in ſo great a man, it is however, the language of nature, and certainly very pardonable. How often do we ſee men give themſelves airs of the greateſt conſequence, on account of the moſt trifling diſcoveries; and we are glad to bear any thing of this nature, for the ſake of the ſmalleſt advantage to the cauſe of truth and ſcience.

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* Birch's Hiſtory, Vol. 3, p. 5.

Before the time of Newton we find no hypotheſis concerning colours of the leaſt conſequence, or indeed, that was held in any degree of eſteem among philoſophers. And even thoſe who oppoſed Newton advanced no other hypotheſis, on which they themſelves laid much ſtreſs. The opinions of the old philoſophers are not worth repeating in this place. I ſhall, however, juſt mention ſome of them, and bring ſome other diſtinguiſhed hypotheſes into one view, though moſt of them have been related before, as they may ſerve, by way of contraſt, to heighten our value for the capital advancement in real knowledge, which is the ſubject of this part of my work.

The Pythagoreans called colour the ſuperficies of body. Plato ſaid that it was a flame iſſuing from them. According to Zeno it is the firſt configuration of matter, and Ariſtotle ſaid it was that which moved bodies actually tranſparent. Deſcartes very ſenſibly argued that colour is a modification of light; but he ſuppoſed that the difference of colour, ariſes from the prevalence of the direct or rotatory motion of the particles of which it conſiſts. Father Grimaldi, Dechales, and many others thought the differences of colour depended upon the condenſation and rarefaction of light. Molebranche was of opinion, that differences in colour depend upon the quick or ſlow vibrations of a certain elaſtic medium filling the whole univerſe. Rohault imagined that the different colours were made by the rays of light entering the eye at different angles with reſpect to the optic axis; and from the phenomenon of the rainbow he pretended to calculate the preciſe angle that conſtituted each particular colour. Laſtly, Dr. Hooke, the rival of Newton, imagined, that colour is cauſed by the ſenſation of the oblique, or uneven pulſe of light; and this being capable of no more than two varieties, he concluded that there could be no more than two primary colours.

After theſe random conjectures, unſupported by a ſhadow of evidence, or ſo much as an attempt at experimental proof, with what ſatisfaction is it, that we, at length, come to the diſcovery of a theory of light and colours, ſtriking and beautiful in itſelf, drawn from the cleareſt and moſt deciſive experiments. This theory, therefore, with its proper evidence, I ſhall now proceed to lay before my reader, and I ſhall chuſe to begin with the author's own account of it, when he firſt communicated it to the Royal Society; as, by this means, we ſhall, in ſome meaſure, ſee the natural progreſs of his ideas, while he was conducting the inveſtigation.

SECTION I.

OF THE DIFFERENT REFRANGIBILITY OF THE RAYS OF LIGHT.

IN the year 1666, at which time Sir Iſaac Newton was buſy in grinding optic glaſſes, he procured a triangular glaſs priſm, to ſatisfy himſelf concerning the celebrated phenomena of colours. In order to this, having darkened his chamber, and made a ſmall hole in his window ſhutter, to let in a convenient quantity of the ſun's light; he placed his priſm at the entrance of it, that it might be refracted to the oppoſite wall. Things being diſpoſed in this manner, our philoſopher was, at firſt, agreeably amuſed with viewing the vivid and intenſe colours produced by the priſm; but, conſidering them with attention, he was more ſurpriſed to obſerve the oblong form of the image, which, according to the received laws of refraction, he expected would have been circular. For, F. Grimaldi's treatiſe being publiſhed only that very year, he was not acquainted, that the very ſame obſervation had been made before. The ſides of this oblong image were terminated by ſtraight lines, but at the ends the decay of light was ſo gradual, that he could not eaſily determine of what figure they were, though he thought they were ſemicircular.

Upon meaſuring this coloured ſpectrum, he found that its length was about 5 times greater than its breadth; which was ſuch a diſproportion, that it excited in him, as he ſays, a more than ordinary curioſity to examine from whence it might proceed. He could hardly think that any difference in the thickneſs, in the difference of the glaſs, or the termination of it by ſhadow, or darkneſs, could have any influence on light, to produce ſuch an effect. Not concluding any thing, however, *a priori*, he proceeded to examine theſe circumſtances; and particularly tried what would be the conſequence of tranſmitting the light through parts of the glaſs that were of different thickneſſes, or through holes in the window ſhutter of different ſizes; or by ſetting the priſm on the outſide of the ſhutter, that the light might paſs through it, and be refracted before it was terminated by the hole.

He then ſuſpected that theſe colours might ariſe from the light being dilated by ſome uneveneſs in the glaſs, or ſome other accidental irregularity; and to try this, he took another priſm, like the former, and placed it in ſuch a manner, as that the light, paſſing through them both, might be refracted contrary ways, and ſo be returned by the latter into the ſame courſe from which it had been diverted by the former. In this manner he thought that the regular effects of the firſt priſm would be deſtroyed by the ſecond; but that the irregular ones would be augmented by the multiplicity of refractions. The event was, that the light, which by the firſt priſm was diffuſed into an oblong form, was by the ſecond reduced into a *circular* one, with as much regularity as if it had not paſſed through either of them.

That this experiment may be better underſtood, let EG, fig. 69, repreſent the window ſhutter, and F the hole in it, through which the light is tranſmitted to the priſm ABC, which refracts the light towards PT, where it appears in an oblong form. In fig. 70, *agb* repreſents a ſecond priſm, which refracts the light back again to Q, where the image is round; whereas, without the interpoſition of this ſecond priſm, the light would have proceeded to PT, and conſequently have been oblong as before. In this experiment he took care that the plane *ag*, was placed exactly parallel to BC, and alſo *bg* to AC, that the rays might be equally refracted, though in contrary ways, by both priſms. He alſo obſerved, that the priſms muſt be placed very near to one another; for if their diſtance be ſo great, that colours begin to appear in the light, before its incidence on the ſecond priſm, thoſe colours will not be deſtroyed by the refraction of that priſm.

He then proceeded to examine more accurately what might be effected by the difference of the incidence of rays coming from different parts of the ſun's diſk; and for this purpoſe he meaſured all the lines and angles belonging to the image. Its diſtance from the hole or priſm was 22 feet, its greateſt length 13 $\frac{1}{4}$ inches, its breadth 2 $\frac{5}{8}$, and the diameter of the hole $\frac{1}{4}$ of an inch. The angle which the rays, tending towards the middle of the image, made with thoſe lines in which they would have proceeded without refraction was $44^\circ\ 56'$, and the vertical angle of the priſm $63^\circ\ 12'$. Alſo the angles of refraction on both ſides of the priſm, that is, of the incident and emerging rays, were, as near as he could make them, equal, and conſequently were $54^\circ\ 4''$, and the rays fell perpendicularly upon the wall. Subtracting the diameter of the hole from the length and breadth of the image, he found that there remained 13 inches in the length, and $2\frac{3}{8}$ in the breadth, comprehended by thoſe rays which paſſed through the center of the ſaid hole; and conſequently the angle, which that breadth ſubtended at the hole was about 31 minutes, anſwering to the ſun's diameter; but that the angle ſubtended by its length was more than five ſuch diameters, viz. $2^{\circ}.49'$

Having made theſe obſervations, he firſt computed from them the refractive power of the glaſs of his priſm, and found that it meaſured, by the ratio of the ſines, 20 to 31, and then by that ratio he computed the refractions of the two rays coming from oppoſite parts of the ſun's diſk, ſo as to differ 31 minutes in their obliquity of incidence, and found that the emerging rays ſhould have comprehended an angle of about 31 minutes, as they did before they were incident.

But becauſe this computation was founded on the hypotheſis of the proportionality of the ſines of incidence and refraction (though, by his own experience, he could not imagine it to be ſo erroneous as to make that angle but 31 minutes, which, in reality, was $2^{\circ}49'$) his curioſity induced him to take his priſm a ſecond time; and having placed it at the hole in the window ſhutter as before, he obſerved that, by turning it a little about its axis, ſo as not to vary its obliquity to the light more than by an angle of 4 or 5 degrees, the colours were not thereby ſenſibly removed from their place on the wall; ſo that the quantity of refraction was not ſenſibly changed by that variation of incidence. By this experiment, therefore, as well as by the former computation, it was evident to him, that the difference of the incidence of rays flowing from different parts of the ſun could not make them, after croſſing one another, to diverge at a ſenſibly greater angle, than that at which they before converged; which, being at moſt but about 31 or 32 minutes, there ſtill remained ſome other cauſe to make it 2°49′.

He then began to ſuſpect that the rays, after being tranſmitted through the priſm, might move in curve lines, and, according to their greater or leſs degree of curvature, tend to different parts of the wall; and his ſuſpicion was increaſed by remembring to have often ſeen a tennis ball, ſtruck with an oblique raquet, to deſcribe ſuch a curve. For a circular, as well as a progreſſive motion, being communicated to it by that ſtroke, its parts, on that ſide where the motions conſpire, muſt preſs, and beat the contiguous air more violently than on the other, and there excite a proportionably greater reaction of the air.* He therefore concluded, that if the rays of light ſhould be globular bodies, and by their oblique paſſage out of one medium into another, acquire a circular motion, they ought to meet with a greater reſiſtance from the ambient æther, on that ſide where the motions conſpire, and thence be continually bended to the other. But, notwithſtanding this plauſible ground of ſuſpicion, when he came to examine it, he could not find any ſuch curvature; and beſides, which was enough for his purpoſe, he obſerved, that the difference in the length of the image and the diameter of the hole through which the light was tranſmitted, was proportionable to their diſtance.

The gradual removal of theſe ſuſpicions led him at length to what he juſtly calls the *experimentum crucis*, which completed this great diſcovery. He took two boards, and placed one of them cloſe behind the priſm at the window, ſo that the light might paſs through a ſmall hole made in it for the purpoſe, and fall on the other board, which he placed at the diſtance of about 12 feet; having firſt made a ſmall hole in it alſo, for ſome of that incident light to paſs through. He then placed another priſm behind the ſecond board, ſo that the light which was tranſmitted through both the boards might paſs through that alſo, and be again refracted before it arrived at the wall. This being done, he took the firſt priſm in his hand, and turned it about its axis, ſo much as to make the ſeveral parts of the image, caſt on the ſecond board, ſucceſſively to paſs through the hole in it, that he might obſerve to what places on the wall the ſecond priſm would refract them; and he ſaw, by the change of thoſe places, that the light tending to that end of the image, towards which the refraction of the firſt priſm was made, did, in the ſecond priſm, ſuffer a refraction conſiderably greater than the light which tended to the other end. The true cauſe, therefore, of the length of that image was diſcovered to be no other, than that light is not ſimilar, or *homogeneal*, but that it conſiſts of rays, ſome of which are more refrangible than others; ſo that, without any difference in their incidence on the ſame medium, ſome of them ſhall be more refracted than others; and therefore, that, according to their particular degrees of refrangibility, they will be tranſmitted through the priſm to different parts of the oppoſite wall.*

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* With reſpect to this fact; our author ſeems to have been miſtaken.

To make this capital experiment the more intelligible, let SF, fig. 71, repreſent a ray of the ſun, which, after paſſing through a hole in the window ſhutter F, is received by the priſm ABC, cloſe behind which is placed a board DE, with a hole in it at G, to admit any of the rays after they had been ſeparated by the priſm. Then *de* will repreſent the other board, placed at a conſiderable diſtance from the former, with a hole in it *g*, to receive any part of the light tranſmitted through the other board. Behind this ſecond board is placed another priſm, *abc*, through which different rays of light, falling upon it in the very ſame place, and with preciſely the ſame angle of incidence, will be refracted higher or lower, on the oppoſite wall MN.

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* Phil. Tranſ. ab. Vol. 1, p. 128.

That we may have ſome idea of the extreme circumſpection of this philoſopher, in the care he took to adjuſt his priſms, and other circumſtances of the foregoing, and alſo the following experiments, I ſhall recite ſome of the particulars, as they are deſcribed by himſelf in his treatiſe on Opticks. The axis of his priſms was always placed perpendicularly to the incident rays. About this axis he turned the priſm ſlowly, ſo as to ſee the coloured image of the ſun upon the wall firſt to deſcend and then to aſcend ; and between the deſcent and aſcent, when the image ſeemed ſtationary, he fixed the priſm, that it might move no more. For, in that poſition of the priſm, the refractions of the light at both its ſides are equal to one another. The priſm being thus fixed, he let the refracted light fall perpendicularly upon a ſheet of white paper at the oppoſite wall of the chamber. If the priſm was turned about its axis that way which made the rays emerge more obliquely out of the ſecond refracting ſurface of the priſm, the image ſoon became an inch or two longer or more ; and if the priſm was turned about the contrary way, ſo as to make the rays fall more obliquely on the firſt refracting ſurface, the image ſoon became an inch or two ſhorter.

The firſt priſm that he made uſe of had ſome veins running along within the glaſs, from one end to the other, which ſcattered ſome of the light irregularly, but had no ſenſible effect in increaſing the length of the coloured image; for he tried the ſame experiment with other priſms, with the ſame ſucceſs, and particularly with one which ſeemed free from ſuch veins, and whoſe refracting angle was 62 ½ degrees. With this priſm the length of the image was 9 ¾ or 10 inches, at the diſtance of 18 ½ feet from the priſm, the breadth of the hole in the window ſhutter being ¼ of an inch, as before. And becauſe it was eaſy to make a miſtake in placing the priſm, he repeated the experiment four or five times, and always found the length of the image the ſame as above.

With another priſm of clearer glaſs, and of better poliſh, which ſeemed free from veins, and whoſe refracting angle was 63 ½ degrees, the length of the image, at the ſame diſtance of 18 ½ feet, was alſo about 10 inches, or 10 ⅛. Beyond theſe meaſures, for about ¼ or ⅓ of an inch at either end of the image, the light of the clouds ſeemed to be tinged with red and violet, but ſo very faintly, that he ſuſpected the tinge might ariſe from ſome rays of the image being ſcattered irregularly, by inequalities in the ſubſtance and poliſh of the glaſs, and therefore he did not include it in thoſe meaſures.

Neither the different magnitude of the hole in the window ſhutter, nor the different thickneſſes of the priſm, at the place where the rays paſſed through it, nor the different inclinations of the priſm to the horizon made any ſenſible change in the length of the image. Neither did the different matter of the priſms make any; for in a veſſel made of poliſhed plates of glaſs, cemented together in the ſhape of a priſm, and filled with water, the experiments ſucceeded as before, according to the quantity of refraction.*

Though the truth of this famous hypotheſis, of the different refrangibility of the rays of light, be ſufficiently evident from the foregoing deciſive experiments, it cannot but give pleaſure to my reader, to ſee an account of ſome of the moſt ſtriking of the ſubſequent experiments which our author publiſhed in his treatiſe on Opticks, as a farther confirmation of the ſame concluſion.

In a beam of the ſun, which was tranſmitted into the room through a hole in the window ſhutter, at the diſtance of ſome feet from the hole, uſing the ſame precautions as are mentioned above, he looked at the hole through the priſm, and obſerved the length of its refracted image to be many times greater than its breadth. The moſt refracted part of it was violet, the leaſt refracted red, the middle parts blue, green, and yellow, in order. The ſame thing happened when he removed the priſm out of the ſun's light, and looked through it upon the hole ſhining by the light of the clouds beyond it. †

Conſidering that, if the image of the ſun ſhould be drawn into an oblong form, either by a dilatation of every ray, or by any other caſual inequality of the refractions, the ſame oblong image would, by a ſecond refraction, made ſide ways, be drawn out as much in breadth, he placed a ſecond priſm immediately after the firſt, in an oblique poſition with reſpect to it, that it might again refract the light of the beam of the ſun's light, which came to it through the firſt priſm; ſo that in the firſt priſm, the beam would be refracted upwards, and in the ſecond ſideways. But he found that the breadth of the image was not increaſed by the refraction of the ſecond priſm, but only its upper part, which, in the firſt priſm ſuffered the greateſt refraction, and appeared violet and blue, did again, in the ſecond priſm, ſuffer a greater refraction than the lower part of it, which was red and yellow, and this without any dilatation of the breadth of the image.

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* Newton's Opticks, p. 22. † Ib. p. 27.

Thus let S, fig. 72, repreſent the ſun, F the hole in the window, ABC the firſt priſm, and DH the ſecond. If Y repreſent the round image of the ſun, made by a direct beam of light, when the priſms are taken away, PT will be the oblong image of the ſun, made by the ſame beam paſſing through the firſt priſm only, and *p t* will be the image made by the croſs refractions of both priſms together.*

Sometimes he placed a third priſm after the ſecond, and ſometimes a fourth after the third; by all which the image might be often refracted ſideways, but the rays which were more refracted than the reſt in the firſt priſm, were alſo more refracted in all the others, and that without any dilatation of the image ſideways; and therefore thoſe rays, for their conſtancy of a greater refraction, he juſtly called the *more refrangible* ones.

That the meaning of this experiment may be more evident, our author obſerves, that all the rays which are equally refrangible, fall upon a circle anſwering to the ſun's diſk. Let therefore AG, fig. 73, repreſent the circle which all the moſt refrangible rays, tranſmitted from the whole diſk of the ſun, would illuminate, and paint upon the oppoſite wall, if they were alone. Let EL be the circle which all the leaſt refrangible rays would, in like manner, illuminate, and paint, if they were alone, and let BH, CJ, and DK be the circles which ſo many intermediate kinds of rays would ſucceſſively paint upon the wall, if they were ſingly propagated from the ſun, the reſt being always intercepted, and conceive that there are other intermediate circles without number, which other innumerable intermediate kinds of rays would ſucceſſively paint upon the wall, if the ſun ſhould ſucceſſively emit every kind by itſelf. Now ſince the ſun emits rays of all theſe kinds at once, they muſt altogether illuminate and paint innumerable equal circles, of all which, being ranged according to their different degrees of refrangibility, the oblong image PT before deſcribed is compoſed.

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* Newton's Opticks, p. 28.

Now if the ſun's circular image, Y, which is made by an unrefracted beam of light, was by any dilatation of the ſingle rays, or by any other irregularity in the refraction of the firſt priſm, converted into the oblong image PT, then ought every circle in the image to be in like manner drawn out into a ſimilar oblong figure, contrary to the reſult of this experiment.

He conſidered farther, that by the breadth of the hole through which the light enters into the dark chamber, there is a penumbra made in the circumference of the image Y, which is alſo viſible at the ſides of the oblong images PT and *pt*. He therefore, placed at that hole a lens, or object-glaſs of a teleſcope, which might caſt the image of the ſun diſtinctly on Y, without any penumbra at all; and he found that the penumbra of the rectilinear ſides of the oblong images PT and *pt* was alſo thereby taken away, ſo that they were as diſtinctly defined as the circumference of the firſt image Y.

There are ſome other circumſtances attending this experiment, by which the concluſion drawn from it is made ſtill more plain and convincing. Let the ſecond priſm, DH, fig. 74, be placed not immediately after the firſt, but at ſome diſtance from it, ſo that the light from the firſt priſm may fall upon it in the form of an oblong *ſpectrum* $q\ r$, parallel to this ſecond priſm, and may be refracted ſideways, to form the oblong image $p\ t$, upon the wall, and it will be found that this image $p\ t$ is inclined to the image PT, which the firſt priſm would have formed without the ſecond; the blue ends, P and p, being farther diſtant from one another than the red ones, T and t; and conſequently the rays which go to the blue end q, of the image $q\ r$, and which, therefore, ſuffer the greateſt refraction in the firſt priſm, are again, in the ſecond priſm, more refracted than the reſt.*

At two holes made near one another in his window ſhutter he placed two priſms, one at each, which might caſt upon the oppoſite wall two oblong coloured images of the ſun; and at a little diſtance from the wall he placed a long ſlender paper, with ſtraight and parallel edges, and he placed the priſms and papers, ſo that the red colour of one image, at T, fig. 75, might fall directly upon one half of the paper, and the violet colour M, of the other image, upon the other half of the ſame paper. Then with a black cloth he covered the wall behind the paper, that no light might be reflected from it to diſturb the experiment; and viewing the paper through a third priſm, held parallel to it, he ſaw that half of it which was illuminated by the violet light to be divided from the other half, by a greater refraction, eſpecially when he retired to a conſiderable diſtance from the paper.

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* Newton's Opticks, p. 35.

He farther cauſed the two images PT and MN, fig. 76, to coincide, in an inverted order of their colours, the red end of each falling on the violet end of the other; and then viewing them through a priſm DH, held parallel to their length, they no longer appeared coincident, as when they were viewed with the naked eye, but in the form of two diſtinct images, *p t* and *m n*, croſſing one another in the middle; which ſhews that the red of the one image, and the violet of the other, which were coincident at PN and MT, being parted from one another by a greater refraction of the violet to *p m*, than that of the red to *n* and *t*, differ in degree of refrangibility.*

Having placed a priſm, whoſe two angles at its baſe were equal to one another, and half right ones, and the third a right one, in a beam of the ſun's light, admitted into the room as before, he turned it ſlowly about its axis, till all the light which went through one of its angles, and was refracted by it, began to be reflected by its baſe (at which, till then, it went out of the glaſs) and then he obſerved that thoſe rays which had ſuffered the greateſt refraction were ſooner reflected than the reſt. He imagined, therefore, that thoſe rays of the reflected light, which were moſt refrangible, did firſt of all, by a total reflexion, become more copious in that light than the reſt; and that afterwards, the reſt, alſo, by a total reflexion, became as copious as theſe. To try this, he made the reflected light paſs through another priſm, and, being refracted by it, to fall afterwards upon a ſheet of white paper, at ſome diſtance behind it, and there to paint the uſual colours of the priſm. Then cauſing the firſt priſm to be turned about its axis, he obſerved, that when thoſe rays which, in this priſm, had ſuffered the greateſt refraction, and were of a blue and violet colour, began to be wholly reflected, the blue and violet light on the paper, which received the rays from the ſecond priſm, was ſenſibly increaſed, above that of the red and yellow, which was leaſt refracted; and afterwards, when the reſt of the light, which was green, yellow, and red, began to be wholly reflected in the firſt priſm, the light of thoſe colours on the paper received as great an increaſe as the violet and blue had done before. From this it is manifeſt that the beam reflected by the baſe of the priſm, being augmented firſt by the more refrangible rays, and afterwards by the leſs refrangible ones, is compounded of rays differently refrangible. This experiment is illuſtrated by fig. 77, in which ABC repreſents the firſt priſm, on the baſe of which the light falls, at M. When this priſm is turned about its axis, according to the order of the letters ABC, the more refrangible rays MH emerge more and more obliquely; and at length, after their moſt oblique emergence, are reflected towards N, and going on to *p* increaſe the number of rays N*p*. By continuing the motion of the firſt priſm, the leſs refrangible rays MG are reflected to N, and increaſe the number of rays N *t*.

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* Newton's Opticks, p. 39.

Since it appears from Sir Iſaac Newton's experiments, that different rays of light have different degrees of refrangibility, it neceſſarily follows that the rules laid down by preceding philoſophers concerning the refractive power of water, glaſs, &c. muſt be limited to the middle kind of rays, as it may be ſuppoſed that Kepler, Snellius, and others would attend to them principally. Sir Iſaac, however, proves that the ſine of the incidence of every kind of light, conſidered apart, is to its ſine of refraction in a given ratio. This he deduces both by experiment, and alſo geometrically, from the ſuppoſition that bodies refract the light by acting upon its rays in lines perpendicular to their ſurfaces.*

SECTION II.

Of the nature of COLOURS.

AT the ſame time that this excellent philoſopher communicated to the Royal Society his theory of the different refrangibily of the rays of light, he gave them his thoughts concerning *colours*, which neceſſarily reſulted from it, in ſo much that they could hardly be called two different diſcoveries. His obſervations on this ſubject he compriſed in a number of propoſitions, the more important of which are the following.

1. As the rays of light differ in refrangibility, ſo they alſo differ in their diſpoſition to exhibit this or that particular colour. Colours are not modifications of light, derived from refractions or reflexions of natural bodies, as it is generally believed, but original and connate properties, which are different in different rays. Some rays are diſpoſed to exhibit a red colour, and no other, ſome a yellow and no other, ſome a green and no other, and ſo of the reſt. Nor are there only rays proper to the more eminent colours, but even to all their intermediate gradations.

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* Newton's Opticks, p. 68.

2. To the ſame degree of refrangibility always belongs the ſame colour, and to the ſame colour the ſame degree of refrangibility.

3. The ſame ſpecies of colour, and degree of refrangibility, proper to any particular kind of rays, was not changeable by refraction and by reflexion of natural bodies, nor by any other cauſe that he could obſerve. When any one ſort of rays had been well ſeparated from thoſe of other kinds, they afterwards obſtinately retained their colour, notwithſtanding his utmoſt endeavours to change it. He refracted it with priſms, and reflected it with bodies, which in day-light were of other colours. He intercepted it with the coloured film of air, intervening between two compreſſed plates of glaſs. He tranſmitted it through coloured mediums, and through mediums irradiated with other kinds of rays, and he terminated it in a variety of ways; but he could never produce any new colour out of it. It would, by contracting, or dilating, become more briſk or faint, and by the loſs of many rays would, in ſome caſes, be very dark and obſcure, but he could never ſee it change in *ſpecie.*

4. A ſeeming tranſmutation of colours may be made by a mixture of different kinds of rays, but in ſuch mixtures the component colours themſelves do not appear; but, by their mutually allaying each other, conſtitute a middle colour; and therefore if, by refraction, the different rays be ſeparated, colours will emerge different from that of the compoſition. Thus blue and yellow powders finely mixed, appear green to the naked eye, and yet the colours of the component particles are not thereby really changed, but only blended; for when they are viewed with a microſcope, they ſtill appear blue and yellow.

5. There are alſo two ſorts of colours, the one original and ſimple, the other compounded of theſe. The original or primary colours are red, orange, yellow, green, blue, indigo, and a violet purple, and an indefinite variety of intermediate gradations.

6. The ſame colours in *ſpecie* with theſe primary ones may alſo be produced by a compoſition. Thus a mixture of yellow and blue makes green, of red and yellow makes orange, and a mixture of orange and yellowiſh green makes yellow. And in general, if any two colours be mixed, which, in the ſeries of thoſe that are produced by the priſm, are not too far diſtant from one another, they, by their mutual alloy, compoſe that colour which appears in the midway between them; but thoſe which are ſituated at too great diſtance have not this effect. Thus orange and indigo do not produce the intermediate green, nor ſcarlet and green the intermediate yellow.

7. The moſt ſurpriſing and wonderful compoſition is that of *whiteneſs*. There is no one ſort of rays which alone can exhibit this. It is always compounded, and all the aforeſaid primary colours, mixed in a certain proportion, are requiſite to form it. He often, with admiration, obſerved that all the colours of the priſm, being made to converge, and thereby to be again mixed, as they were in the light before it was incident on the priſm, reproduced light intirely and perfectly white, ſo as not ſenſibly to differ from the direct light of the ſun, except when the glaſſes he uſed were not ſufficiently clean; for then they were a little inclined to their colour.

To examine this theory, he propoſed the following experiment. In a darkened room make a hole in the window ſhutter, whoſe diameter may be about $\frac{1}{3}$ part of an inch, in order to admit a convenient quantity of the ſun's light, and there place a clear and colourleſs priſm, to refract the light towards the farther part of the room. Then place a lens, of about 3 feet radius, as the broad object glaſs of a 3 foot teleſcope, at the diſtance of about 4 or 5 feet from it, through which all thoſe colours may at once be tranſmitted, and be made to meet, at the diſtance of about 10 or 12 feet farther. If, ſays he, at that diſtance, you intercept this light, with a ſheet of white paper, you will ſee the colours converted into whiteneſs again by being mingled. But it is requiſite that the priſm and lens be placed ſteady, and that the paper on which the colours are caſt be moved to and fro; for by ſuch motion you will not only find at what diſtance the whiteneſs is moſt perfect, but alſo ſee how the colours gradually meet and vaniſh into whiteneſs; and afterwards, having croſſed one another in the place of perfect whiteneſs, are again ſeparated, and, in an inverted order, retain the ſame colours which they had before they were fixed in the focus of the lens. You may alſo ſee that, if any of the colours of the lens be intercepted, the whiteneſs will be changed into other colours. In order, therefore, that the white be as perfect as poſſible, care muſt be taken that none of the colours fall beſides the lens.

To make this experiment the more intelligible, let ABC, fig. 78, repreſent the priſm, and MN the lens; then FP and FR will be two rays unequally refracted, and which, after their ſeparation, the lens makes to converge to Q, and after croſſing one another to diverge again; and HI is the paper on which the colours are received.

From this our author concludes, that *whiteneſs* which is the uſual colour of light, is a confuſed aggregate of rays, endued with all ſorts of colours, as they are promiſcuouſly darted from the various parts of luminous bodies. Of this confuſed aggregate whiteneſs will be produced, if there be a due proportion of the ingredients; but if any one predominate, the light will incline to that colour, as it happens in the blue flame of brimſtone, the yellow flame of a candle, and the various colours of the fixed ſtars.

If we would try the impoſſibility of changing any one compounding colour, he ſays it is requiſite that the room be made very dark, leſt any ſcattered light, mixing with the colour, render it compound. It is requiſite, alſo, he ſays, that there be a more perfect ſeparation of the colours than can be made by a ſingle priſm, but that if the trial be made with colours not perfectly ſeparated, allowance muſt be made for the change of colour proportioned to the mixture.*

That the coloured light of the ſun conſiſts of rays differently refrangible is abundantly manifeſt from many of the experiments in the former ſection; that the colours of natural bodies are produced by rays which have the ſame differences with reſpect to refrangibility might be concluded, ſince it is evident, from ſeveral of the ſame experiments, that natural bodies appear coloured by reflecting the coloured rays of the ſun. Our author, however, contrived the following ingenious experiments, which are well worth reciting, in order to prove his propoſition with reſpect to the colour of natural bodies directly.

He took a black oblong ſtiff paper, terminated by parallel ſides, and with a perpendicular right line, drawn croſs it from one ſide to the other, divided it into two equal parts. One of theſe he painted with red colour, and the other with blue, and viewing it through a priſm, he found that, if the refractive angle of the priſm was turned upwards, the blue half was lifted higher by the refraction than the red; but if the refracting angle was turned downwards, the blue half was carried lower than the red; ſo that, in both caſes, the light which came from the blue half of the paper, through the priſm into the eye, ſuffered a greater refraction than the light which came from the red half.

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* Phil. Tranſ. ab. Vol. 1, p. 113.

About the ſame paper he wrapped ſlender threads of black ſilk, and when it was ſtrongly illuminated by a candle, he placed, at the diſtance of ſix feet from the paper, a glaſs lens, 4 inches broad, to collect the rays coming from the ſeveral points of the paper, and make them converge to ſo many other points, at the ſame diſtance on the other ſide of the lens, and ſo form an image of the coloured paper upon a white paper placed there. Then, moving his white paper backwards and forwards, and carefully noting the places where the images of the red and blue halves of the coloured paper agpeared moſt diſtinct, he found that where the red half of the paper appeared diſtinct, the blue half appeared confuſed, ſo that the black lines drawn upon it could ſcarcely be ſeen; and, on the contrary, where the blue half appeared moſt diſtinct, the red half appeared confuſed; and between the two places where theſe images appeared diſtinct there was a diſtance of an inch and a half, the focus of the blue rays being ſo much nearer than thoſe of the red, in conſequence of being the more refrangible of the two.

Both theſe colours, however, conſiſted of rays differently refrangible; but being few in proportion to the reſt, they, ſerved, as he ſays, to diminiſh the event of this experiment, but were not able to deſtroy it. For if the red and blue colours were more dilute, the diſtance of the images would be leſs than an inch and a half; but if they were more intenſe, the diſtance would be greater.*

Having ſhewn, in a former ſection, the extremes of the different degrees of refrangibility in the different kinds of light, I ſhall now proceed to give the reſult of this accurate philoſopher's inveſtigation, concerning the different degrees of refrangibility of all theſe different kinds of light, according to their ſeveral colours; but I muſt firſt explain the method he made uſe of to define the boundaries of each colour, in the oblong image of the ſun, more accurately than I have hitherto deſcribed it to have been done by him. In the image that has been already deſcribed, notwithſtanding there was a manifeſt difference of colour, not only between the two extremes, but alſo in the intermediate parts, yet the exact place at which any one colour ended, and another began, was far from being ſufficiently diſtinguiſhable. The reaſon of this indiſtinctneſs is, that rays of every kind coming from all parts of the ſun's diſk, an intire image of the ſun is projected on the paper, conſiſting of a circle of each particular colour; and as the rays differ in kind by infiniteſimal degrees, from the extreme red to the extreme violet, there muſt, in fact, be thouſands of theſe circles in the ſame oblong image, the centers of which are infinitely near to one another; ſo that the light is intimately mixed, eſpecially in the middle of the image, where it is the brighteſt.

If theſe circles, as he obſerves, whilſt their centers keep their diſtances and poſitions, could be made leſs in diameter, their interfering one with another, and conſequently the mixture of heterogenious rays, would be proportionably diminiſhed. Thus, in PT, fig. 79, the circles of which the ſolar image conſiſts, expand into one another; but in the ſame fig. *p t*, being compoſed of leſs circles, but having their diameters at the ſame diſtance as the former, do not extend into one another, the mixture being diminiſhed in proportion to the diameters of the circles.

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* Newton's Opticks, p. 16,

Now theſe circles would be diminiſhed, if, without the room, at a great diſtance from the priſm, towards the ſun, ſome opaque body was placed, having a round hole in the middle of it, to intercept all the ſun's light, excepting ſo much as, coming from the middle of its diſk, could paſs through that hole to the priſm; for ſo the ſeparate circles would no longer anſwer to the whole diſk of the ſun, but only to that part of it which can be ſeen from the priſm, through that hole. But that theſe circles may anſwer more diſtinctly to the hole, a lens is to be placed by the priſm, to caſt the image of the hole, that is, of each ſeparate circle, diſtinctly upon the paper; and if this be done, it will not be neceſſary to place that hole very far off, not even beyond the window. Inſtead, therefore, of that hole, he made uſe of the hole in his window ſhutter, in the following manner.

At about 10 or 12 feet from the window, he placed a lens, by which the image of the hole might be diſtinctly caſt upon a ſheet of white paper, at the diſtance of 6, 8, 10, or 12 feet from the lens. Then, immediately after the lens, he placed a priſm, by which the refracted light might be thrown upwards, or ſideways; and he moved the paper that received it, either towards the priſm, or from it, till he found the exact diſtance at which the ſides of the image appeared moſt diſtinct. By this means the circular images of the hole were terminated moſt diſtinctly, without any penumbra, and therefore extended into one another the leaſt that they could; and conſequently the mixture of the heterogeneous rays was the leaſt of all. And by uſing a greater or leſs hole in the window ſhutter, he made the circular images greater or leſs at pleaſure, and thereby the mixture of rays in the oblong image was as much or as little as he choſe. He ſometimes made the breadth of the image 40 times, and ſometimes 60 or 70 times leſs than its length. In this manner, he ſays, light is made ſufficiently ſimple, and homogeneal, for trying any of his experiments about ſimple light; for that the heterogeneous rays in this light are ſo few, as hardly to be perceived, excepting perhaps, in the indigo and violet, which, being dark colours, do eaſily ſuffer a ſenſible allay by that little ſcattering light which uſed to be refracted irregularly by the inequalities of the priſm. The whole proceſs of this experiment is ſo evident, by inſpection of fig. 80, that it needs no particular illuſtration.

Inſtead of a circular hole, our author recommends a hole ſhaped like a long parallelogram, with its length parallel to the priſm. For if this hole be an inch or two long, and but a 10th or 20th part of an inch broad, or narrower, the light of the image will be as ſimple as before, or more ſimple, and the image will become much broader, and therefore more fit for theſe experiments.

Or, inſtead of this hole, another may be formed of a triangle of equal ſides, whoſe baſe may be about the 10th part of an inch, and its height an inch or more. For, by this means, if the axis of the priſm be parallel to the perpendicular of the triangle, the image *p t* fig. 81, will now be formed of equicrural triangles *ag*, *b h*, *c i*, *d k*, *e l*, *f m*, &c. and innumerable other intermediate ones, anſwering to the triangular hole in ſhape and bigneſs, and lying one after another in a continual ſeries, between two parallel lines *af*, *g m*. Theſe triangles are a little intermingled at their baſes, but not at their vertices; and therefore the light on the brighter ſide, *a f*, of the image, where the baſes of the triangles are, is a little compounded, but on the darker ſide, *g m*, it is altogether uncompounded; and in all places between the ſides, the compoſition is proportionable to the diſtances of the places from that obſcurer ſide *g m*; and having an image of ſuch a compoſition, we may try experiments either in its ſtronger and leſs ſimple light, near the ſide *a f*; or in its weaker and more ſimple light, near the other ſide *g m*, as ſhall ſeem moſt convenient.

In making theſe experiments, he adviſes, that the chamber be made very dark, that the lens be very good, being made of glaſs free from bubbles and veins, its ſides truly plane, and its poliſh elaborate, with an angle of about 65 or 70 degrees; and the edges of the priſm and lens, as far as they make any irregular refraction, ſhould be covered with black paper glued on them. He alſo obſerves, that all the uſeleſs light ſhould be intercepted, with black paper or other black obſtacles. It being difficult to get glaſs priſms fit for theſe nice experiments, he ſometimes uſed priſmatick veſſels made with pieces of broken looking glaſſes, and filled with rain water; and to increaſe the refraction, he ſometimes ſtrongly impregnated the water with *ſaccharum ſaturni*.*

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* Newton's Opticks, p. 61.

When he had, by this means, got the ſides of the coloured image, as AF, GM, fig. 82, diſtinctly defined, he delineated on paper the out lines of it FAPGMT, and held the paper ſo that the image might fall on this figure, and coincide with it exactly; whilſt an aſſiſtant, whoſe eyes could diſtinguiſh colours better than his own, did, by right lines drawn croſs the image, mark the confines of each colour; and this operation being frequently repeated, both on the ſame and on different papers, he found that the obſervations agreed well enough with one another, and that the ſides MG and FA were by this means divided like a muſical chord; ſo that if GM were produced to X, making MX equal to GM, and if GX, *l*X, *i*X, *b*X, *e*X, *g*X, *a*X, MX, were in proportion to one another, as the numbers 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, and $\frac{1}{2}$, and ſo repreſented the chords of the key, and of a tone, a third minor, a fourth, a fifth, a ſixth major, a ſeventh, and an eighth above that key. And the intervals M*a*, *a g*, *g e*, *e b*, *b i*, *i l*, and *l* G, were the ſpaces which the ſeveral colours red, orange, yellow, green, blue, indigo, and violet took up.

Now theſe intervals or ſpaces, ſubtending the differences of the refractions of the rays, going to the limit of thoſe colours, that is to the points M, *a*, *g*, *e*, *b*, *i*, *l*, G, may, without any ſenſible error, be accounted proportional to the differences of the ſines of refraction of thoſe rays, having one common ſine of incidence; and therefore, ſince the common ſine of incidence of the moſt and leaſt refrangible rays, out of glaſs into air, was found in proportion to their ſines of refraction as 50 to 77 and 78; if the difference between 77 and 78 be divided as the line GM is divided by thoſe intervals, there will be 77, 77$\frac{1}{8}$, 77$\frac{1}{5}$, 77$\frac{1}{3}$, 77$\frac{1}{2}$, 77$\frac{2}{3}$, 77$\frac{7}{9}$, 78 for the ſines of refraction of thoſe rays out of glaſs into air, their common ſine of incidence being 50. So then the ſines of the incidences of all the red making rays out of glaſs into air were to the ſines of their refractions not greater than 50 to 77, nor leſs than 50 to $77\frac{1}{8}$; but they varied from one another according to all their intermediate proportions, and ſo of the other colours. *

Having demonſtrated that the light of the ſun conſiſts of a certain proportion of differently coloured light, our author proves, by another ſeries of experiments, what has been already demonſtrated by a ſingle one mentioned above, that when a beam of light has been divided into its component parts; if they be again mixed, they will produce white, or if any one of them be intercepted, the image will appear tinged, and in a different manner, according to the different colours that are thereby prevented from mixing with the reſt; and to complete the whole, he obſerves, that intercepting all the colours that compoſe the white image except one, and thereby making it to exhibit the appearance of all the colours in order; yet, if he made this ſucceſſion of all the colours very quick, the appearance was always white, though it was demonſtrable that only one colour took place at any one time; and he juſtly obſerves, that if each of theſe colours in ſucceſſion give the idea of whiteneſs, much more will they produce that effect, when they are ſo intimately mixed as they are in a natural ſun beam.

Not content with compoſing whiteneſs from the ſeparately coloured rays of the ſun, he attempted, and ſucceeded in his attempt, to do the ſame with natural coloured bodies, obſerving the ſame proportions of the reſpective colours that he had found

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* Newton's Opticks, p. 110, &c.

in the ſolar image. The coloured powders which he made uſe of at firſt produced only a kind of grey, but this was in fact a dull white, or whiteneſs mixed with ſhade; for when he contrived to throw a very ſtrong light upon it, it became intenſely white, ſo that a friend of his, who happened to call upon him while he was buſy about theſe curious experiments, and who knew nothing of what he had been doing, pronounced that the powders he had been mixing, when thus illuminated, made a better white than ſome very fine white paper, with which he was comparing it.*

As the preceding proportion of all the priſmatic colours makes a white, it is evident that when they are mixed in different proportions, or when only a few of them are uſed, they will make different colours; and our philoſopher has given us the following ingenious method of knowing, in a mixture of primary colours, the quantity and quality of each being given, the colour of the compound. With the center O, fig. 83, and the radius OD, deſcribe a circle ADF, and divide the circumference into 7 parts, proportional to the 7 muſical tones or intervals, contained in an octave, that is in proportion to the numbers $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{10}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{16}$, $\frac{1}{9}$. Let the firſt DE repreſent a red colour, the ſecond EF orange, and ſo of the reſt; and let all theſe colours be ſuppoſed to paſs gradually into one another. Let p be the center of gravity of the arch DE, and q, r, s, t, u, x, be the centers of gravity of the other arches; and about thoſe centers let circles, proportional to the number of rays of each colour in the given mixture be deſcribed. Then find a common center of gravity of all thoſe circles; and if a line be drawn from the center of the circle O, through this point, as ſuppoſe at z, the point Y, in which it terminates at the circumference, will ſhew the colour that ariſes from the mixture, and the line OZ will be proportioned to the fulneſs or intenſeneſs of the colour, the center O repreſenting perfect white. But if only two of the primary colours, which in this circle are oppoſite to one another, be mixed in an equal proportion, the point Z will fall upon the center O, and yet the colour compounded of thoſe two will not be perfectly white, but ſome faint anonymous colour; for he could never, by mixing only two primary colours, produce a perfect white. Whether it might be the reſult of three taken at equal diſtances in the circumference he could not tell; but he did not much queſtion, but that 4 or 5 of them would be ſufficient. Theſe, however, as he obſerves, are curioſities of little or no moment to the underſtanding of the phenomena of nature, ſince, in all natural whites, there is a mixture of all kinds of rays.*

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* Newton's Opticks, p. 133.

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* Newton's Opticks, p. 134, &c.

SECTION III.

Several appearances explained by the preceding discoveries.

SIR Iſaac Newton, having inveſtigated the true theory of light and colours, in a ſpecimen of the moſt perfect analyſis that was ever exhibited, and which is juſtly conſidered as the beſt model for all future inquiries into the powers of nature, proceeds ſynthetically to explain ſeveral phenomena by the help of it.

In the firſt place, he ſhews the reaſon why, upon looking at any object through a priſm, the edges only appear tinged with certain colours, and in a certain order. Let ABC, in fig. 84, repreſent a priſm, refracting the light of the ſun which comes into a dark chamber, through a hole F*f*, almoſt as broad as the priſm, and let MN repreſent a white paper, on which the refracted light is caſt, and ſuppoſe the moſt refrangible, or deepeſt violet rays fall upon the ſpace P*p*, the leaſt refrangible, or deepeſt rays upon the ſpace T*t*, the middle kind between the indigo and blue rays upon the ſpace QX, the middle kind of the green rays upon the ſpace R*r*, the middle kind between the yellow and orange rays upon the ſpace S*s*, and other intermediate kinds upon intermediate ſpaces. For ſo the ſpaces upon which the ſeveral kinds of rays adequately fall will, by reaſon of their different refrangibility, be one lower than another. Now if the paper MN be ſo near the priſm, that the ſpaces PT and *p t* do not interfere with one another, the diſtance between them T*p* will be illuminated by all the kinds of rays, and in the ſame proportion to one another that they had at their firſt coming out of the priſm, and conſequently be white. But the ſpaces above and below this will not be illuminated by them all, and therefore will appear coloured, and variouſly, according to their different rays.

The ſun's light, however, he obſerves, is not perfectly white, but inclines to yellow, and therefore the exceſs of the yellow rays being mixed with the faint blue, between S and T, will draw it to a faint green, and ſo the colours in order from P to *t* ought to be violet, indigo, blue, very faint green, white, faint yellow, orange, and red. Thus, ſays he, it is by computation, and they who pleaſe to view the colours made by a priſm will find it ſo in fact.

Theſe are the colours on both ſides the white, when the paper is held between the priſm and the point X, where the colours meet, and the interjacent white vaniſhes; for if the paper be held ſtill farther off from the priſm, the moſt refrangible and leaſt refrangible rays will be wanting in the middle of the light, and the reſt of the rays which are found there will, by mixture, produce a fuller green than before. Alſo the yellow and blue will now become leſs compounded, and conſequently more intenſe than before, which alſo agrees with experience.

If a perſon look through a priſm on a white object, ſurrounded with blackneſs or darkneſs, the reaſon of the colours ariſing on the edges is much the ſame; as will appear to any perſon who will a little conſider it. If a black object be ſurrounded with a white one, the colours which appear through the priſm are to be derived from the light of the illuminated one, ſpreading into the regions of the black; and therefore they appear in a contrary order to that in which they are ſeen, when a white object is ſurrounded with a black one. The ſame is to be underſtood when an object is viewed, the parts of which are leſs luminous than others; for in the borders of the more and leſs luminous parts, colours ought always, on the ſame principle, to ariſe, viz. from the exceſs of the light of the more luminous object, and to be of the ſame kind as if the darker parts were black, but yet to be more faint and dilate.

What is ſaid of colours made by priſms may be eaſily applied to colours made by the glaſſes of teleſcopes and microſcopes, or by the humours of the eye. For if the object glaſs of a teleſcope be thicker on one ſide than on the other, or if one half of the glaſs, or one half of the pupil of the eye, be covered with any opaque ſubſtance, the object glaſs, or that part of it, or of the eye, which is not covered, may be conſidered as a wedge with crooked ſides; and every wedge of glaſs or other pellucid ſubſtance, has the effect of a priſm, in refracting the light which paſſes through it.*

There is another phenomenon that occurs in the uſe of a priſm, which, he ſays, is ſo odd a one, and ſo difficult to explain by the vulgar hypotheſis, that he could not but think it deſerved to be taken notice of. It is as follows: Let HFG, fig. 85, repreſent a priſm in the open air, and S the eye of the ſpectator, viewing the clouds by reflexion from the baſe of it. When the priſm and the eye are conveniently placed, ſo that the angles of incidence and reflexion may be about 40 degrees, a bow, MN, will be ſeen, of a blue colour, running from one end of the baſe to the other, with the concave ſide towards the ſpectator, and that part of the baſe of the priſm which lies beyond this bow will be brighter than the other. This blue colour, he ſays, is made by nothing but by reflexion; and to explain it, he bids us ſuppoſe the plane ABC to cut the ſides and baſe of the priſm perpendicularly. From the eye to the line BC, draw the lines S *p* and S *t*, making the angles S *pc* 50 $\frac{1}{9}$ degrees, and S *tc* 49 $\frac{1}{28}$ degrees; and the point *p* will be the limit, beyond which none of the moſt refrangible rays can paſs through the baſe of the priſm, and be refracted, if their incidence be ſuch, that they can be reflected to the eye. The point *t* will be the like limit for the leaſt refrangible rays, and the point *r* will be the like limit for the mean refrangible ones; and therefore all the leaſt refrangible rays which fall upon the baſe beyond *t*, that is between *t* and B, and which can come from thence to the eye, will be reflected to it; but on this ſide *t*, many of theſe rays will be tranſmitted through the baſe. For the ſame reaſon, all the moſt refrangible rays, which fall upon the baſe beyond *p*, and which can by reflexion come from thence to the eye, will be reflected to it; but every where between *p* and C many of theſe rays will get through the baſe, and be refracted; and the ſame is to be underſtood of the mean refrangible rays on either ſide of the point *r*. It follows from hence, that the baſe of the priſm muſt every where, between *t* and B, in conſequence of a total reflexion of all kinds of rays to the eye, look white and bright, whereas the ſpace between *p* and C, by reaſon of the tranſmiſſion of many

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* Newton's Opticks, p. 141.

rays

rays of every kind, will look more pale, obſcure, and dark. But at *r*, and in other places, between *p* and *t*, where all the more refrangible rays are reflected to the eye, and many of the leſs refrangible are tranſmitted, the exceſs of the more refrangible in the reflected light will tinge it with their colour, which is violet and blue.*

In the next place we find our excellent philoſopher applying his new theory of light and colours to the celebrated phenomena of the *rainbow*, taking this remarkable object of philoſophical inquiry where De Dominis and Deſcartes, for want of this knowledge, were obliged to leave their inveſtigations imperfect. For they could give no good reaſon why the bow ſhould be coloured, and much leſs could they give any ſatisfactory account of the order in which the colours appear.

If different particles of light had not different degrees of refrangibility, on which the colours depend, the rainbow, beſides being much narrower than it is, would be colourleſs; but the different refrangibility of differently coloured rays being admitted, the reaſon is obvious, both why the bow ſhould be coloured, and alſo why the colours ſhould appear in the order in which they are obſerved. Let A, fig. 86, be a drop of water, and S a pencil of light; which, on its leaving the drop of water, reaches the eye of the ſpectator. This ray, at its entrance into the drop, begins to be decompoſed into its proper colours, and upon leaving the drop, after one reflexion and a ſecond refraction, it is farther decompoſed into as many ſmall differently coloured pencils, as there are primitive colours in the light. Three of them only are drawn in this figure, of which the blue is the moſt, and the red the leaſt refracted.

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* Newton's Opticks, p. 145.

The doctrine of the different refrangibility of light enables us to give a reaſon for the ſize of a bow of each particular colour. Newton, having found that the ſines of refraction of the moſt refrangible and leaſt refrangible rays, in paſſing from rain water into air, are in the proportion of 185 to 182, when the ſine of incidence is 138, calculated the ſize of the bow; and he found that, if the ſun was only a phyſical point, without ſenſible magnitude, the breadth of the inner bow would be 2 degrees; and if to this 30′ was added, for the apparent diameter of the ſun, the whole breadth would be 2 ½ degrees. But as the outermoſt colours, eſpecially the violet, are extremely faint, the breadth of the bow will not, in reality, appear to exceed two degrees. He finds, by the ſame principles, that the breadth of the exterior bow, if it was every where equally vivid, would be 4° 20′. But, in this caſe, there is a greater deduction to be made, on account of the faintneſs of the light of the exterior bow; ſo that, in fact, it will not appear to be more than 3 degrees broad.*

Laſtly, by the help of the ſame excellent theory, our author makes the following obſervations, in order to an explanation of the colours of natural bodies. According to this theory they all ariſe from ſome bodies reflecting ſome kinds of rays more copiouſly than others, as was obſerved before. Thus minium reflects the leaſt refrangible, or red rays moſt copiouſly, and thence appears red; whereas violets reflect the moſt refrangible rays moſt copiouſly, and every other body reflects the rays of its own colour more copiouſly than the reſt; and if they be placed in the homogeneous lights ſeparated by the priſm, every body will look the moſt ſplendid and luminous in the light of its own colour.

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* Montucla, Vol. 2, p. 649.

Cinnabar in the red light is moſt reſplendent, in the green light it is leſs ſo, and in the blue light leſs ſtill. Indigo is more ſtrongly illuminated by the violet blue light, and a leek by the green light. To make theſe experiments the more manifeſt, ſuch bodies ought to be choſen as have the fulleſt and moſt vivid colours, and two of theſe ought to be compared together.

But in trying experiments of this kind, care muſt be taken that the light be ſufficiently ſeparated; for if the bodies be illuminated by the ordinary priſmatick colours, they will appear neither of their own day light colours, nor of the colour of the light caſt on them, but of ſome middle colour between both, as our author found by experience. Thus red lead, illuminated with the ordinary priſmatick green, will not appear either red or green, but orange or yellow, or between yellow and green, according as the green light, by which it is illuminated, is more or leſs compounded. For becauſe red lead appears red when illuminated with white light, in which all kinds of rays are equally mixed, and in the green light all kinds of rays are not equally mixed, the exceſs of the yellow, green, and blue rays in the incident green light will cauſe thoſe rays to abound ſo much, as to draw the colour of red towards themſelves. And becauſe the red lead reflects the orange and yellow rays moſt copiouſly, after the red ones, theſe rays will be more in proportion in the reflected light than they were in the incident light, and therefore the red lead will appear neither red nor green, but of a colour between both.

In tranſparently coloured liquors, it is obſervable, that the colour varies with their thickneſs. Thus a red liquor in a conical glaſs, held between the light and the eye, looks of a pale and dilute

lute yellow at the bottom, where it is thin. A little higher, where it is thicker, it is orange, where it is ſtill thicker it becomes red, and where it is thickeſt the red is deepeſt and darkeſt. For it is to be conceived that ſuch a liquor ſtops the violet rays the moſt eaſily, the blue and green with more difficulty, and the red with the moſt difficulty. This illuſtrates an obſervation related to our author by Dr. Halley, who in diving deep into the ſea, in a diving veſſel, found in a clear ſun ſhine day, that when he was many fathoms under water, the upper part of his hand, on which the ſun ſhone through the water, appeared like a damaſk roſe; whereas the water below him, and the under part of his hand, illuminated by light reflected from the water below, looked green. From this circumſtance our author collects that the ſea-water reflects the violet and blue rays moſt eaſily, and lets the red rays paſs moſt freely and copiouſly to great depths; for thereby the ſun's direct light at all great depths muſt appear red, and the greater the depth is, the fuller and more intenſe muſt the red be; and at ſuch depths as the violet rays ſcarce reach, the blue, green, and yellow rays, being reflected from below more copiouſly than the red ones, muſt make a green.

If there be two liquors of full colours, for inſtance, a red and a blue, and both of them be ſo thick as to make their colours ſufficiently full, though either of the liquors be tranſparent ſeparately, they will not be ſo when put together. For if only the red rays paſs through one liquor, and the blue only through the other, no rays can paſs through them both. This obſervation Dr. Hooke, as we have ſeen, made caſually with glaſs wedges filled with red and blue liquors, and was ſurpriſed at the unexpected reſult, the reaſon of it being then unknown. This circumſtance makes our author depend upon the fact the more, though he had not tried it himſelf. He adviſes the perſon who would repeat this experiment, to take care that the liquors be of very good and full colours.

Since bodies become coloured by reflecting or tranſmitting one kind of rays more copiouſly than the reſt, our author ſuppoſes that they ſtop and ſtifle within themſelves, thoſe rays which they do not reflect or tranſmit. If a piece of leaf gold, as Mr. Boyle obſerved, be held between the eye and the light, it appears of a greeniſh blue, and therefore our author concludes, that maſſy gold lets into its body the blue rays to be reflected backward and forwards within its ſubſtance, till they be ſtopped and ſtifled; but it reflects the yellow rays, and by that means appears yellow.

After the ſame manner there are ſome liquors, as the tincture of *lignum nephriticum*, and ſome kinds of glaſs, which tranſmit one kind of light more copiouſly, and reflect another, ſo as to appear of ſeveral colours, according to the poſition of the eye to the light. If theſe liquors or glaſſes were ſo thick and maſſy, that no light could get through them, he did not queſtion but that they would, like all other opaque bodies, appear of one and the ſame colour in all poſitions of the eye, though he could not affirm it from experience. All coloured bodies, he ſays, as far as his obſervation reached, might be ſeen through, if they were made ſufficiently thin. He therefore concludes that the moſt opaque bodies are, in ſome meaſure, tranſparent, and differ only in their degree of tranſparency from tinged tranſparent liquors; theſe liquors, as well as thoſe bodies, becoming opaque by nothing but a ſufficient thickneſs.

A tranſparent body, which is of any particular colour by tranſmitted light, may alſo appear of the ſame colour by reflected light, the light of that colour being reflected by the farther ſurface of the body, or by the air beyond it. In this caſe, he ſays, the reflected colour will be diminiſhed, and perhaps intirely ceaſe, by making the body very thick, and by laying pitch on the back ſide of it, to diminiſh the reflexion of its farther ſurface, ſo that the light reflected from the tinging particles may predominate. In ſuch caſes the colour of the reflected light will be apt to vary from that of the light tranſmitted.*

SECTION IV.

Observations on the colours of THIN SUBSTANCES.

THIS great man, having ſatisfactorily unfolded ſeveral important properties relating to the nature of light and colours, and having demonſtrated that every different colour conſiſts of rays which have a different and ſpecific degree of refrangibility, and that natural bodies appear of this or that colour, according to their diſpoſition to reflect this or that ſpecies of rays, proceeds to inquire farther into the circumſtances which are the cauſe of their diſpoſition to reflect different kinds of rays; and in this moſt abſtruſe inquiry, he appears to great advantage, both as a philoſopher and as a mathematician, though he does not acquit himſelf quite ſo much to our ſatisfaction as he has done in the former part of his ſubject.

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* Newton's Opticks, p. 156.

Two obſervations ſeem to have given him the hint to theſe curious inquiries; both of which had been noticed by Dr. Hooke, though one of them ſeems to have been caſually obſerved by himſelf. Dr. Hooke had remarked, that thin tranſparent ſubſtances, particularly water and ſoap, blown into bubbles, exhibit various colours, according to their thinneſs, though, when they have a conſiderable thickneſs, they appear colourleſs; and he himſelf obſerved, as he was compreſſing two priſms hard together, in order to make their ſides (which happened to be a little convex) to touch one another, that in the place of contact they were perfectly tranſparent, as if they had both been one continued piece of glaſs. For when the light fell ſo obliquely on the air, which, in other places, was between them, as to be all reflected, it ſeemed in that place of contact to be wholly tranſmitted, in ſo much that it looked like a black and dark ſpot; and when it was looked through, it appeared like a hole in that air, which was formed into a thin plate, by being compreſſed between the glaſſes. Through this hole objects might be ſeen very diſtinctly, though they were not viſible through other parts of the glaſſes, where the air intervened. Though theſe glaſſes were a little convex, yet this tranſparent ſpot was of a conſiderable breadth, which ſeemed to be owing to the parts of the glaſs giving way when they were preſſed. For by preſſing them very hard together, it would become much broader than before.

When this plate of air, by turning the priſms about their common axis, became ſo little inclined to the incident rays, that ſome of them began to be tranſmitted, there aroſe in it many ſlender arcs of colours, which at firſt were ſhaped almoſt like a conchoid, as they are delineated in fig. 87. And by continuing the motion of the priſms, thoſe arcs increaſed, and bended more and more about the tranſparent ſpot, till they were completed into circles, or rings ſurrounding it; and afterwards they grew continually more and more contracted.*

To obſerve more nicely the order of the colours produced, as he ſuppoſed, by this thin plate of air, he took two object glaſſes, one of them a plano-convex belonging to a 14 foot refracting telescope; and the other a large double convex one, for a teleſcope of about 50 feet, and laying the former of them upon the latter, with its plain ſide downwards, he preſſed them ſlowly together, in order to make the colours emerge ſucceſſively in the middle of the circle. By this means he was able to obſerve diſtinctly the order and quality of the colours from the central ſpot, to a very conſiderable diſtance.

Next to the pellucid central ſpot, made by the contact of the glaſſes, ſucceeded blue, white, yellow, and red. The blue was ſo little in quantity, that he could not diſcern it in the circles made by the priſms; nor could he well diſtinguiſh any violet in it; but the yellow and red were pretty copious, and ſeemed about as much in extent as the white, and 4 or 5 times more than the blue. The next circuit immediately ſurrounding theſe conſiſted of violet, blue, green, yellow, and red, and theſe were all of them copious and vivid, except the green, which was very little in quantity, and ſeemed much more faint and dilute than the other colours. Of the other four, the violet was the leaſt in extent, and the blue leſs than the yellow or red. The third circle of colours was purple, blue, green, yellow, and red. In this the purple ſeemed more reddiſh than the violet in the former circuit, and the green was much more conſpicuous; being as briſk and copious as any of the other colours, except the yellow; but the red began to be a little faded, inclining very much to purple. The fourth circle conſiſted of green and red, and of theſe the green was very copious and lively, inclining on the one ſide to blue, and on the other ſide to yellow; but in this fourth circle there was neither violet, blue, or yellow, and the red was very imperfect and dirty. All the ſucceeding colours grew more and more imperfect and dilute, till, after three or four revolutions, they ended in perfect whiteneſs.

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* Newton's Opticks, p. 169.

The appearance of theſe circles, when the glaſſes were moſt compreſſed, ſo as to make the black ſpot appear in the center, is delineated in fig. 88, in which *a, b, c, d, e*: *f, g, h, i, k*: *l, m, n, o, p*: *q, r*: *s, t*: *v, x*: *y, z* denote the colours reckoned in order from the center, viz. black, blue, white, yellow, red; violet, blue, green, yellow, red; purple, blue, green, yellow, red; green, red; greeniſh blue, red; greeniſh blue, pale red; greeniſh blue, reddiſh white.

To determine the interval of the glaſſes, or the thickneſs of the plate of air, by which he ſuppoſed that the colours were produced, he meaſured the diameters of the firſt 6 circles, at their moſt lucid parts, and ſquaring them, he found their ſquares to be in the arithmetical progreſſion of the odd numbers 1, 3, 5, 7, 9, 11; and ſince one of the glaſſes was plane and the other ſpherical, their intervals at thoſe circles muſt, he ſays, be in the ſame progreſſion. He alſo meaſured the diameters of the dark or faint rings between the more lucid colours, and he found their ſquares to be in the arithmetical progreſſion of the even numbers 2, 4, 6, 8, 10, 12, and ſince it was very nice and difficult to take theſe meaſures exactly, he repeated them ſeveral times at different parts of the glaſs.*

He next proceeded, with his uſual circumſpection and accuracy, to meaſure the preciſe thickneſs of this plate of air, and after uſing every precaution that he could think of, he concluded that the thickneſs of the air at the darkeſt part of the firſt dark ring, made by perpendicular rays, to be in the neareſt round numbers $\frac{1}{89000}$ part of an inch, half of which, multiplied by the progreſſion, 1, 3, 5, 7, 9, 11, &c. gives the thickneſſes of the air at the moſt luminous parts of all the brighteſt rings, their arithmetical means being its thickneſs at the darkeſt parts of all the dark ones.

Having examined the appearance of theſe coloured rings on the upper ſide of the thin plate by reflected light, he proceeded to examine the under ſide, in order to obſerve what light was tranſmitted. He now found that the central ſpot was white, and the order of the colours was yellowiſh red; black, violet, blue, white, yellow, red; violet, blue, green, yellow, red, &c. but theſe colours were very faint and dilute, except when the light was tranſmitted very obliquely through the glaſſes; for by that means they became very vivid, only the firſt yellowiſh red, like the blue in the former obſervation, was ſo little and faint, as hardly to be diſcernible. Comparing the coloured rings made by reflexion with thoſe made by the tranſmiſſion of light, he found that white was oppoſite to black, red to blue, yellow to violet, and green to a compound of red and violet. The manner of this is repreſented in fig. 89, in which AB, CD are the ſurfaces of the glaſſes contiguous at E, and the black lines between them are their diſtances in arithmetical progreſſion. The colours expreſſed above are ſeen in reflected light, and thoſe below by light tranſmitted.*

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* Newton's Opticks, p. 173.

The colours above mentioned were produced, as Sir Iſaac Newton thought, by means of a thin plate of air intercepted between the two glaſſes. When he put water between them, and meaſured the rings again, he found the proportion of their diameters, to the diameters of the like circles made by air, to be about 7 to 8; ſo that the intervals of the glaſſes, at ſimilar circles, cauſed by theſe two mediums water and air, were about 3 to 4; from whence our author concludes, that if any other medium leſs denſe than water be compreſſed between the glaſſes, their diſtances at the rings will be to their diſtances cauſed by the interjacent air as the ſines which meaſure the refraction out of that medium into air. But this he expreſſes with ſome degree of doubt, for he only ſays that *perhaps* this may be a general rule.

A remarkable phenomenon occured while our author was examining theſe colours made with water. When the water was between the glaſſes, if he preſſed the upper glaſs variouſly at its edges, in order to make the rings move nimbly from one place to another, a little white ſpot would immediately follow the center of them; and when the ambient water came to that place, it would preſently vaniſh. It was ſuch an appearance as interjacent air would have cauſed, and it exhibited the ſame colours, but yet it was not air; for where any bubbles of air were in the water, they would not vaniſh. He therefore concludes that this reflexion muſt have been cauſed by a more ſubtle medium, which could recede through the glaſſes, as the water came between them. * There is little reaſon to doubt but that this white ſpot was a mere *vacuum*, and no fluid whatever.

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* Newton's Opticks, p, 181.

In order to examine farther the phenomena of the coloured rings, produced by theſe glaſſes, our philoſopher darkened his room, and viewed them by the coloured light of a priſm caſt on a ſheet of white paper, his eye being ſo placed, that he could ſee the coloured paper by reflexion in the glaſſes, as in a looking glaſs; and by this means the rings became more diſtinct, and viſible to a far greater number than in the open air. He ſometimes ſaw more than 20 of them, whereas in the open air he could not diſcern above 8 or 9.

Appointing an aſſiſtant to move the priſm about its axis, that all the colours might fall ſucceſſively on that part of the paper which he ſaw by reflexion, from that part of the glaſſes where the circles appeared (ſo that all the colours might be ſucceſſively reflected from the circles to his eye, whilſt he held it immoveable) he found that the circles made by the red light were manifeſtly larger than thoſe which were made by the blue and violet; and it was very pleaſant, he ſays, to ſee them gradually ſwell or contract, as the colour of the light was changed. The diſtance of the glaſſes at any of the rings, when they were made by the outermoſt red light, was to their interval at the ſame ring made by the outermoſt violet, greater than 3 to 2, and leſs than 13 to 8. According to moſt of his obſervations, it was as 14 to 9; and from other curious obſervations on theſe rings, made by different kinds of light thrown upon them, he collected that the thickneſſes of the air between the glaſſes, where the rings are ſucceſſively made, by the limits of the 7 colours, red, orange, yellow, green, blue, indigo, and violet, in order, are one to another as the cube roots of the ſquares of the 8 lengths of a chord, which ſound the notes in an octave ſol, la, fa, ſol, la, mi, fa, ſol, that is as the cube roots of the ſquares of the numbers 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{8}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$.

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* Newton's Opticks, p. 183.

Theſe rings were not of different colours, like thoſe made in the open air, but appeared all over of that priſmatic colour only with which they were illuminated; and by projecting the priſmatic colours immediately upon the glaſſes, he found that the light, which fell on the dark ſpaces between the coloured rings was tranſmitted through the glaſſes without any change of colour. For on a white paper placed behind it, it would paint the rings of the ſame colour with thoſe that were reflected, and of the ſize of their intermediate ſpaces. From this circumſtance our author thought that the origin of theſe rings is manifeſt; for that the air between the glaſſes is diſpoſed, according to its various thickneſs, in ſome places to reflect, and in others to tranſmit the light of any particular colour, and in the ſame place to reflect that of one colour, where it tranſmits that of another, as is repreſented fig. 90.*

All the preceding obſervations were made with a rarer medium, ſurrounded by a denſer; but in the next place he proceeds to examine the phenomena of colours made by a denſer medium ſurrounded by a rarer, as thoſe which appear in plates of muſcovy glaſs, bubbles of ſoap and water, &c. And the colours which are produced in theſe circumſtances, our author found to be much more vivid that the others.† Bubbles made with ſoap and water, it is probable, had long been the diverſion of children, before they were noticed by Mr. Boyle, or Dr. Hooke; but here we ſee them dignified by the attention of a much greater philoſopher, who made the moſt important uſe of them.

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* Newton's Opticks, p. 183. † Ib. 195.

After reciting the particular appearances of theſe colours, Sir Iſaac Newton obſerves that they have a great affinity with thoſe that are made by air, deſcribed above. The circles of colours in all theſe plates appeared dilated when he viewed them obliquely, for then the plate of air or water, &c. was varied in its thickneſs; and agreeable to this, he ſays, that he has ſometimes obſerved that the colours which ariſe on poliſhed ſteel, by heating it, or on bell metal, and ſome other metalline ſubſtances, when they are melted and poured on the ground to cool in the open air, have likewiſe been a little changed by being viewed at different obliquities; and particularly that a deep blue or violet, when viewed very obliquely, had been changed to a deep red. But the changes of theſe colours, he ſays, are not ſo great and ſenſible as thoſe made by water; for, the ſcoriæ or vitrified part of the metal, which moſt metals when heated or melted continually protrude, and which, by covering the metals in the form of a thin glaſſy coat, cauſes theſe colours, is much denſer than water; and he found that the change made by the oblique poſition of the eye was the leaſt in the colours of the denſeſt thin ſubſtances. *

Leſt it ſhould be ſuſpected that theſe colours depend upon ſome other circumſtance than the thickneſs of the thin plate, beſides ſome other experiments, he wetted a thin plate of muſcovy glaſs,

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* Newton's Opticks, p. 194.

but he only obſerved that the colours became more faint and languid. There was no change of their ſpecies.*

Comparing the quantity of light reflected from the ſeveral rings, he found that it was moſt copious from the firſt or innermoſt; and in the exterior rings became gradually leſs and leſs. Alſo the whiteneſs of the firſt ring was ſtronger than that which was reflected from thoſe parts of the thin medium or plate which were without the rings, as he could clearly perceive by viewing at a diſtance the rings that were made by the two object glaſſes, or by comparing two bubbles of water blown at different times; in the firſt of which, that whiteneſs appeared which ſucceeded all the colours, and in the others, that whiteneſs which preceded them all.†

In many of the preceding caſes, when he could perceive few or none of the above mentioned rings with his naked eye, great numbers were viſible on viewing them through a priſm.§

Converſing with Mr. Michell upon the ſubject of this ſection, it immediately occurred to him, that, ſince a variety of obſervations, which will be recited in the laſt period of this work, make it ſufficiently evident, that the coloured rings do not depend upon the *plate of air*, all the effect of the *water* interpoſed between the glaſſes may, perhaps, be to puſh them farther aſunder, in conſequence of which, the rings will be contracted of courſe. This, as he obſerved to me, is ſimilar to the effect of water introduced into the form of many bodies, as clay, which always ſwells with moiſture. Ropes are alſo affected in the ſame manner by throwing water upon them; and the force with which theſe bodies are diſtended, by this means, can hardly be conceived; whereas the parts of glaſs yield to a very ſlight preſſure.

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* Newton's Opticks, p. 195. † Ib. Dr. Jurin ſuppoſes that Newton was led into a miſtake with reſpect to this ring of light, imagining it to be broader than it really is, on account of its contiguity to the black ſpot, the reaſon of which has been hinted already, and will be more fully explained hereafter. Jurin's Eſſay on diſtinct and indiſtinct viſion, p 171, § Newton's Opticks, p. 196.

SECTION V.

THE APPLICATION OF THE OBSERVATIONS ON THE COLOURS OF THIN PLATES TO THE EXPLICATION OF THE COLOURS OF NATURAL BODIES.

NEW and ſtriking as are the obſervations in the preceding ſection, on the colours of thin tranſparent plates, our author did not content himſelf with his accurate inveſtigation of them, but made uſe of them to explain the colours of natural colours; and, in order to this, he firſt preſents us with a curious mathematical ſcheme, drawn from the preceding *data*, to enable a perſon to diſtinguiſh readily and accurately what priſmatic colours enter into each of the coloured rings above-mentioned; by which it appears that thoſe which are neareſt to the central ſpot, and which are produced by the thinneſt part of the plate, are the moſt uncompounded, and that the reſt are diluted with various other colours, till at laſt the reſult is mere white.

In order to comprehend this, our author directs that there be taken, in any right line, from the point Y, fig. 91, the lengths YA, YB, YC, YD, YE, YF, YG, YH, in proportion to one another as the cube roots of the ſquares of the numbers $\frac{1}{2}$, $\frac{9}{16}$, $\frac{3}{5}$, $\frac{2}{3}$, $\frac{3}{4}$, $\frac{5}{6}$, $\frac{8}{9}$, 1, by which the lengths of a muſical chord, to ſound all the notes in an eighth, are repreſented; that is, in the proportion of the numbers 6300, 6814, 7114, 7631, 8255, 8855, 9243, 10000. And at the points A,B,C,D,E,F,G,H, let perpendiculars A*a*, B*b*, &c. be erected, by whoſe intervals the extent of the ſeveral colours ſet underneath againſt them is to be repreſented. Then divide the line A *a* in ſuch proportion as the numbers 1, 2, 3, 5, 6, 7, 9, 10, 11, &c. ſet at the points of diviſion denote; and through thoſe diviſions, from Y, draw lines 1 I, 2 K, 3 L, 5 M, 6 N, 7 O, &c.

Now if A 2 be ſuppoſed to repreſent the thickneſs of any thin tranſparent body, at which the outermoſt violet is moſt copiouſly reflected, in the firſt ring or ſeries of colours; then HK will repreſent the thickneſs at which the outermoſt red is moſt copiouſly reflected in the ſame ſeries. Alſo A 6 and HN will denote the thickneſſes at which thoſe extreme colours are moſt copiouſly reflected in the ſecond ſeries, and A 10 and HQ the thickneſſes at which they are moſt copiouſly reflected in the third ſeries, and ſo on. And the thickneſs at which any of the intermediate colours are reflected moſt copiouſly will be defined by the diſtance of the line AH from the intermediate parts of the lines 2 K, 6 N, 10 Q, &c. againſt which the names of thoſe colours are written below.

But, farther to define the latitude of theſe colours, in each ring or ſeries, let A 1 repreſent the leaſt thickneſs, and A 3 the greateſt thickneſs at which the extreme violet in the firſt ſeries is reflected, and let HI, and HL repreſent the like limits for the extreme red, and let the intermediate colours be limited by the intermediate parts of the lines 1 I, and 3 L, againſt which the names of thoſe colours are written, and ſo on; but yet with this caution, that the reflexions be ſuppoſed ſtrongeſt at the intermediate ſpaces 2 K, 6 N, 10 Q, &c. and from thence to decreaſe gradually towards theſe limits 1 I, 3 L, 5 M, 7 O, &c. on either ſide, where they muſt not be conceived to be preciſely limited, but to decay indefinitely. And though the ſame latitude is here aſſigned to every ſeries, it is becauſe, although the colours in the firſt ſeries ſeem to be a little broader than the reſt, by reaſon of a ſtronger reflection there, yet that inequality is ſo inſenſible, as ſcarcely to be determined by obſervation.

Now, according to this deſcription, conceiving that the rays originally of ſeveral colours are, by turns, reflected at the ſpaces 1 I L 3, 5 M O 7, 9 P R 11, &c. and tranſmitted at the ſpaces AHI 1, 3 L M 5, 7 O P 9, &c. it is eaſy to know what colour muſt in the open air be exhibited at any thickneſs of a tranſparent thin body. For if a ruler be applied parallel to AH, at that diſtance from it by which the thickneſs of the body is repreſented, the alternate ſpaces 1 I L 3, 5 M O 7, &c. which it croſſes, will denote the reflected original colours, of which the colour exhibited in the open air is compounded. Thus if the conſtitution of the green in the third ſeries of colours be deſired, apply the ruler as at *p r s f*, and by its paſſing through ſome of the blue at *p*, and yellow at *s*, as well as through the green at *r*, it may be concluded that the green exhibited at that thickneſs of the body is principally conſtituted of original green, but not without a mixture of ſome blue and yellow.*

On theſe grounds he contrived the following uſeful table, wherein the thickneſſes of air, water, and glaſs, at which the colours of every order appear, are expreſſed in parts of an inch, divided into ten hundred thouſand equal parts.

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* Newton's Opticks, p. 200.

The thickneſs of coloured PLATES *and* PARTICLES *of*

		Air.	*Water.*	*Glaſs.*
The Colours of the firſt Order,	Very black	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{10}{31}$
	Black	1	$\frac{3}{4}$	$\frac{20}{31}$
	Beginning of black	2	$1\frac{1}{2}$	$1\frac{2}{7}$
	Blue	$2\frac{2}{5}$	$1\frac{4}{5}$	$1\frac{11}{20}$
	White	$5\frac{1}{4}$	$3\frac{7}{8}$	$3\frac{2}{5}$
	Yellow	$7\frac{1}{9}$	$5\frac{1}{3}$	$4\frac{3}{5}$
	Orange	8	6	$5\frac{1}{6}$
	Red	9	$6\frac{3}{4}$	$5\frac{4}{5}$
Of the ſecond Order.	Violet	$11\frac{1}{6}$	$8\frac{3}{8}$	$7\frac{1}{5}$
	Indigo	$12\frac{5}{6}$	$9\frac{5}{8}$	$8\frac{2}{11}$
	Blue	14	$10\frac{1}{2}$	9
	Green	$15\frac{1}{8}$	$11\frac{1}{3}$	$9\frac{5}{7}$
	Yellow	$16\frac{2}{7}$	$12\frac{1}{5}$	$10\frac{2}{5}$
	Orange	$17\frac{2}{9}$	13	$11\frac{1}{9}$
	Bright Red	$18\frac{1}{3}$	$13\frac{3}{4}$	$11\frac{5}{6}$
	Scarlet	$19\frac{2}{3}$	$14\frac{3}{4}$	$12\frac{2}{3}$
Of the third Order.	Purple	21	$15\frac{3}{4}$	$13\frac{11}{20}$
	Indigo	$22\frac{1}{10}$	$16\frac{4}{7}$	$14\frac{1}{4}$
	Blue	$23\frac{2}{5}$	$17\frac{11}{20}$	$15\frac{1}{10}$
	Green	$25\frac{1}{5}$	$18\frac{9}{10}$	$16\frac{1}{4}$
	Yellow	$27\frac{1}{7}$	$20\frac{1}{3}$	$17\frac{1}{2}$
	Red	29	$21\frac{3}{4}$	$18\frac{5}{7}$
	Bluiſh red	32	24	$20\frac{2}{3}$
Of the fourth Order,	Bluiſh green	34	$25\frac{1}{2}$	22
	Green	$35\frac{2}{7}$	$26\frac{1}{2}$	$22\frac{3}{4}$
	Yellowiſh green	36	27	$23\frac{2}{9}$
	Red	$40\frac{1}{3}$	$30\frac{1}{4}$	26
Of the fifth Order,	Greeniſh blue	46	$34\frac{1}{2}$	$29\frac{2}{3}$
	Red	$52\frac{1}{2}$	$39\frac{3}{8}$	34
Of the ſixth Order,	Greeniſh blue	$58\frac{3}{4}$	44	38
	Red	65	$48\frac{3}{4}$	42
Of the ſeventh Order,	Greeniſh blue	71	$53\frac{1}{4}$	$45\frac{4}{5}$
	Ruddy White	77	$57\frac{3}{4}$	$49\frac{2}{3}$

By the help of this table our author obſerves, that the bigneſs of the parts of natural bodies may be conjectured by their colours. Alſo, if two or more very thin plates be laid one upon another, ſo as to compoſe one plate, equalling them all in thickneſs, the reſulting colour may be hereby determined. For inſtance, Dr. Hooke obſerved, as is mentioned in his *Micrographia,* that a faint yellow plate of Muſcovy glaſs, laid upon a blue one, conſtituted a very deep purple. The yellow of the firſt order is a faint one, and the thickneſs of the plate exhibiting it, according to the table, is $4\frac{3}{5}$ to which add 9, the thickneſs exhibiting blue of the ſecond order, and the ſum will be $13\frac{3}{5}$, which is very nearly the thickneſs exhibiting the purple of the third order. *

He then proceeds to give a curious ſolution of ſeveral particular appearances in his obſervations on theſe rings, which are too minute for this hiſtory. Thoſe who would examine them muſt conſult the latter part of the ſecond book of his Opticks, p. 2.

To apply the preceding principles to the colours of natural bodies, our author makes a variety of obſervations, the moſt important of which, with the evidence by which he ſupports them, I ſhall here introduce.

The ſuperficies of tranſparent bodies, ſuch as air, water, oil, common glaſs, cryſtal, metalline glaſſes, Iſland cryſtal, white tranſparent arſenick, diamonds, &c. have a ſtronger or weaker reflexion, according to the greater or leſs refractive power of the mediums that are contiguous to them. For at the confine of air and ſal-gem, it is ſtronger than at the confine of air and water, and ſtill ſtronger between air and common glaſs, or cryſtal, and ſtronger ſtill between air and a diamond. If any of theſe be immerſed in water, its reflexion becomes much weaker than before, and it is weaker ſtill if it be immerſed in liquors of greater refractive powers. If water be divided into two parts by any imaginary ſurface, the reflexion in the confine of thoſe two parts is none at all; and, for the ſame reaſon, there can be no ſenſible reflexion in the confine of two glaſſes of equal denſity. The reaſon, therefore, why all uniform pellucid mediums have no ſenſible reflexion, but at their external ſurfaces, where they are contiguous to mediums of different denſities, is that their contiguous parts have preciſely the ſame degree of denſity.

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* Newton's Opticks, p. 208.

The leaſt parts of almoſt all natural bodies are, in ſome meaſure tranſparent, and their opacity ariſes from the multitude of reflexions in their internal parts. This obſervation, our author ſays, has been made by other perſons, and will eaſily be granted by thoſe who have been converſant with microſcopes. It may alſo be tried by applying any ſubſtance to a hole, through which ſome light is admitted into a dark room: for, how opaque ſoever the ſubſtance may ſeem in the open air, it will, by that means, appear manifeſtly tranſparent, if it be ſufficiently thin. White metalline bodies, however, muſt be excepted; for, on account of their exceſſive denſity, they ſeem to reflect almoſt all the light that falls on their firſt ſurface, unleſs, by ſolution in menſtruums, they be reduced into very ſmall particles; and then they all become tranſparent. * It will appear hereafter, in the account of M. Bouguer's experiments, that even poliſhed metalline bodies do not reflect more than about half the light that falls upon them.

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* Newton's Opticks, p. 220.

The parts of bodies, and their interſtices, muſt not be leſs than of ſome definite bigneſs to render them opaque and coloured; for the moſt opaque bodies, as has juſt been obſerved, if they be minutely divided, become perfectly tranſparent. There was alſo no ſenſible reflexion at the ſurface of the object glaſſes, when they were very near one another, though they did not abſolutely touch; and the reflexion of the water bubble where it was thinneſt was almoſt inſenſible, ſo as to cauſe very black ſpots to appear on the top of the bubble, through the want of reflected light. For this reaſon he ſuppoſes it is, that water, ſalt, glaſs, ſtones, and ſuch like ſubſtances, are tranſparent; for though, on ſeveral accounts, they ſeem to be as full of pores, as other bodies, yet their parts, and the interſtices between them, are too ſmall to cauſe reflexions at their common ſurfaces.

It is an obvious deduction from the preceding phenomena of the plates, that the tranſparent parts of bodies, according to their ſeveral ſizes, reflect rays of one colour, and tranſmit thoſe of another; on the ſame account that thin plates, or bubbles, reflect or tranſmit thoſe rays; and this he took to be the reaſon of all their colours. In his illuſtration of this propoſition, he obſerves that, if we conſider the various phenomena of the atmoſphere, we may obſerve, that when vapours are firſt raiſed, they do not hinder the tranſparency of the air, being divided into parts that are too ſmall to cauſe any reflexion at their ſurfaces; but when, in order to compoſe drops of rain, they begin to coaleſce, and conſtitute globules of all intermediate ſizes, thoſe globules, when they come to be of a ſize proper to reflect ſome colours and tranſmit others, may conſtitute clouds of various colours, according to their ſizes; and he did not think that any thing was requiſite for the production of theſe colours in ſo tranſparent a ſubſtance as water, beſides the various ſizes of its fluid and globular parts, and he could not ſee that any thing elſe could rationally account for theſe colours in ſuch a ſubſtance as water. * This opinion we ſhall find juſtly controverted by Mr. Melville, with reſpect to the red colour of the morning and evening clouds.

One of the moſt aſtoniſhing, and yet one of the cleareſt deductions from the phenomena of thin plates, is that the ſize of thoſe component parts of natural bodies that affect the light, may be conjectured by their colours. For ſince the parts of theſe bodies, moſt probably, exhibit the ſame colours with a plate of equal thickneſs, provided they have the ſame refractive denſity; and ſince their parts ſeem, for the moſt part, to have much the ſame denſity with water or glaſs (which he thought might eaſily be collected from many circumſtances) the preceding table, in which the thickneſs of water or glaſs exhibiting any particular colour is expreſſed, will be ſufficient to determine the ſizes of the parts of other bodies that reflect the ſame colours. Thus, if it be deſired to know the diameter of a particle, which, being of equal denſity with glaſs, ſhall reflect green of the third order, the number 16 $\frac{1}{4}$ ſhews it to be $\frac{16\frac{1}{4}}{10000}$ parts of an inch.

The greateſt difficulty in this inveſtigation, as our author acknowledges, will be to determine the order to which the particular colour of any body belongs, that is, of which colour of the ſame ſpecies it is, with reſpect to its diſtance from the central ſpot. But any perſon may be aſſiſted in judging of this, who ſhall conſider accurately the vividneſs, the particular tinge, and other circumſtances relating to theſe colours; or, without ſeeing

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* Newton's Opticks, p. 226, 228.

them himſelf, ſhall accurately conſider our author's minute deſcription of them, p. 198, &c. to which I refer my reader, as I muſt alſo do for ſeveral curious applications of them, in his attempt to explain the natural colours of ſome particular bodies, p. 230, &c. To be as conciſe as poſſible, I muſt content myſelf with delivering his general doctrine upon this ſubject. The reaſon why he is ſo particular in theſe deſcriptions, he ſays, is becauſe it is not impoſſible, but that microſcopes may be improved ſo far as to enable us to diſcover thoſe particles of bodies on which their colour depends, if they be not already, in ſome meaſure, arrived to that degree of perfection. For if theſe inſtruments are, or can be ſo far improved, as, with ſufficient diſtinctneſs, to repreſent objects 5 or 6 hundred times bigger than, at a foot diſtance, they appear to our naked eyes, he ſhould hope that we might be able to diſcover ſome of the largeſt of thoſe particles; and by one that ſhould magnify 3 or 4 thouſand times, he thought they might perhaps be all diſcovered, except thoſe which produce blackneſs. The diſcovery of theſe particles, however, he fears will be the utmoſt improvement of the ſenſe of viſion; it being impoſſible to ſee the more ſecret and noble works of nature within theſe particles, on account of their tranſparency. *

The cauſe of the reflexion of light, our philoſopher obſerves, is not the impinging of it on the ſolid or the impervious parts of bodies, as is commonly ſuppoſed; and for this opinion he alledges ſeveral reaſons, the moſt ſtriking of which I ſhall quote. 1ſt. In the paſſage of light out of glaſs into air, there is as ſtrong a reflexion as in its paſſage out of air into glaſs, or rather a little

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* Newton's Opticks, p. 336.

ſtronger than in its paſſage out of glaſs into water; and it is not probable that air ſhould have more reflecting parts than water or glaſs. But if this ſhould be ſuppoſed, it will avail nothing; ſince the reflexion is as ſtrong, or ſtronger, when the air is drawn away from the glaſs.

2dly, If the colours made by a priſm, placed at the entrance of a beam of light into a darkened room, be ſucceſſively caſt on a ſecond priſm, placed at a greater diſtance from the former, in ſuch a manner as that they ſhall be all alike incident upon it, the ſecond priſm may be ſo inclined to the incident rays, that thoſe which are of a blue colour ſhall be all reflected by it, and yet thoſe of a red colour ſhall be copiouſly tranſmitted. Now if the reflexion be cauſed by the parts of air, or glaſs, our author aſks, why, at that obliquity of incidence, the blue ſhould all impinge on thoſe parts, ſo as to be all reflected, and yet the red find pores enow to be, in a great meaſure, tranſmitted.

3dly, If reflexion was cauſed by the ſolid parts of reflecting bodies, it will be impoſſible for thin plates, or bubbles, at one and the ſame place, to reflect the rays of one colour, and tranſmit thoſe of another, as they do. For it is not to be imagined that at one place the rays, which, for inſtance, exhibit a blue colour, ſhould have the fortune to daſh upon the ſolid parts, while thoſe which exhibit a red colour, hit upon the pores of the body, and then, at another place, where the ſubſtance is either a little thicker or a little thinner, that the blue ſhould hit upon its pores, and the red upon the ſolid parts.

Laſtly, were the rays of light reflected by impinging on the ſolid parts of bodies, their reflexions from poliſhed ſubſtances could not be ſo regular as they are. For in poliſhing glaſs with ſand, putty, or tripoli, it is not to be imagined that thoſe ſubſtances can, by grating and fretting the glaſs, bring all its leaſt particles to an accurate poliſh, ſo that all their ſurfaces ſhall be truly plane, or truly ſpherical, and look all the ſame way, and together compoſe one even ſubſtance. The ſmaller the particles of thoſe ſubſtances are, the ſmaller will be the ſcratches by which they continually fret and wear away the glaſs, till it be poliſhed; but be they ever ſo ſmall, they can only wear away the glaſs by grating and ſcratching it, and breaking the protuberances; and therefore they poliſh it no otherwiſe than by bringing its roughneſs to a very fine grain; ſo that the ſcratchings and frettings of the ſurface become too ſmall to be viſible; and therefore, if light were reflected by impinging upon the ſolid parts of the glaſs, it would be ſcattered as much by the moſt poliſhed glaſs as by the rougheſt. It remains a problem then how glaſs, poliſhed by fretting ſubſtances, can reflect the light ſo regularly as it does; and this problem can hardly be ſolved, but by ſuppoſing that the reflexion of a ray is made not by a ſingle point of the reflecting body, but by ſome *power* of the body, which is diffuſed evenly all over its ſurface, and by which it acts upon the ray without immediate contact. And that the parts of bodies do act upon light at a diſtance will be evident from the experiments of our author upon the inflection of light.

He concludes with obſerving that, if light be reflected not by impinging on the ſolid parts of bodies, but by ſome other principle, it is probable that as many of its rays as impinge on the ſolid parts of bodies are not reflected, but ſtifled, and loſt in the bodies. Otherwiſe, he ſays, we muſt ſuppoſe two kinds of reflexion; for ſhould all the rays be reflected which impinge on the internal parts of clear water or cryſtal, thoſe ſubſtances would rather have a cloudy colour, than a clear tranſparency. To make bodies look black, it is neceſſary that many rays be ſtopped, retained, and loſt in them; and it does not ſeem probable that any rays can be ſtopped and ſtifled in them, which do not impinge on their parts; and hence, he ſays, we may underſtand, that bodies are much more rare and porous than is commonly believed. Water is 19 times lighter, and conſequently 19 times rarer than gold, and yet that gold, and the hardeſt and moſt ſolid bodies are extremely porous, he argues from ſeveral conſiderations; but his argument from their ready tranſmiſſion of the magnetic effluvia does not ſeem to be ſatisfactory. It is atleaſt a doubt whether there be any ſuch effluvium, or if there be, it may act through glaſs, like the electric fluid, without an actual tranſmiſſion. That light is ſtifled or loſt by impinging on the ſolid parts of bodies, we ſhall find, is not admitted by M. Bouguer.

To aſſiſt us to conceive how bodies may be exceedingly porous, he obſerves that, if we conſider the particles of bodies to be ſo diſpoſed among themſelves, as that the interſtices, or empty ſpaces between them, may be equal in magnitude to them all, and that theſe particles may be compoſed of other, and much ſmaller particles, and ſo on perpetually, till we come to ſolid particles, three ſuch degrees of particles, the leaſt of which are ſolid, will conſtitute a body which ſhall have 7 times more pores than ſolid parts. Four ſuch degrees of particles will make a body with 15 times more pores than ſolid parts, and ſo on perpetually. There are alſo other ways of conceiving how bodies may be exceedingly porous; but what is really their inward frame, is not yet known to us.

It was firſt advanced by Sir Iſaac Newton, that bodies reflect and refract light by one and the ſame power in different circumſtances. This, he ſays, appears from ſeveral conſiderations. Firſt, becauſe when light goes out of glaſs into air, as obliquely as it can poſſibly do, if its incidence be made more oblique, it becomes wholly reflected; for the power of the glaſs, after it has refracted the light as obliquely as poſſible, if the incidence be made more oblique, becomes too ſtrong to let any of its rays go through, and conſequently cauſes a total reflexion. Secondly, light is alternately reflected and tranſmitted by thin plates of glaſs for many ſucceſſions, according as the thickneſs of the plate increaſes in an arithmetical progreſſion; for here the thickneſs of the glaſs determines whether that power by which glaſs acts upon light ſhall cauſe it to be reflected or ſuffered to be tranſmitted. And thirdly, becauſe thoſe ſurfaces of tranſparent bodies which have the greateſt refractive power, do alſo reflect the greateſt quantity of light.*

By the help of a mathematical demonſtration, our author proves that, if light be ſwifter in bodies than in vacuo, in the proportion of the ſines which meaſure the refraction of bodies, the forces of the bodies to reflect and refract light, are very nearly proportional to the denſities of the ſame bodies, excepting that unctuous and ſulphureous bodies refract more than others of the ſame denſity.†

This propoſition he likewiſe proves by experiments on various bodies, the reſult of which he has expreſſed in a table, part of which I ſhall copy; wherein the proportion of the ſines which meaſure the refractions of ſeveral bodies; the denſity of the bodies, eſtimated by their ſpecific gravity; and their refractive power in reſpect of their denſities are ſet down in ſeparate columns.

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* Newton's Opticks, p. 244. † Ib. 245.

The refracting Bodies.	*The Proportion of the Sines of Incidence and Refraction of yellow Light.*	*The denſity and ſpecifick gravity of the Body.*	*The refractive Power of the Body in reſpect of its denſity.*
A Pſeudo-Topaz, being a natural, pellucid, brittle, hairy Stone, of a yellow Colour.	23 to 14	4’27	3979
Air.	3201 to 3200	0’0012	5208
Glaſs of Antimony.	17 to 9	5’28	4864
A Selenitis.	61 to 41	2’252	5386
Glaſs Vulgar.	31 to 20	2’52	5436
Cryſtal of the Rock.	25 to 16	2’65	5450
Iſland Cryſtal.	5 to 3	2’72	6536
Sal Gemmæ.	17 to 11	2’143	6477
Alum.	35 to 24	1’714	6570
Borax.	22 to 15	1’714	6716
Niter.	32 to 21	1’9	7079
Dantzick Vitriol.	303 to 200	1’715	7551
Oil of Vitriol	10 to 7	1’7	6124
Rain Water.	529 to 396	1’	7845
Gum Arabic.	31 to 21	1’375	8574
Spirit of Wine well rectified-	100 to 73	0’866	10121
Camphire.	3 to 2	0’996	12551
Oil Olive.	22 to 15	0’913	12607
Linſeed Oil.	40 to 27	0’932	12819
Spirit of Turpentine.	25 to 17	0’874	13222
Amber.	14 to 9	1’04	13654
A Diamond.	100 to 41	3’4	14556

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* This table, as well as that of Mr. Haukſbee, will be reprinted in another form, together with ſome of Mr. Euler's, in the laſt period of this hiſtory.

The refraction of the air in this table is determined by that of the atmoſphere obſerved by aſtronomers. For if light paſs through ever ſo many refracting mediums, which are gradually denſer and denſer, the ſum of all the refractions will be equal to the ſingle refraction which it would have ſuffered in paſſing immediately out of the firſt medium into the laſt. And therefore the whole refraction of light, in paſſing through the atmoſphere, muſt be equal to the refraction which it would ſuffer in paſſing, at the ſame obliquity, out of a vacuum immediately into air, of equal denſity with that which is in the loweſt part of the atmoſphere. *

Our author's reaſoning concerning the different refractive power of certain ſubſtances is ſo curious, and his obſervations on the ſubject are ſo likely to be a means of making farther diſcoveries into the powers of natural bodies, that I ſhall recite them intire.

Though a Pſeudo Topaz, ſelenitis, rock cryſtal, iſland cryſtal, common glaſs, and glaſs of antimony, which are terreſtrial, ſtony, alkalizate concretes, and air, which probably ariſes from ſuch ſubſtances by fermentation, differ very much from one another in denſity, yet they appear, by this table, to have refractive powers almoſt in the ſame proportion to one another as their denſities, excepting that the refraction of that ſtrange ſubſtance iſland cryſtal is a little ſtronger than the reſt. Air, in particular, which is 3500 times rarer than the pſeudo topaz, and 4400 times rarer than glaſs of antimony, and 2000 times rarer than the ſelenitis, common glaſs, or rock cryſtal, has, notwithſtanding its rarity, the ſame refractive power in reſpect to its denſity,

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* Newton's Opticks, p. 248.

which thoſe very denſe ſubſtances have with reſpect to theirs, excepting ſo far as they differ from one another.

Again, the refraction of camphire, olive oil, linſeed oil, ſpirit of turpentine and amber, which are fat, ſulphureous, unctuous bodies, and alſo a diamond, which is probably an unctuous ſubſtance coagulated, have their refractive powers in proportion to one another as their denſities, without any conſiderable variation. But the refractive powers of theſe unctuous ſubſtances are two or three times greater, with reſpect to their denſities, than the refractive powers of the former ſubſtances are with reſpect to theirs.

Water has a refractive power in a middle degree between thoſe two kinds of ſubſtances, and is, probably, of a middle nature. For out of it grow all vegetable, and animal ſubſtances, which conſiſt as well of ſulphureous, fat, and inflamable parts, as of earthy, lean, and alkalizate ones.

Salts and vitriols have refractive powers in a middle degree between thoſe of earthy ſubſtances and water, and accordingly are compoſed of thoſe two ſorts of ſubſtances. For, by diſtillation and rectification of their ſpirits, a great part of them goes into water, and a great part remains behind, in the form of a dry fixed earth, capable of vitrification.

Spirit of wine has a refractive power in a middle degree between thoſe of water and oily ſubſtances; and, accordingly, ſeems to be compoſed of both, united by fermentation; the water by means of ſome ſaline ſpirits with which it is impregnated, diſſolving the oil, and volatilizing it by the action. For ſpirit of wine is inflammable by means of its oily parts, and being diſtilled often from ſalt of tartar, grows, by every diſtillation, more and more aqueous; and chymiſts obſerve that vegetables, as lavender, rue, marjoram, &c. diſtilled *per ſe* before fermentation, yield oils without any burning ſpirits; but that after fermentation, they yield ardent ſpirits, without oil; which ſhews, that their oil is, by fermentation, converted into ſpirit. They alſo find that, if oil be poured in a ſmall quantity upon fermenting vegetables, they diſtil over, after fermentation, in the form of ſpirits.

It appears, therefore, by the foregoing table, that all bodies ſeem to have their refractive powers proportional to their denſities, or very nearly, excepting ſo far as they partake more or leſs of ſulphureous oily particles, and thereby have their refractive power made greater or leſs. Whence it ſeems reaſonable to attribute the refractive power of all bodies chiefly, if not wholly, to the ſulphureous parts with which they abound, For it is probable that all bodies abound more or leſs with ſulphur, and as light collected by a burning glaſs acts moſt ſtrongly upon ſulphureous bodies, to turn them into fire and flame; ſo, ſince all action is mutual, ſulphur ought to act moſt ſtrongly upon light. For that the action between light and bodies is mutual, may appear from this conſideration, that the denſeſt bodies, which refract and reflect the light moſt ſtrongly, grew hotteſt in the ſummer ſun, by the action of the refracted or reflected light. *

In order to account for the intervals of the coloured rings. In theſe thin plates, and alſo all other caſes of the reflexion or tranſmiſſion of light, Sir Iſaac Newton advances an hypotheſis; but, like a wiſe man and a cautious philoſopher, he profeſſes not to lay much ſtreſs upon it, though he ſeems not to entertain any ſuſpicion of its truth. He ſuppoſes that every ray of light is, at its firſt emiſſion from the luminous body, put into a *tranſient ſtate, or conſtitution,* which, in its progreſs, returns at equal intervals, diſpoſing it, at every return, to be eaſily tranſmitted into any refracting ſurface it may then meet with, whereas, in the interval between theſe returns, it is diſpoſed to be eaſily reflected; ſo that, upon the arrival of a number of rays of light at the ſurface of any medium, thoſe of them which were in a ſtate in which they were diſpoſed to be tranſmitted eaſily, would paſs the interval between the two mediums, and thoſe which were in a contrary ſtate would be reflected; on which account ſome light is generally reflected and ſome tranſmitted, at every different ſurface on which it falls. Theſe ſtates into which the rays of light are put he calls *fits of eaſy reflexion and tranſmiſſion.*

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* Newton's Opticks, p. 248.

He farther conjectures, that theſe *fits of eaſy reflexion and tranſmiſſion* may be occaſioned by the vibrations of a ſubtle fluid, in which the ray paſſes; any ray being diſpoſed to be tranſmitted when the vibration coincides with it, and to be reflected when it is counteracted by it. He alſo thought that theſe vibrations might be excited by the mutual action and reaction of light, of bodies, and of this medium, at the inſtant of reflexion or refraction. In fact, therefore, he ſuppoſes *two* cauſes of this diſpoſition to be reflected or tranſmitted, when rays of light arrive at any new ſurface. One of them is the regular vibration of the etherial medium, affecting them through the whole of their progreſs from the luminous body, and the other the tremulous motion, or irregular vibration of the ſame medium at the ſurfaces of bodies, occaſioned by the action and reaction between light and them. For this laſt cauſe can hardly be ſuppoſed to affect the whole ethereal medium equally, and produce the regular returns of theſe fits in every ray. To account for the reflection and tranſmiſſion of light, conſiſting of all colours promiſcuouſly, at every change of medium, either of theſe hypotheſis may be ſufficient; but it does not appear that either of them will ſuffice for the colours of the thin plates above mentioned.

If every ray of light be affected with theſe fits of eaſy reflexion and tranſmiſſion, during its whole progreſs from the luminous body, and if they commence at the luminous body, the intervals between them muſt depend upon the diſtance from it, and therefore not upon any circumſtance reſpecting the body on which it falls; ſo that, without any regard to the thickneſs or thinneſs of the above mentioned plates, light of any colour muſt be reflected or refracted indifferently, according to the diſtance of the ſurface from the luminous body.

If the whole of this medium vibrate at the ſame time, it muſt affect all kinds of rays alike, ſo that rays of all colours would be diſpoſed to be reflected or refracted at the ſame time, provided they were at the ſame diſtance from the luminous body, even though they ſhould move with different velocities. Or if theſe etherial pulſes be ſuppoſed to affect ſome kinds of rays more than others, that effect will ſtill be uniform, and, conſequently, the very ſame whatever be the thickneſs of the body on which it falls.

If, as Newton is obliged to maintain, the diſpoſition of the rays to be reflected or refracted depend upon the thickneſs of the plate on which the rays fall, as well as upon their own ſpecific quality, the whole hypotheſis of *previous* fits, or fits occaſioned by tremulous motions at their ſurfaces, ſeems to be ſuperfluous; for what would the arrival of any particular ray at the ſurface of any body, in a fit of reflexion ſignify, if the thickneſs of the plate was ſuch as required it to be tranſmitted. Alſo ſince the tremulous motion of the ſurface muſt be perpetually changing with reſpect to every particular kind of rays, the fits of tranſmiſſion or reflexion would return alternately for them all at each ſurface.

This hypotheſis obliges the author to have recourſe to what is certainly an improbable ſuppoſition, viz. That the reflexion or refraction takes place at the ſecond ſurface of the thin plate, for, indeed, as he ſays, if it was performed at the firſt ſurface, it could not depend upon the diſtance between them; and as he ſays that theſe alternate reflexions and tranſmiſſions are obſerved in plates not even leſs than $\frac{1}{4}$ of an inch thick, we muſt ſuppoſe that in theſe caſes the light that forms theſe colours enters ſo far into a body before any part of it be reflected.

M. Boſcovich, one of the firſt rate mathematicians and philoſophers of the preſent age, and who has particularly diſtinguiſhed himſelf by a new and general theory of the laws of nature, of which ſome account will be given in the laſt period of this hiſtory, ſuppoſes, with Newton, that the fits of reflexion or tranſmiſſion affect the rays themſelves, in the whole of their paſſage from the luminous body, but that they ariſe from an alteration in their form, by means of the elaſticity of their component parts, having been originally driven from the luminous body by a force which acted more ſtrongly on the hinder parts than on the reſt of the maſs, and thereby put them into a vibratory motion.

He alſo ſuppoſes, that the intervals of the fits of eaſy reflexion and tranſmiſſion may be different in different rays on the three following accounts, 1. The rays that are differently refrangible have different velocities, at leaſt after refraction; ſo that, though the fits ſhould return at equal intervals, it will affect different rays in different parts of their progreſs. 2. The unequal action between the points that compoſe the ſame particle of light may make a difference in their oſcillations, at the time of changing their medium. Laſtly, rays coming in different inclinations to the new ſurface, the internal motions of theſe points will be in different directions with reſpect to the ſurface, and conſequently the whole maſs of the medium will act upon all the points differently. *

Upon the whole, is it not more probable, that the rays of light are tranſmitted from the ſun, with an uniform diſpoſition, to be reflected or refracted according to the circumſtances of the bodies on which they impinge, and that the tranſmiſſion of ſome of the rays, under the ſame circumſtances, apparently, with others, that are reflected, is owing to the minute vibrations of the ſmall parts of the ſurfaces of the mediums through which the rays paſs, vibrations that are independent of action and reaction between the bodies and the particles of light at the time of their impinging, though, probably, excited by the action of preceding rays. That theſe ſurfaces will have ſuch vibrations can hardly admit of a doubt, whereas it ſeems very doubtful, notwithſtanding the infinite diviſibility of matter, whether the rays themſelves are not too ſmall to admit of having one ſide of them ſo violently propelled, while the other ſide is ſo far out of the reach of the repellency, in the exertion of that power by which they are driven off from the luminous body, as to have any conſiderable vibration excited in them by it. It is poſſible however that M. Boſcovich's ſcheme, though not equally probable, may

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* Theoria, p. 232.

be true, and perhaps both theſe cauſes may concur to produce this effect.

As to the tranſmiſſion or reflexion of certain kinds of light only, producing the colours in the thin plates, the cauſe may be this ; viz. that every particle of the medium has a great number of equal alternate intervals of attraction and repulſion, relatively to the particles of light, but that theſe intervals are of different magnitudes, according as the particles of light are of different colours. Now if the thickneſs of any tranſparent medium in which the particles of matter are uniformly placed, is ſuch, that the attracting intervals of the extreme particles, as well as the repelling intervals, coincide with one another, i. e. attracting with attracting, and repelling with repelling, in regard to any one kind of rays, as the red for inſtance, by the united force of theſe extremes (all the intermediate particles of the medium mutually deſtroying each others effects) theſe rays will be reflected. But where the plate is of an intermediate thickneſs, between this and the next thickneſs, where the attracting intervals coincide, attracting with attracting, and repelling with repelling, the attracting intervals will coincide with the repelling ones, and the repelling ones with the attracting ones, and theſe mutually deſtroying one anothers effects, theſe rays will paſs on freely, and be tranſmitted. But as the intervals of attraction and repulſion are different for differently coloured rays, as has been ſufficiently demonſtrated, in this and the preceding ſection, the thickneſs of the plates at which theſe coincidences will, or will not happen, in the differently coloured rays, will be different.

Upon the whole, therefore, it is probable, that all the myſtery of theſe coloured plates depends upon the attractions and repulſions of the particles of the bodies that compoſe them, affecting different rays in a different manner, according to their thickneſs. My objections to Newton's manner of accounting for the colours of thin plates are of long ſtanding, but the hint of accounting for them in the manner that I have attempted to do, by the doctrine of *attractions and repulſions*, was firſt ſuggeſted to me by Mr. Michell, agreeably to whoſe conjectures relating to this ſubject, I have given the preceding account of the probable cauſe of theſe appearances.

SECTION VI.

OBSERVATIONS ON COLOURS PRODUCED BY THICK PLATES.

SIR Iſaac Newton, purſuing his diſcoveries concerning the colours of thin ſubſtances, found that the ſame was alſo produced by plates of a conſiderable thickneſs, diviſible into leſſer thickneſſes. But till he hit upon this theory the appearances, he acknowledges, were ſtrange and ſurpriſing to him.

There is no glaſs or ſpeculum, he obſerves, how well poliſhed ſoever, but, beſides the light which it refracts or reflects regularly, ſcatters every way, irregularly, a faint light; by means of which the poliſhed ſurface, when illuminated in a dark room, by a beam of the ſun's light, may eaſily be ſeen, in all poſitions of the eye. It was with this ſcattered light that the colours in the following experiments were produced.

The ſun ſhining into his darkened chamber, through a hole one third of an inch wide, he let the beam of light fall perpendicularly upon a glaſs ſpeculum, concave on one ſide, and convex on the other, ground to a ſphere of 5 feet and 11 inches radius, and quick ſilvered over on the convex ſide. Then holding a quire of white paper at the center of the ſphere to which the ſpeculums were ground, that is, at the diſtance of about 5 feet and 11 inches from the ſpeculum, in ſuch a manner, as that the beam of light might paſs through a little hole made in the middle of the paper to the ſpeculum, and thence be reflected back to the ſame hole; he obſerved upon the paper 4 or 5 concentric rings of colours, like rainbows, ſurrounding the hole, very much like thoſe which appeared in the thin plates above mentioned, but larger and fainter. Theſe rings, as they grew larger and larger, became more dilute, ſo that the fifth was hardly viſible; and yet ſometimes, when the ſun ſhone very clear, there appeared faint traces of a ſixth and ſeventh. If the diſtance of the paper from the ſpeculum was either much greater, or much leſs than ſix feet, the rings became dilute, and vaniſhed. And if the diſtance of the ſpeculum from the window was much more than 6 feet, the reflected beam of light would be ſo broad, at the diſtance of 6 feet from the ſpeculum, where the rings appeared, as to obſcure one or two of the innermoſt rings. He therefore uſually placed the ſpeculum at about 6 feet from the window, ſo that its focus might there coincide with the center of its concavity

vity at the rings upon the paper; and this poſition is always to be underſtood in the following obſervations when no other is expreſſed.*

The colours of theſe rings ſucceeded one another from the center outwards, in the ſame form and order with thoſe which were produced by the thin plates, when they were viewed, not by reflected, but by tranſmitted light. The colours were in the ſame order, round a white ſpot of faint light, ſomewhat broader than the reflected beam of light, which ſometimes fell on the middle of the ſpot, and ſometimes, by a little inclination of the ſpeculum, receded from the middle, and left the ſpot white to the center; and, meaſuring the diameter of the rings, he found them to be in the ſame proportion.† Alſo, applying the priſm, he diſcovered more of theſe circles than he could by his naked eye, exactly as in the former caſe. §

By the anology of all theſe phenomena with thoſe of the like rings of colours deſcribed before, it ſeemed to him, that they were produced by this thick plate of glaſs, in a manner ſimilar to thoſe that were produced by very thin plates. For he found that if the quick ſilver was rubbed off from the back of the ſpeculum, the glaſs alone would cauſe the ſame rings, but much more faint than before; ſo that the phenomenon did not depend upon the quick ſilver, except ſo far as, by increaſing the reflexion at the back of the glaſs, it increaſed the light of the coloured rings. He alſo found that a ſpeculum of metal without glaſs produced none of thoſe rings; which made him conclude, that they did not ariſe from one ſurface only, but depended upon the two ſurfaces of the plate of glaſs, of which the ſpeculum was made, and upon the thickneſs of the glaſs between them.*

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* Theoria, p. 265. † Ib. p. 267. § Ib. p. 268.

Upon the whole, after comparing the rings made by the thin plates with thoſe which were made by this thick one, it appeared to him that they had one and the ſame origin, with this difference, that thoſe of the thin plates are made by the alternate reflexions and tranſmiſſions of the rays at the ſecond ſurface of the plate, after one paſſage through it; but that, in this caſe, the rays go through the plate and return before they are alternately reflected and tranſmitted. Firſt they go through it from the firſt ſurface to the quick ſilver, and then return through it from the quick ſilver to the firſt furface, and there are either tranſmitted to the paper, or reflected back to the quick ſilver, according as they are diſpoſed to be reflected or tranſmitted, when they arrive at that ſurface.†

He then proceeded to meaſure the thickneſs of that plate of glaſs, and finding it to be exactly a quarter of an inch, he ſhews, by computation, that his theory was juſt; and thus he ſatisfied himſelf that (theſe rings, being of the ſame kind, and having the ſame origin with thoſe of the thin plates) the alternate diſpoſitions of the rays of light to be reflected or tranſmitted are propagated to great diſtances from every reflecting and refracting ſurface.§

But, to put the matter out of all doubt, he procured another concavo-convex plate, of glaſs $\frac{5}{62}$ parts of an inch thick; and, by comparing the appearance of the colours made by this glaſs with the thickneſs of it, he was ſtill more confirmed in his hypotheſis.

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* Theoria, p. 272. † Ib. p. 274. § Ib. p. 280.

In the preceding experiments we have ſeen that our author produced circles of colours reſembling thoſe of the under ſide of the thin plates; but by diverſifying his apparatus, he ſucceeded ſo far, as to produce them in the ſame order in which they are exhibited on the upper ſide of the ſame plates.

When the beam of the ſun's light was reflected back from the ſpeculum, not directly to the hole in the window, but to a place a little diſtant from it, the common center of that ſpot, and of all the rings of colours, fell in the middle way of the beam of the incident light and the beam of the reflected light, and conſequently in the center of the ſpherical concavity of the ſpeculum; whenever the paper on which the rings fell was placed in that center. And as the beam of reflected light, by inclining the ſpeculum, receded more and more from the beam of incident light, and from the common center of the coloured rings between them, thoſe rings grew larger and larger; as alſo did the white round ſpot, and new rings of colours emerged ſucceſſively out of their common center, while the white ſpot became a white ring ſurrounding them, and the incident and the reflected beams of light always fell upon the oppoſite parts of this white ring, illuminating its circumference, like two mock ſuns in the oppoſite parts of an iris. The diameter of this ring, therefore, meaſured from the middle of its light on the one ſide to the middle of its light on the other ſide, was always equal to the diſtance between the middle of the incident beam of light, and the middle of the reflected beam, meaſured at the paper on which the rings appeared; and the rays which formed this ring were reflected by the ſpeculum in angles equal to their angles of incidence, and conſequently to their angles of refraction at their entrance into the glaſs; but theſe angles of reflexion were not in the ſame plane with the angles of incidence.

The colours of theſe new rings were in a contrary order to thoſe of the former. He then deſcribes, very particularly, the manner in which theſe colours aroſe with the increaſe of the diſtance between the incident and reflected beams of light.

Theſe, ſays he, are the phenomena of thick concavo-convex plates of glaſs, which are every where of the ſame thickneſs. There are yet, he ſays, other phenomena, when theſe plates are a little thicker on one ſide than the other, and others when the plates are more or leſs concave than convex or plano convex, or double convex. For in all theſe caſes the plates made rings of colours, but after various manners, all of which, as far as he could obſerve, agreed with his general hypotheſis laid down before; but the phenomena being very various, and the calculations exceedingly intricate, he contented himſelf with having proſecuted theſe phenomena ſo far as to diſcover the cauſe of them, and thereby confirm his general concluſions concerning this ſubject.*

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* Newton's Opticks, p. 288.

SECTION VII.

Observations on the INFLECTION of light.

IT could not be ſuppoſed that the curious obſervations of Dr. Hooke and Father Grimaldi, concerning the *diffraction*, or, as it has ſince been called, the *inflection* of light, would eſcape the notice of Sir Iſaac Newton, who gave ſo much attention to the ſubject of light and colours; and we find, in the laſt book of his Opticks, that he repeated thoſe experiments with the greateſt care, diverſified them, and purſued them much farther than they had done. So little uſe had been made of Grimaldi's obſervations, that all philoſophers before Newton had aſcribed the broad ſhadows, and even the fringes of light which he deſcribed, to the ordinary refraction of the air; but we ſhall ſee them placed in a very different point of view by our author. As the experiments which Sir Iſaac Newton made relating to the inflection of light exhibit a great variety of new appearances, and the concluſions he draws from them are of great importance in the theory of light and colours, I do not think I can diſcharge my duty as an hiſtorian, without reciting a very conſiderable part of the third book of his Opticks. Beſides, as theſe experiments were the laſt that he made, and are acknowledged by himſelf to be incomplete, it is the more neceſſary to be particular in relating the circumſtances of them; that others may be enabled to repeat them, and carry them ſtill farther.

He made in a piece of lead a ſmall hole with a pin, the breadth of which was the 42d part of an inch. Through this hole he let into his darkened chamber a beam of the ſun's light, and found that the ſhadows of hairs, and other ſlender ſubſtances, placed in it, were conſiderably broader than they would have been, if the rays of light had paſſed by thoſe bodies in right lines. He therefore concluded that they muſt have paſſed as they are repreſented in fig. 92, in which X repreſents a ſection of the hair, and AD, BE, &c. rays of light paſſing by at different diſtances, and then falling upon the wall GQ. Since, when the paper which receives the rays is at a great diſtance from the hair, the ſhadow is broad, it muſt follow, as he obſerves, that the hair acts upon the rays of light at ſome conſiderable diſtance from it; the action being ſtrongeſt on thoſe rays which are at the leaſt diſtance, and growing weaker and weaker on thoſe which are farther off, as is repreſented in this figure; and from hence it comes to paſs, that the ſhadow of the hair is much broader in proportion to the diſtance of the paper from the hair when it is nearer, than when it is at a great diſtance.

He found that it was not material whether the hair was ſurrounded with air, or with any other pellucid ſubſtance; for he wetted a poliſhed plate of glaſs, and laid the hair in the water upon the glaſs, and then laying another poliſhed plate of glaſs upon it, ſo that the water might fill up the ſpace between the glaſſes, and holding them in the beam of light, he found the ſhadow, at the ſame diſtances was as big as before. Alſo the ſhadows of ſcratches made in poliſhed plates of glaſs, and the veins in the glaſs, caſt the like broad ſhadows, ſo that this breadth of ſhadow muſt proceed from ſome other cauſe than the refraction of the air.

The ſhadows of all bodies, metals, ſtones, glaſs, wood, horn, ice, &c. in this light were bordered with three parallel fringes, or bands of coloured light, of which that which was contiguous to the ſhadow was the broadeſt and moſt luminous, while that which was the moſt remote was the narroweſt, and ſo faint, as not eaſily to be viſible. It was difficult to diſtinguiſh theſe colours, unleſs when the light fell very obliquely upon a ſmooth paper, or ſome other ſmooth white body, ſo as to make them appear much broader than they would otherwiſe have done; but in theſe circumſtances the colours were plainly viſible, and in the following order. The firſt or innermoſt fringe was violet, and deep blue next the ſhadow, light blue, green, and yellow in the middle, and red without. The ſecond fringe was almoſt contiguous to the firſt, and the third to the ſecond; and both were blue within, and yellow and red without; but their colours were very faint, eſpecially thoſe of the third. The colours, therefore, proceeded in the following order from the ſhadow, violet, indigo, pale blue, green, yellow, red; blue, yellow, red; pale blue, pale yellow, and red. The ſhadows made by ſcratches and bubbles in poliſhed plates of glaſs were bordered with the like fringes of coloured light.[*]

He alſo obſerves that, by looking on the ſun through a feather, or black ribbon, held cloſe to the eye, ſeveral rainbows will appear, the ſhadows which the fibres or threads caſt on the retina being bordered with the like fringes of colours.[†]

Meaſuring theſe fringes and their intervals with the greateſt accuracy, he found the former to be in the progreſſion of the numbers 1, $\surd\frac{1}{3}$, $\surd\frac{1}{5}$, and their intervals to be in the ſame progreſſion with them, that is, the fringes and their intervals together to be in the continual progreſſion of the numbers 1, $\surd\frac{1}{2}$, $\surd\frac{1}{3}$ $\surd\frac{1}{4}$, $\surd\frac{1}{5}$, or thereabouts. And theſe proportions held the ſame very nearly at all diſtances from the hair, the dark intervals of the fringes being as broad in proportion to the breadth of the fringes at their firſt appearance as afterwards, at great diſtances from the hair, though not ſo dark and diſtinct. *

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[*] Newton's Opticks, p. 293.

[†] Ib. p. 297.

In the next obſervation of our author we find a very remarkable and curious appearance, which we ſhould hardly have expected from the circumſtances, though it is pretty ſimilar to one that was noticed by Dr. Hooke. The ſun ſhining into his darkened chamber, through a hole $\frac{1}{4}$ of an inch broad, he placed, at the diſtance of two or three feet from the hole, a ſheet of paſteboard, black on both ſides, and in the middle of it he had made a hole about $\frac{1}{4}$ of an inch ſquare, for the light to paſs through, and behind the hole he faſtened to the paſteboard the blade of a ſharp knife, to intercept ſome part of the light which paſſed through the hole. The planes of the paſteboard and blade of the knife were parallel to one another, and perpendicular to the rays; and when they were ſo placed that none of the light fell on the paſteboard, but all of it paſſed through the hole to the knife, and there part of it fell upon the blade of the knife, and part of it paſſed by its edge, he let that part of the light which paſſed by fall on a white paper, 2 or 3 feet beyond the knife, and there ſaw two ſtreams of faint light ſhoot out both ways from the beam of light into the ſhadow, like the tails of comets. But becauſe the ſun's direct light, by its brightneſs upon the paper, obſcured theſe faint ſtreams, ſo that he could ſcarce ſee them, he made a

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* Newton's Opticks, p. 300.

little hole in the midſt of the paper for that light to paſs through and fall on a black cloth behind it; and then he ſaw the two ſtreams plainly. They were like one another, and pretty nearly equal in length, breadth, and quantity of light. Their light, at that end which was next to the ſun's direct light, was pretty ſtrong for the ſpace of about $\frac{1}{4}$ of an inch, or $\frac{1}{2}$ an inch, and decreaſed gradually, till it became inſenſible.

The whole length of either of theſe ſtreams, meaſured upon the paper, at the diſtance of 3 feet from the knife, was about 6 or 8 inches, ſo that it ſubtended an angle, at the edge of the knife, of about 10 or 12, or at moſt 14 degrees. Yet ſometimes he thought he ſaw it ſhoot 3 or 4 degrees farther, but with a light ſo very faint, that he could hardly perceive it. This light he ſuſpected might, in part at leaſt, ariſe from ſome other cauſe than the two ſtreams. For, placing his eye in that light, beyond the end of that ſtream which was behind the knife, and looking towards the knife, he could ſee a line of light upon its edge, and that not only when his eye was in the line of the ſtreams, but alſo when it was out of that line, either towards the point of the knife, or towards the handle. This line of light appeared contiguous to the edge of the knife, and was narrower than the light of the innermoſt fringe, and narroweſt when his eye was fartheſt from the direct light; and therefore ſeemed to paſs between the light of that fringe and the edge of the knife; and that which paſſed neareſt the edge ſeemed to be moſt bent, though not all of it.

He then placed another knife by the former, ſo that their edges might be parallel, and look towards one another, and that the beam of light might fall upon both the knives, and ſome part of it paſs between their edges. In this ſituation he obſerved that, when the diſtance of their edges was about the 400th part of an inch, the ſtream divided in the middle, and left a ſhadow between the two parts. This ſhadow was ſo black and dark, that all the light which paſſed between the knives ſeemed to be bent and turned aſide to the one hand or to the other; and as the knives ſtill approached one another, the ſhadow grew broader, and the ſtreams ſhorter, next to it, till, upon the contact of the knives, all the light vaniſhed.

From this experiment our author concludes, that the light which is leaſt bent, and which goes to the inward ends of the ſtreams, paſſes by the edges of the knives at the greateſt diſtance; and this diſtance, when the ſhadow began to appear between the ſtreams, was about the 800th part of an inch; and the light which paſſed by the edges of the knives at diſtances ſtill leſs and leſs was more and more faint, and went to thoſe parts of the ſtreams, which were farther from the direct light; becauſe when the knives approached one another till they touched, thoſe parts of the ſtreams vaniſhed laſt which were fartheſt from the direct light.

In the experiment of one knife only, the coloured fringes did not appear; but, on account of the breadth of the hole in the window, became ſo broad as to run into one another, and, by joining, to make one continual light in the beginning of the ſtreams; but in the laſt experiment, as the knives approached one another, a little before the ſhadow appeared between the two ſtreams, the fringes began to appear on the inner ends of the ſtreams, on either ſide of the direct light, three on one ſide, made by the edge of one knife, and three on the other ſide, made by the edge of the other knife. They were the moſt diſtinct when the knives were placed at the greateſt diſtance from the hole in the window, and became ſtill more diſtinct by making the hole leſs; ſo that he could ſometimes ſee a faint trace of a 4th fringe, beyond the three above mentioned; and as the knives approached one another, the fringes grew more diſtinct and larger, till they vaniſhed; the outermoſt vaniſhing firſt, and the innermoſt laſt. After they were all vaniſhed, and the line of light which was in the middle between them was grown very broad, extending itſelf on both ſides into the ſtreams of light deſcribed before, the above mentioned ſhadow began to appear in the middle of this line, and to divide it along the middle into two lines of light, and increaſed till all the light vaniſhed. This enlargement of the fringes was ſo great, that the rays which went to the innermoſt fringe ſeemed to be bent about 20 times more when the fringe was ready to vaniſh, than when one of the knives was taken away.

From both theſe experiments compared together, our author concluded, that the light of the firſt fringe paſſed by the edge of the knife at a diſtance greater than the 800th part of an inch, that the light of the ſecond fringe paſſed by the edge of the knife at a greater diſtance than the light of the firſt fringe, and that of the third at a greater diſtance than that of the ſecond; and that the light of which the ſtreams above mentioned conſiſted, paſſed by the edges of the knives at leſs diſtances than that of any of the fringes.

He then got the edges of two knives ground truly ſtraight, and pricking their points into a board, ſo that their edges might look towards one another, and, meeting near their points, contain a rectilinear angle, he faſtened their handles together, to make the angle invariable. The diſtance of the edges of the knives from one another, at the diſtance of 4 inches from the angular point, where the edges of the knives met, was the 8th part of an inch, ſo that the angle contained by their edges was about 1° 54′. The knives being thus fixed together, he placed them in a beam of the ſun's light let into his darkened chamber, through a hole the 42th part of an inch wide, at the diſtance of 10 or 15 feet from the hole; and he let the light which paſſed between their edges fall very obliquely on a ſmooth white ruler, at the diſtance of ½ an inch, or an inch from the knives; and there he ſaw the fringes made by the two edges of the knives run along the edges of the ſhadows of the knives, in lines parallel to thoſe edges, without growing ſenſibly broader, till they met in angles equal to the angle contained by the edges of the knives; and where they met and joined they ended, without croſſing one another. But if the ruler was held at a much greater diſtance from the knives, the fringes, where they were farther from the place of their meeting, were a little narrower, and they became ſomething broader as they approached nearer to one another, and after they met they croſſed one another, and then became much broader than before.

From theſe obſervations he concluded, that the diſtances at which the light compoſing the fringes paſſed by the knives were not increaſed or altered by the approach of the knives, but that the angles in which the rays were there bent were much increaſed by that approach, and that the knife which was neareſt to any ray determined which way the ray ſhould be bent, but that the other knife increaſed the bending.

When the rays fell very obliquely upon the ruler, at the diſtance of a third part of an inch from the knives, the dark line between the firſt and ſecond fringe of the ſhadow of one knife, and the dark line between the firſt and ſecond fringe of the ſhadow of the other knife met one another, at the diſtance of the 5th part of an inch from the end of the light which paſſed between the knives, where their edges met one another; ſo that the diſtance of the edges of the knives, at the meeting of theſe dark lines, was the 160th part of an inch; and one half of that light paſſed by the edge of one knife, at a diſtance not greater than the 320th part of an inch, and, falling upon the paper, made the fringes of the ſhadow of that knife; while the other half paſſed by the edge of the other knife, at a diſtance not greater than the 320th part of an inch, and falling upon the paper, made the fringes of the ſhadow of the other knife. But if the paper was held at a diſtance from the knives greater than the 3d part of an inch, the dark lines above mentioned met at a greater diſtance than the fifth part of an inch from the end of the light which paſſed between the knives, at the meeting of their edges; ſo that the light which fell upon the paper where thoſe dark lines met paſſed between the knives, where their edges were farther diſtant than the 160th part of an inch. For at another time, when the two knives were 8 feet and 5 inches from the little hole in the window, the light which fell upon the paper where the above mentioned dark lines met paſſed between the knives, where the diſtance between their edges was, as in the following table, at the diſtances from the paper there noted.

Diſtances of the paper from the knives in inches.	Diſtances between the edges of the knives in milleſimal parts of an inch.
1 $\frac{1}{2}$	0,012
3 $\frac{1}{3}$	0,020
8 $\frac{3}{5}$	0,034
32	0,057
96	0,081
131	0,087

From theſe obſervations he concluded that the light which makes the fringes upon the paper is not the ſame light, at all diſtances of the paper from the knives; but that when the paper is held near the knives, the fringes are made by light which paſſes by the edges of the knives at a leſs diſtance, and is more bent than when the paper is held at a greater diſtance from the knives.

When the fringes of the ſhadows of the knives fell perpendicularly upon the paper, at a great diſtance from the knives, they were in the form of hyperbola's, their dimenſions being as follows. Let CA, CB fig. 93, repreſent lines drawn upon the paper, parallel to the edges of the knives; and between which all the light would fall if it ſuffered no inflection. DE is a right line drawn through C, making the angles ACD, BCE equal to one another, and terminating all the light which falls upon the paper, from the point where the edges of the knives meet. Then *e i s*, *f k t*, *and g l v*, will be three hyperbolical lines, repreſenting the boundaries of the ſhadow of one of the knives, the dark line between the firſt and ſecond fringes of that ſhadow, and the dark line between the ſecond and third fringes of the ſame ſhadow. Alſo *x i p*, *y k q*, *and z l r* will be three other hyperbolical lines, repreſenting the boundaries of the ſhadow of the other knife, the dark line between the firſt and ſecond fringes of that ſhadow, and the dark line between the ſecond and third fringes of the ſame ſhadow. Theſe three hyperbola's are ſimilar, and equal to the former three, and croſs them in the points *i*, *k*, and *l*; ſo that the ſhadows of the knives are terminated, and diſtinguiſhed from the firſt luminous fringes, by the lines *e i s* and *x i p*, till the meeting and croſſing of the fringes; and then thoſe lines croſs the fringes in the form of dark lines terminating the firſt luminous fringes on the inſide, and diſtinguiſhing them from another light, which begins to appear at *i*, and illuminates all the triangular ſpace *i p* DE *s*, comprehended by thoſe dark lines and the right line DE. Of theſe hyperbola's one aſymptote is the line DE, and the other aſymptotes are parallel to the lines CA and CB.*

The ſun ſhining into his darkened room through the ſmall hole mentioned above, he placed at the hole a priſm to refract the light, and to form, on the oppoſite wall, the coloured image of the ſun; and he found that the ſhadows of all bodies, held in the coloured light between the priſm and the wall, were bordered with fringes of the colour of that light in which they were held; and comparing the fringes made in the ſeveral coloured lights, he found that thoſe made in the red light were the largeſt, thoſe made in the violet were the leaſt, and thoſe made in the green were of a middle bigneſs. For the fringes with which the ſhadow of a man's hair were bordered, being meaſured croſs the ſhadow, at the diſtance of ſix inches from the hair, the diſtance between the middle and moſt luminous part of the firſt or innermoſt fringe on one ſide of the ſhadow, and that of the like fringe on the other ſide of the ſhadow, was, in the full red light $\frac{1}{37\frac{1}{4}}$ of an inch, and in the full violet $\frac{1}{46}$. The like diſtance between the middle and moſt luminous parts of the ſecond fringes, on either ſide of the ſhadow, was in the full red light $\frac{1}{22}$, and the violet $\frac{1}{27}$ of an inch; and theſe diſtances of the fringes held the ſame proportion at all diſtances from the hair, without any ſenſible variation.

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* Newton's Opticks, p. 309.

From theſe obſervations it was evident, that the rays which made the fringes in the red light, paſſed by the hair at a greater diſtance than thoſe which made the like fringes in the violet; ſo that the hair, in cauſing theſe fringes, acted alike upon the red light or leaſt refrangible rays, at a greater diſtance, and upon the violet or moſt refrangible rays at a leſs diſtance; and thereby occaſioned fringes of different ſizes, without any change in the colour of any ſort of light.

It may, therefore, be concluded, that when the hair in the firſt obſervation was held in the white beam of the ſun's light, and caſt a ſhadow which was bordered with three fringes of coloured light, thoſe colours aroſe not from any new modifications impreſſed upon the rays of light by the hair, but only from the various inflections whereby the ſeveral ſorts of rays were ſeparated from one another, which before ſeparation, by the mixture of all their colours, compoſed the white beam of the ſun's light; but when ſeparated, compoſed lights of the ſeveral colours which they are originally diſpoſed to exhibit.*

When our author made the forgoing experiments, he deſigned to repeat moſt of them with more care and exactneſs, and to make ſome new ones, to determine the manner in which the rays are bent in their paſſage by bodies, for making the fringes of colours with the dark lines between them, but he was then interrupted, and never purſued the ſubject any farther. In his *Optical queries*, however, we find a few relating to the ſubject of inflection, which I ſhall recite in this place; obſerving that the preceding obſervations ſeem to authorize us to anſwer them all in the affirmative.

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* Newton's Opticks, p. 311.

1. Do not bodies act upon light at a diſtance, and by this action bend its rays; and is not this action the ſtrongeſt at the leaſt diſtance?

2. Do not the rays which differ in refrangibility differ alſo in inflexibility, and are they not, by their different inflections, ſeparated from one another, ſo as to make the coloured fringes above mentioned; and after what manner are they inflected to make thoſe fringes?

3. Are not the rays of light, in paſſing by the edges and ſides of bodies, bent ſeveral times backwards and forwards, with a motion like that of an eel; and do not the three fringes of coloured light above mentioned ariſe from three ſuch bendings?

4. Do not the rays of light which fall upon bodies, and which are reflected or refracted, begin to bend before they arrive at the bodies; and are they not reflected, refracted, and inflected by one and the ſame principle, acting differently in different circumſtances?

SECTION VIII.

Miſcellaneous Articles.

CHAPTER I.

Of the PHYSICAL CAUSE of the reflexion, refraction, and inflection of light.

DESCARTES ſeems to have been the firſt perſon who attempted to inveſtigate the phyſical cauſe of the reflexion and refraction of light; but both he and his opponents went upon the ſuppoſition of light meeting with a reſiſtance at its entrance into all bodies; and they differed only with reſpect to the proportional quantity of reſiſtance in different bodies. It never occurred to any perſon before Sir Iſaac Newton, that reflexion and refraction may be cauſed by *powers of repulſion and attraction*, belonging to bodies, and extending to a certain diſtance beyond their ſurfaces. Upon this ſuppoſition, and that of light conſiſting of particles emitted from luminous bodies, he demonſtrates, in his *Principia*, that the ſine of the angle of incidence muſt always be to the ſine of the angle of refraction in ſome certain ratio. For, as he proves geometrically, if two ſimilar mediums be ſeparated from each other by a ſpace terminated on both ſides by parallel planes, and a body, in its paſſage through that ſpace, be attracted or impelled perpendicularly towards either of thoſe mediums, and not agitated or hindered by any other force; and if the attraction be every where the ſame, at equal diſtances from either plane, taken towards the ſame hand of the plane, the ſine of incidence upon either plane will be to the ſine of emergence from the other plane in a given ratio.*

In a corollary to this propoſition, he alſo ſhews, that if, inſtead of one parallel ſpace, bodies be ſurrounded with ſeveral, or an infinite number of them, the attractive power of each of which differs from the next, increaſing or decreaſing, ſtill, the ratio reſpecting any two contiguous ones being given, the ratio of the extremes will be given.

He alſo demonſtrates that, if the cauſe of refraction be the attraction of the refracting medium, the velocity of light before its incidence will be to its velocity afterwards, as the ſine of the angle of refraction is to the ſine of the angle of incidence; ſo that light in paſſing from a rarer medium into a denſer, receives an increaſe of velocity.

The ſame things being ſuppoſed, he ſhews that if the velocity before incidence be greater than afterwards, ſo that the angle of emergence muſt always increaſe, in conſequence of the repulſive power prevailing over the attractive one, the body will at length be reflected; and that, in this caſe, the angle of reflexion muſt neceſſarily be equal to the angle of incidence.

That light does not conſiſt of any mode of action upon a fluid medium, he concludes from having demonſtrated that preſſure cannot be propagated through a fluid in rectilinear directions, unleſs where the particles of the fluid lie in a right line, but muſt diverge into the unmoved ſpaces; and that every tremulous body, in an elaſtic medium, propagates the motion of the pulſes on every ſide ſtraight forwards.* Admitting this, there is hardly any other hypotheſis that will agree with the phenomena of light, but that of particles emitted from the luminous body, affected by the attractions and repulſions of other bodies. There is no other hypotheſis, therefore, but that, to which the above mentioned propoſitions are applicable.

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* Newton's Principia, by Motte, Vol. 1, p. 311.

Having explained the ſentiments of Sir Iſaac Newton on the ſubject of reflexion, refraction, and inflection, I ſhall juſt take notice of ſome other theories that have been advanced ſince the publication of his. Among theſe, that of M. Leibnitz, the great rival of Newton in the buſineſs of fluxions, and indeed the only foreigner of all his cotemporaries, whoſe name is ever mentioned in compariſon of him, deſerves the firſt notice. Being diſſatisfied with the theory of Deſcartes, he adopted the opinion of M. Fermat, as has been obſerved already; maintaining that light proceeds from one point to another, in that way which is the eaſieſt for it. But, beſides what was obſerved before, Dr. Smith ſhews that his hypotheſis only agrees to the caſe of refraction at a plain ſurface; and that, had he proceeded the leaſt ſtep farther than he did, and endeavoured to accommodate his principle to concave and convex ſurfaces, as he talked of doing at firſt, he would ſoon have perceived the inſufficiency of it. Beſides it is not eaſy to underſtand what this author means by *eaſy* and *difficult*, applied to the paſſage of rays of light; but, be they what they will, they muſt both of them be nothing at all in vacuo, and therefore the eaſieſt way for a ray to paſs from a given point in any reſiſting medium into a vacuum, is to go in the perpendicular to the refracting ſurface, as being the ſhorteſt way, through any difficulty or reſiſtance whatever; which, being quite over at the refracting ſurface, it may then take any other courſe in vacuo, without any farther difficulty; and, on the contrary, in returning back from the vacuum into the denſe medium, it muſt take the ſhorteſt courſe through the ſame perpendicular, as before. Thus, when the ſun ſhines upon the atmoſphere, all his rays ſhould be refracted into lines tending to the center of the earth, as being the ſhorteſt and eaſieſt way through the atmoſphere, and then we ſhould ſee the ſun exactly over our heads, in all places and in all times. But, ſays he, it is no wonder that ſtrange conſequences ſhould follow from an arbitrary hypotheſis.*

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* Newton's Principia, by Motte, Vol. 2, p. 163, 167.

Both the Bernoulli's, father and ſon, have attempted to explain the cauſe of refraction on mechanical principles, the former on the equilibrium of forces, and the latter on the ſame principles, with the ſuppoſition of etherial vortices; but neither of their hypotheſis have gained credit enough to make it worth my while to explain them. M. Mairan, alſo, attempted the explanation of this ſubject upon the ſuppoſition of a ſubtle fluid, filling the pores of all bodies, and extending, like an atmoſphere, to a ſmall diſtance beyond their ſurfaces; and then he ſuppoſes that the refraction of light is nothing more than the neceſſary and nechanical effect of the incidence of a ſmall body in thoſe circumſtances. There is more, he ſays, of the refracting fluid in water than in air, leſs in water than in glaſs, and in general leſs in a denſe medium than in one that is rarer. §

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* Smith's Opticks, Remarks, p. 70.

§ Saverien's Dictionaire, Vol. 2, p. 371.

Dr. Smith obſerves, that all other theories for explaining the reflexion and refraction of light, except Sir Iſaac Newton's, ſuppoſe that it ſtrikes upon bodies and is reſiſted by them; which has never been proved by any deduction from experience. On the contrary, it appears by various conſiderations, and might be ſhewn by Mr. Molyneux and Profeſſor Bradley's obſervations on the parallax of the fixed ſtars, which will be recited hereafter, that their rays are not at all impelled by the rapid motion of the earth's atmoſphere, nor by the object glaſs of the teleſcope through which they paſs. And by Sir Iſaac Newton's theory of refraction, which is grounded upon experience only, it appears that light is ſo far from being reſiſted and retarded by refraction into any denſe medium, that it is ſwifter there than in vacuo, in the ratio of the ſine of incidence in vacuo, to the ſine of refraction into the denſe medium.*

M. De Maupertuis, after examining and rejecting the rules which ſeveral other philoſophers have given concerning the refraction of light, ſuppoſes that the courſe which every ray takes, in paſſing from one medium to another, is that which requires the leaſt quantity of action, which depends upon the velocity of the body and the ſpace it paſſes over; ſo that it is in proportion to the ſum of the ſpaces multiplied by the velocity with which bodies paſs over them. From this principle he deduces the neceſſity of the ſine of the angle of incidence being in a conſtant proportion to that of refraction, and alſo all the other laws relating to the propagation and reflexion of light.§

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* Smith's Opticks, Remarks, p. 70.

§ Ac. Par. 1743. M. p. 573.

CHAPTER II.

Methods of measuring the refractive power of different substances.

CONSIDERING that the new doctrine of the different refrangibility of the rays of light would require a more accurate meaſure of the proportion between the angles of reflex- and refraction of different mediums, our author contrived ſome practical methods to aſcertain it, both with reſpect to fluid and ſolid ſubſtances, of which he gives a full deſcription in his *Optical lectures*, p. 51; but the following account of them is taken, very nearly, from Dr. Smith.

The firſt of his methods was to fill a ſmall deep veſſel with the liquor or ſubſtance, on which the experiment was to be made. This he faſtened to a ſquare beam of wood, of a conſiderable length, its oppoſite ſides being perfectly plane and parallel; and a quadrant was fixed on the ſide oppoſite to the veſſel; that when the ray of light was refracted through this veſſel, exactly parallel to the beam, the index of the quadrant might ſhew the angle in which it was inclined; which being compared with the ſun's altitude, would ſhew the angle of refraction compared to that of incidence. A ſufficient idea of this method may be conceived by the inſpection of fig. 94, in which CF repreſents the veſſel containing the refracting ſubſtance, HK the beam to which it is faſtened, and *k r* the plumb of the quadrant. FR ſhews the direction of the refracted ray, from a ſmall hole in the bottom of the veſſel, to a mark at the end of the beam, at exactly the ſame diſtance from the ſide of it.

But the moſt accurate method of determining the ratio of refraction is the following, by means of a priſm. It was obſerved before, that when the axis of the priſm is held perpendicular to the ſun's rays, and the refractions are made upwards, by turning the priſm ſlowly about its axis, the refracted light, or the ſun's coloured image caſt upon a wall, will firſt deſcend and then aſcend, during the ſame continued rotation of the priſm. Between the aſcent and deſcent, when the image is ſtationary, let the priſm be ſtopped, and remain fixed in this poſture, and then the refractions of the rays, at their ingreſs and egreſs on each ſide of the priſm, will be equal. In this poſition of the priſm, the angle of refraction, at the ingreſs of the ray, is equal to half the refracting angle of the priſm, * which may be meaſured by laying two rulers croſs each other, upon the ſide of a ſmooth table, as in fig. 95, for then two lines drawn upon the table, by the ſides of each ruler, will give the refracting angle, which may then be meaſured by a ſector.

The priſm being placed in the manner directed above, let the altitudes of the incident and emergent rays SD, EP, fig. 96, be taken with the quadrant, and the angle of incidence, SDL, will be equal to half the ſum of thoſe altitudes.

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* M. Beguelin obſerves, that it is not exactly true, that the half of the angle of depreſſion, joined to the refracting angle of the priſm, gives the angle of refraction; for that it is only in neglecting the thickneſs of the priſm that this will be the caſe; but that this thickneſs may be neglected at conſiderable diſtances. Ac. Berl 1762, p. 69.

If the ſun be more elevated, till the emergent ray EP becomes parallel to the horizon, one of the angles above mentioned will vaniſh; and if the ſun be ſtill higher, the emergent ray will tend downwards, in which caſe half the difference of the altitudes of the incident and emergent rays muſt be added to half the refracting angle of the priſm, to get the angle of incidence.

Sir Iſaac Newton has given us the following example of this method.* In a glaſs priſm, whoſe refracting angle was 62 ½ degrees, the half of it, 31°. 15′, was the angle of refraction into the priſm, the ſine of which was 5188, the radius being 10000. When the axis of the priſm was parallel to the horizon, and the ſun's image upon the wall was at its limit of regreſſion, he obſerved with the quadrant the angle which the mean refrangible rays made with the horizon; and by adding this angle to the ſun's altitude, obſerved at the ſame time, he found the angle PIM, made by the incident and emergent rays, to be 44° 40′; the half of which, 22° 20′, being added to the angle of refraction, 31° 15′, makes the angle of incidence 53° 35′, the ſine of which is 8047; and the ratio of theſe ſines in round numbers is 20 to 31.

The excellency of this method appears from theſe conſiderations, that it requires no other inſtruments, but a quadrant and a priſm; that the refraction of the ray being double, an error in the practice, is but half of what it would be in a ſingle refraction; that it is very eaſy to place the priſm in the required poſition, and that a ſmall deviation from it does not alter the place of the image, or the ſum of the two refractions, as is evident upon trial, and becauſe this ſum is the leaſt of all others.*

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* Opticks, p. 71.

If a priſmatick veſſel be made of wood, and two oppoſite holes, for the light to paſs through, be made any where in the ſides containing the refracting angle, and if pieces of broken looking-glaſs be cemented over the outſides of the holes, and be ſet exactly to a right angle (as being the eaſieſt to be examined by a ſquare) and the veſſel be filled with water, and the like experiment be repeated with this priſm as was tried before with that of glaſs, the refractive power of water will be found. For the incident and emergent rays in the air will be inclined to the intermediate rays within the water in the very ſame angles, as they would have been if the water had been contiguous to the air. Sir Iſaac Newton, by this accurate method, found the ratio of refraction of the red rays out of air into water to be 4 to 3.§ It was in this method alſo that Mr. Haukſbee determined the refractive power of air, as was mentioned above.

The firſt accurate table of the refractive power of the atmoſphere, at every different degree of altitude, was made by Sir Iſaac Newton, and was publiſhed by Dr. Halley. It may be ſeen in Phil. Tranſ. ab. Vol. 6, p. 160. It makes the horizontal refraction 33′. 45″, that of 1 degree 23′. 7″, and that of 75 degrees, at which the table ends, 15″. In the tables of M. De la Hire, the firſt of theſe is 32′. 20″, the ſecond 27. 56, and the third 16″.†

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* Smith's Opticks, Remarks, p. 67. § Ib. p. 68. † Saverien's Dictionaire, Vol. 2, p. 372.

CHAPTER

CHAPTER III.

Sir Isaac Newton's Improvements in TELESCOPES.

TILL the time of Sir Iſaac Newton, no optician had imagined that the object glaſſes of teleſcopes were ſubject to any error except what aroſe from their ſpherical figure, and therefore they had directed all their efforts to the conſtruction of them with other kinds of curvature; but Newton had no ſooner demonſtrated the different refrangibility of the rays of light, than he diſcovered in this circumſtance a new and a much more conſiderable cauſe of error in teleſcopes.

Since, therefore, the pencils of each kind of light have their foci in different places, ſome nearer and ſome farther from the lens, it is evident that the whole beam cannot be brought into any one point, but that it will be drawn the neareſt to a point in the middle place between the focus of the moſt and of the leaſt refrangible rays, ſo that the focus will be a circular ſpace of a conſiderable diameter. Thus fig. 97, repreſents a beam of light paſſing through a lens, the moſt refrangible of the rays having their focus at F, and the leaſt refrangible at *f*. Now it is evident, from the figure, that the ſmalleſt ſpace into which the light can be drawn is a circle, the diameter of which is *i* I, paſſing through G, the middle point between F and *f*. Newton ſhews that this ſpace is about the 55th part of the aperture of the teleſcope, and that the focus of the moſt refrangible rays is nearer to the object glaſs, than the focus of the leaſt refrangible ones, by about the $27\frac{1}{2}$th part of the diſtance between the object glaſs and the focus of the mean refrangible rays.* But he ſays that, if the rays flow from a lucid point, as far from the lens on one ſide as their focus is on the other, the focus of the moſt refrangible rays will be nearer to the lens than that of the leaſt refrangible ones, by more than the 14th part of the whole diſtance.§ He alſo verifies the former of theſe concluſions by an accurate menſuration of the diſtance of the focus of the different kinds of light, throwing them ſeparately on a printed book, and obſerving at what diſtances the letters appeared well defined.

From all theſe obſervations he concludes, that, if all the rays of light were equally refrangible, the error in teleſcopes, ariſing from the ſpherical figure of the glaſs, would be many hundred times leſs than it now is; for that the error ariſing from the ſpherical figure of the glaſs, is to the error ariſing from the different refrangibility of the rays of light as 1 to 5449; and upon the whole he obſerves, that it is a wonder that teleſcopes repreſent objects ſo diſtinctly as they do. The reaſons which he gives why this indiſtinctneſs of viſion is not ſo ſenſible in them are curious, and well worth reciting. It is, ſays he,† becauſe the diſperſed rays are not ſcattered uniformly over all the circular ſpace mentioned above, but are infinitely more denſe in the center than in any other part of the circle; and that, in the way from the center to the circumference, they grow continually rarer and rarer, and at their circumference are infinitely rare; ſo that theſe diſperſed rays are not copious enough to be viſible, except in the center of the circle, or very near it.

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* Newton's Opticks, p. 73. § Ib. p, 83. † Ib. p. 85.

He alſo obſerves that the moſt luminous of the priſmatick colours are the yellow and the orange. Theſe, ſays he, affect the ſenſes more ſtrongly than all the reſt put together, and next to theſe in ſtrength are the red and the green. The blue, compared to theſe, is a faint and dark colour, and the indigo and violet are much darker and fainter; ſo that theſe, compared with the ſtronger colours, are little to be regarded. The images of objects are, therefore, to be placed not in the focus of the mean refrangible rays, which are in the confine of green and blue, but in the focus of thoſe rays which are the middle of the orange and yellow, where the colour is the moſt luminous, that is, in the brighteſt yellow, which inclines more to orange than to green. By the refraction of theſe rays, whoſe ſines of incidence and refraction in glaſs are as 17 and 11, the refraction of glaſs for optical uſes is to be meaſured.

If, therefore, the image of an object be placed in the focus of theſe rays, all the yellow and orange will fall within a circle, whoſe diameter is about the 250th part of the diameter of the aperture of the glaſs, and the light of all the colours that fall without this circle will be ſo much rarer than that within it, as hardly to affect the eye. It may therefore be concluded, that the ſenſible image of a lucid point is hardly broader than a circle whoſe diameter is the 250th part of the aperture of the object glaſs of a good teleſcope; ſo that, if the aperture be 4 inches, and the length of it a 100 feet, it will not exceed 2″. 45‴, or 3″; and in a teleſcope whoſe aperture is 2 inches, and the length

20 or 30 feet, it may be about 5″ or 6″; and this, he ſays, anſwers well to experience.*

Upon the whole he concludes, that, in an 100 foot teleſcope, with an aperture of 4 inches, the greateſt errors ariſing from the ſpherical figure in the glaſs would be to the greateſt ſenſible errors ariſing from the different refrangibility of the rays only as 1 to 1200; which, however, ſufficiently ſhews, that it is not the ſpherical figure of the glaſſes, but the different refrangibility of the rays of light, which hinders the perfection of teleſcopes.†

He alſo mentions another argument to prove that the different refrangibility of the rays of light is the true cauſe of the imperfection of teleſcopes. For the diſperſions of the rays ariſing from the ſpherical figures of object glaſſes are as the cubes of their apertures; and therefore, to make teleſcopes of various lengths magnify with equal diſtinctneſs, the apertures of the object glaſſes and the charges, or magnifying powers, ought to be as the cubes of the ſquare roots of their lengths, which doth not anſwer to experience. But the errors of the rays ariſing from the different refrangibility are as the apertures of the object glaſſes, and thence to make teleſcopes of various lengths magnify with equal diſtinctneſs, their apertures and charges ought to be as the ſquare roots of their lengths; and this anſwers to experience, as is well known. For inſtance, a teleſcope of 64 feet in length, with an aperture of 2⅔ inches, magnifies about 120 times, with as much diſtinctneſs as one of a foot in length, with ⅓ of an inch aperture, magnifies 15 times.

Now, were it not for this different refrangibility of the rays, teleſcopes might be brought to a greater perfection than we have yet deſcribed, by compoſing the object glaſs of two glaſſes with water between them. For by this means the refractions on the concave ſides of the glaſſes will very much correct the errors of the refractions on the convex ſides, ſo far as they ariſe from their ſpherical figure. By this means, he ſays, that teleſcopes might be brought to ſufficient perfection, were it not for the different refrangibility of different kinds of rays; but on this account he did not ſee any other means of improving teleſcopes by refraction only, except by increaſing their length. He therefore greatly commends the contrivance of M. Huygens, whereby object glaſſes of a great focal length were uſed without any tube, by being faſtened upon a ſtrong upright pole, as has been deſcribed before.*

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* Newton's Opticks, p. 85. † Ib. p. 89.

Deſpairing, therefore, of carrying teleſcopes to any great degree of perfection by means of refracted light, this great man had recourſe to reflexion; and knowing, by the experience of Gregory and others, the inſuperable difficulty that attended the conſtruction of mirrors in the form of the conic ſections, he kept to the eaſier form of the ſphere, and actually conſtructed a teleſcope with a metallic ſpeculum, which magnified 30 or 40 times. But not attaining to that diſtinctneſs of viſion which he wiſhed for, on account of his not being able to give his metallic ſpeculum a ſufficient poliſh, he recommends the uſe of glaſſes ground concave on one ſide, and convex on the other, ſo as to be every where of an equal thickneſs, and quick-ſilvered on the convex ſide. This glaſs, repreſented by *a b c d*, fig. 98, he propoſed to fix at the bottom of the tube *v x y z* made very black within. *efg* repreſents a priſm of glaſs, or cryſtal,* placed near the other end of the tube, in the middle of it, and ſo that the rays of light reflected from the ſpeculum may be reflected at its baſe,, and converge to *t*, the common focus of the ſpeculum, and of a plano convex eye glaſs, *h*, through which thoſe rays muſt paſs to the eye. He alſo adviſes that the rays, at their coming out of the glaſs, ſhould paſs through a round ſmall hole made in a plate of metal, no bigger than is neceſſary for light enough to paſs through, in order to intercept the diſperſed light, which comes from the extremities of the ſpeculum, and thereby render the image more diſtinct. Such an inſtrument well made, if it be ſix feet long, reckoning from the ſpeculum to the priſm; and thence to the focus *t*, will bear an aperture of ſix inches at the ſpeculum, and magnify between 2 and 300 times. It will be convenient, he ſays, that the ſpeculum be at leaſt an inch or two broader than the aperture, and that the glaſs be ſo thick, as not to bend in the working. The priſm ſhould be no bigger than neceſſary, and the back ſide not quick-ſilvered over; ſince it may be ſo placed, as that, without quick-ſilver, it will reflect all the light that falls upon it from the ſpeculum.

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* Newton's Opticks, p. 89.

In this inſtrument, he ſays, the object will appear inverted, but it may be made erect, by making the ſides of the priſm not plane, but ſpherically convex, that the rays may croſs one another both before they come to it, and afterwards, between it and the eye glaſs. If it be deſired that the inſtrument ſhould bear a larger aperture, it may be done, he ſays, by making the ſpeculum of two glaſſes, with water between them. *

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* Sir Iſaac Newton did not ſeem to be aware that all kinds of cryſtal have a double refraction, and therefore that no cryſtal could anſwer this purpoſe.

Our author concludes this deſcription of his teleſcope with the following general obſervations on the ſubject. If the theory of making teleſcopes could be perfected, and fully reduced to practice, yet there would be certain bounds beyond which teleſcopes could not be of any uſe. For the air, through which we look upon the ſtars, is in a perpetual tremor, as may be ſeen by the tremulous motion of the ſhadows of high towers, and by the twinkling of the fixed ſtars. Theſe ſtars do not twinkle when viewed through teleſcopes, which have large apertures; for the rays of light which paſs through different parts of the aperture tremble ſeparately, and by this means fall at the ſame time upon different points in the bottom of the eye, and their tremblings are too quick and confuſed to be perceived ſeparately; and all theſe illuminated points conſtitute one broad lucid point, and thereby cauſe the ſtar to appear broader than it is, without any trembling of the whole. Long teleſcopes may cauſe objects to appear brighter and larger than ſhort ones can do, but they cannot be ſo formed as to take away all that confuſion of the rays which ariſes from the tremors of the atmoſphere. The only re-

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* It has been found, by the experience of the beſt makers of reflecting teleſcopes, that it is eaſy to make them ſo near to the form of the conic ſections, as to make them far more perfect than could poſſibly be done by any ſuch ſcheme as this of Sir Iſaac Newton's. Many errors would ariſe from the *glaſs*, greater than thoſe depending upon its ſpherical figure, particularly a conſiderable one from the bending of the glaſs by its own weight, beſides thoſe which could not but flow from inequalities in its texture, and which muſt diſturb its refraction. Beſides, ſeveral experiments have been made by Mr. Short, which ſhew, that no reflecting teleſcopes made with glaſs are comparable to theſe that are made with metal. The quantity of light reflected from them is conſiderably leſs.

medy is a moſt ſerene and quiet air, ſuch as may, perhaps, be found on the tops of the higheſt mountains, above the groſſer clouds.*

A teleſcope of Newton's, which he had ſent to the Royal Society, was examined in the preſence of the king, Dr. Hooke, and many other perſons, at Whitehall, January 11th, 1672; and they had ſo good an opinion of it, that they thought proper that a deſcription of it ſhould be ſent, by the Secretary of the Royal Society to M. Huygens, who was then at Paris; and Newton himſelf ſent a drawing and a deſcription of it for that purpoſe. A letter which he wrote to the Secretary upon that occaſion, may be ſeen in Birch's Hiſtory, Vol. 3, p, 2. At that time his election into the ſociety was depending; and he concludes his letter with ſaying that, "if he ſhould be elected, he "would endeavour to teſtify his gratitude, by communicating "what his poor and ſolitary endeavours could effect towards pro"moting their philoſophical deſign." Such was the modeſty of the man who was the glory of the ſociety, of the nation, and of the world. In January 18, his teleſcope was examined before the Royal Society, and applauded. †

When Newton had publiſhed an account of his teleſcopes in the philoſophical tranſactions, M. Caſſegrain, a Frenchman, in the *Journal des Scavans* of the ſame year 1672, claimed the honour of a ſimilar invention, and ſaid that, before he heard of Newton's improvement, he had hit upon a better conſtruction, by uſing a ſmall convex mirror inſtead of the reflecting priſm. This teleſcope was not much unlike that of Gregory, the large mirror being perforated; and Montucla obſerves that, to conſider the thing in theory, M. Caſſegrain's teleſcope ſeems to have many advantages; for that, beſide being much ſhorter, the convex mirror, by diſperſing the rays, muſt greatly enlarge the image made by the large concave mirror. Newton made many objections to M. Caſſegrain's conſtruction; but ſeveral of them equally affect that of Gregory, which has neverthleſs, been found to anſwer remarkably well, in the hands of good artiſts.* Dr. Smith took the pains to make many calculations of the magnifying power both of Newton's and Caſſegrain's teleſcopes, in order to their farther improvement, which may be ſeen in Smith's Opticks, Remarks, p. 97. Mr. Short, I am informed, made ſeveral teleſcopes on the plan of M. Caſſegrain, but though their magnifying power might be made very conſiderable, in proportion to their length, it was not eaſy to make the image ſufficiently diſtinct, for want of light.

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* Newton's Opticks, p. 98. † Birch's Hiſtory, Vol. 3, p. 4.

It is ſomething remarkable that, notwithſtanding the promiſing appearance of Newton's theory of teleſcopes, thirty years ſhould have elapſed, in that philoſophical and buſy age, before any uſe was made of it; for the firſt teleſcope of this kind, of any conſiderable length, was made by Mr. Hadley in 1718, of which an account will be given hereafter.

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* Montucla's Hiſtoire, Vol. 2, p. 647.

CHAPTER IV.

Of the opposition which Newton's doctrine concerning light met with.

CONSIDERING how deciſive were the very firſt proofs that Sir Iſaac Newton produced in ſupport of his hypotheſis, concerning the different refrangibility of the rays of light, and the invariable colour of the differently refrangible rays, (though all the experiments I have recited were not publiſhed to the world till ſometime after, and he had not then acquired that diſtinguiſhed reputation which he afterwards gained by the publication of his *Principia*) it is not eaſy to account for the violent oppoſition which his theory met with, and the vexatious diſputes which it occaſioned among philoſophers, eſpecially as theſe diſcoveries were a real addition to what was known before concerning the ſubject, and were not merely the overturning of preceding hypotheſes. Indeed, as I have obſerved before, there was not then any hypotheſis about the matter that could be ſaid to be *eſtabliſhed*, or that was held in any degree of repute among philoſophers. Beſides, the chief oppoſers of Newton were not perſons who had advanced any hypotheſis of their own, to which they could be ſuppoſed to be attached. But ſuch is the nature of man, that as ſoon as any perſon ariſes to any degree of eminence, the emulation of others is offended at ſeeing the rank in the eſteem of mankind, to which themſelves are puſhing, occupied by another, and one avenue more to diſtinction and fame ſhut up. Not being able, therefore, to riſe into reputation by any opinions or diſcoveries of their own, they hope to make themſelves noticed by their oppoſition to thoſe of others. Our philoſopher was ſo chagrined by thoſe diſputes, into which he entered with ſome degree of ſpirit at firſt, that, dreading to involve himſelf in more altercation, he could not be prevailed upon to publiſh his treatiſe on Opticks, which he had promiſed to the world, till the year 1704; and that, contrary to his uſual cuſtom, in Engliſh only (which was a language very little known abroad in thoſe times) and with an earneſt requeſt that no perſon would tranſlate it without his leave; and yet the principal of his diſcoveries concerning light and colours were communicated to the Royal Society in 1675, and were publiſhed in the Philoſophical Tranſactions. Theſe, and other inſtances of unexpected and unmerited oppoſition, contributed to make our author more reſerved, and more uncommunicative to the end of his life.

The moſt induſtrious of all mankind in philoſophical purſuits, at the time that Newton made his firſt communications upon this ſubject, was Dr. Hooke. He could bear no rival in any thing that he himſelf pretended to; and, unfortunately, the buſineſs of light and colours had been one of his favourite objects; and, in ſeveral reſpects, we have ſeen that this branch of ſcience ſtands indebted to him. It will not be wondered therefore, that he took umbrage at the riſing reputation of a new philoſopher. He inſtantly and ſtrenuouſly oppoſed the new ideas of Newton, and obſtinately maintained an hypotheſis of light, which he had modified from Deſcartes, founded upon the notion that colours conſiſt of vibrations in an etherial medium. In ſeveral particulars he, moreover, charges Newton with plagiariſm with reſpect to himſelf, to which our author modeſtly replies, by ſtating Dr. Hooke's opinion and his own, and acknowledging that he had made uſe of ſome of his obſervations, particularly that of the inflexion of the rays of light, for which, he ſays, he quoted him, that of opacity ariſing from the interſtices of the parts of bodies, and that of plated bodies exhibiting colours, a phenomenon for the notice of which he thanks him, but, ſays our author, "he "left me to find out, and make ſuch experiments about it as "might inform me of the manner of the production of thoſe "colours, to ground an hypotheſis upon; he having given no "farther inſight into it than this, that the colour depended on "ſome certain thickneſs of the plate, though what that thick"neſs was, at every colour, he confeſſes in his *Micrography*, he "had attempted in vain to learn, and therefore, ſince I was left "to meaſure it myſelf, I ſuppoſe he will allow me to make uſe of "what I took the pains to find out. This I hope may vindicate "me from what Dr. Hooke has been pleaſed to charge me "with." *

Of foreigners, the firſt who made any public objection to Newton's philoſophy concerning light and colours was Father Pardies, but he was ſoon convinced of his miſtake, and was ſo ingenuous as to acknowledge it; but no perſon diſtinguiſhed himſelf ſo much by his oppoſition to Newton as the ingenious Mr. Marriotte, who could not ſucceed in making the experiments.

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* Birch's Hiſtory, Vol. 3, p. 279.

And as M. Leibnitz, who was the editor of the *Acta Eruditorum* at that time, in the volume for the year 1713, p. 447, requeſted that Newton himſelf would remove M. Marriotte's difficulty, for the benefit of others, he himſelf being at that time dead, Sir Iſaac deſired Dr. Deſaguliers to try the experiments in which M. Marriotte had failed, and he did it accordingly with perfect ſucceſs, before many members of the Royal Society, and alſo, afterwards, in the preſence of M. Monmort, and ſeveral gentlemen of the Royal Academy of ſciences.*

Sometime after, Rizetti, an Italian, pretending to refute Newton's theory, by experiments, our philoſopher was ably defended by M. Richter. This controverſy, however, was the occaſion of Dr. Deſaguliers repeating his experiments, with ſome new ones, leading to the ſame concluſions, in 1728; after which no perſon, who choſe to give his name to the public, or whoſe name is worth recording, made any more oppoſition to it; ſo that, at preſent, no hypotheſis in philoſophy ſtands upon ſurer ground, or is more generally acquieſced in, than that of the different refrangibility of the rays of light.†

As ſome of the obſervations and experiments which Dr. Deſaguliers made upon this latter occaſion, will tend to throw light upon the ſubject, I ſhall cloſe this ſection with the recital of a few of them. Let us, ſays he, ſuppoſe a priſm, ACB, fig. 99, to have the attracting power of its inferior ſurface extend as far as the line *mm*; if another priſm, GDE (the attracting force of whoſe upper ſurface extends as far as *nn*) be brought very near to the firſt priſm, where the attracting powers of the priſms interfere, they will deſtroy one another, becauſe they act in contrary directions, and thereby the limits of attraction of each of the ſurfaces will be contracted, the power of AB extending no farther than *nn* and that of DF no farther than *mm* whilſt the ſpace *nn*, *mm* loſes all the force that it had, and would have, upon the removal of either priſm, to turn a ray of light moving obliquely out of its direction.

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* Phil. Tranſ. ab. Vol. 4, p. 173.

† Montucla, Vol. 2, p. 621.

Now in this ſituation of the priſms, a ray of light entering the ſurface CB at right angles, will go through the ſecond priſm alſo at right angles, not exactly in the ſame line, but in a line parallel to the direction of the incident ray. For example, let the ray R *a*, not refracted at, becauſe perpendicular to the ſurface CB, emerge from the firſt priſm at *a*, in the direction *a r*, its changed direction at *a* will become *a* a, and at *b*, *b* b, or rather the ray will be inflexed in the curve *a b*; and at *b* getting out of the power of the attraction of the ſurface AB, it will, for the reaſons before given, move in a ſtrait line, from *b* to *c*, where it will be bent again the contrary way, in the curve *c d*, of the ſame kind as *a b*, and laſtly, emerge in the direction *d* d, parallel to the firſt direction R *r*. From hence it follows, that when the priſms are brought ſo near as to touch, their mutual attractions deſtroying each other; the rays of light will not be bent, but paſs through the two priſms (which, in this caſe, performs the part of a parallelopiped) in the ſame direction with which they came into the firſt priſm, and conſequently produce no colours, contrary to what was affirmed by Rizzetti; and when the rays R *a* fall obliquely upon the ſurface CB, the effect of their refraction at their immerſion at S, to produce colours, is taken off by the refraction which they ſuffer at their emerſion at Z.

If the ray of light R *a b c d* d, changing its direction, in the manner above mentioned, makes an angle of about 45 degrees with the perpendicular P *a*, upon the removal of the lower priſm, the ray will be turned up again, as in fig. 100; but if the angle P *a* R be greater, the ray will ſtill be turned up again in a curve, as *a b c d e f* (fig 101) notwithſtanding the lower priſm is at DFG; but if that priſm be brought up cloſer to the ſurface AB, the curves will be deſtroyed where the priſms touch, and all the rays in the place of contact will be brought down through the lower priſm.

The moſt refrangible rays conſiſt of ſmaller particles than the leaſt refrangible rays, and therefore muſt have the leaſt momentum, the velocity of all the rays being the ſame, and conſequently are more eaſily turned out of the way by attraction or repulſion, which makes the curves made by the purple and violet rays under the ſurface AB to be leſs, and nearer the ſaid ſurface than the curves made by the red and orange rays. Suppoſe a violet R*a*, fig. 101, moving in the direction R r, to be ſo bent under the ſurface AB, that, at the vertex of the curve, or where its tangent *c* c is parallel to AB, there ſtill remains a ſmall ſpace between the curve and the line *n n*, where the limits of attraction (contracted by the proximity of the undermoſt priſm DFG) ends, that ray will be turned up again in the curve *d e f*, and ſo reflected in the line *f* f, the directions having been ſucceſſively changed, as in fig, 100; but a red ray, with the ſame inclination, would paſs on into the lower priſm, as was explained in fig. 99; becauſe the momentum of the red ray being greater than that of the violet, the ſame degree of attraction could not give it the ſame flexure. This is confirmed by experiments, for when the lower priſm is not preſſed hard againſt the upper (~~as in fig. 76~~) the rays brought down to R make a ſpot, of a colour made up chiefly of red and orange rays; but when the priſms are preſſed cloſer, the ſpot grows bigger, and perfectly white in its middle, becauſe all ſorts of rays are brought down to the ſpot; but it is encloſed round with a reddiſh border, occaſioned by the parts of the priſm which are very near, but not in contact, or at leaſt not near enough to bring down the green, blue, purple, and violet rays. This ſhews that the reflexion is not made from the interior ſolid parts of the glaſs, nor from the parts in the ſurface, as Rizzetti affirms. But this is made more evident by another experiment.

A candle being in the poſition K, fig. 102, the eye at E, and the priſm at ABC, a ſtrong image of the candle was ſeen at K; but lifting up a veſſel of water VSSV, till the ſurface of the water VV touched AB, the lower ſurface of the priſm, the image of the candle became almoſt inſenſible, as the eye loſt all thoſe rays which now were attracted into the water. And for a farther proof that the reflection is made under the ſurface, and not in it; when the priſm was taken out of the water, being wet at its lower ſurface, or having a coating of water, whoſe ſurface was VV, under AB, the image of the candle did again become vivid, the rays being turned up again under VV. Indeed the image in this caſe, though ſtrong, did not appear well defined, by reaſon of the unevenneſs of the watry ſurface VV.*

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* Phil. Tranſ. ab. Vol. 6, p. 136.

PERIOD

PERIOD VI.

DISCOVERIES RELATING TO VISION, LIGHT, AND COLOURS, SINCE THE TIME OF SIR ISAAC NEWTON.

IT will ſurpriſe many of my readers to ſee ſo great a proportion of the materials of this part of my hiſtory fall within the period that is now opening upon us. The bulk of mankind, I may perhaps add, of philoſophers too, have a notion, that little or nothing has been done, on the ſubject of viſion, light, and colours, ſince the time of Sir Iſaac Newton, imperfect as he left many of his experiments; in ſo much, that they are perpetually expreſſing their wonder, that no perſon has taken them up after him, and begun where he left off. To ſuch perſons, the hiſtory of this period will afford a pleaſing entertainment. They will find it a copious field, abounding not with trifling obſervations, and frivolous minutiæ, but with real and important diſcoveries, reſpecting the moſt fundamental principles of light, and of bodies reſpecting light; and alſo with a great number of illuſtrations on the phenomena of viſion. And I can aſſure my reader that, far from having been obliged to make the moſt of my materials for this period, they have furniſhed more exerciſe for me in the way of *abridging*, in order to comprize a great deal in a ſmall compaſs, than thoſe of any part of the work.

To give the moſt diſtinct view of the diſcoveries and improvements that have been made within this fruitful period, I ſhall diſtribute them into ſeveral ſections, moſt of which it will be neceſſary for me to ſubdivide; and in every ſection, if my reader has gone no farther than Sir Iſaac Newton, he will find many original and uſeful obſervations; and, if he has made himſelf maſter of every thing that has been publiſhed in Engliſh only, ſince Newton, the greateſt part of the contents of ſeveral of the ſections will be new to him, and he cannot but eſteem them to be curious and valuable.

So many are the perſons who have diſtinguiſhed themſelves by their diſcoveries within this period, that I ſhall not take up my reader's time with the recital of their names and merit, in this introduction; and of the many *ſocieties* for promoting natural knowledge, which have been formed within the courſe of it. I ſhall but juſt mention two, whoſe publications are the moſt regular and copious. They are thoſe of Peterſburg and Berlin, and each of them promiſes to preſent the world with a very voluminous and valuable collection of materials, for all the branches of philoſophical knowledge.

SECTION I.

Observations concerning the NATURE OF LIGHT.

CHAPTER I.

Of M. EULER's hypothesis concerning light.

CONCLUSIVE as the generality of philoſophers imagined the arguments againſt the opinion, that light conſiſts in the vibrations of a fluid medium, to be, and particularly after the publication of Sir Iſaac Newton's *Principia*, in which the impoſſibility of that hypotheſis ſeemed to have been demonſtrated; ſeveral philoſophers, and eſpecially ſome eminent foreigners, ſtill adhered to it; and it was with great difficulty that many, even of the Engliſh, gave it up. But no writer oppoſed the hypotheſis of Newton, if it may be called his (becauſe it was only propoſed by him in one of his queries, viz. that light conſiſts of ſmall particles emitted from luminous bodies) ſo ſtrenuouſly, or took ſo much pains with the ſubject, as that celebrated mathematician M. Euler, in his treatiſe intitled *Nova Theoria lucis et colorum*, in which he revived and maintained the hypotheſis of M. Huygens, who ſuppoſed light to conſiſt in vibrations propagated from the luminous body, through a ſubtle etherial medium.

As I do not propoſe to trouble my reader with mere hypotheſes, any farther than they are abſolutely neceſſary to explain the appearances of nature, I ſhall content myſelf with giving a ſhort ſummary of Mr. Euler's objections to the Newtonian doctrine, as an introduction to the pertinent and uſeful remarks upon the ſubject, that were made by others, who wrote in anſwer to him, and to ſome new and important facts, which tend to eſtabliſh what is now the commonly received opinion.

M. Euler himſelf gives the following ſummary of his arguments againſt the common opinion. Beſides that the ſun would in time be exhauſted by ſuch a copious emiſſion of material particles (to ſupply which, Newton is obliged to have recourſe to the comets, and to ſuppoſe that they fall into the ſun) ſuch a quantity of matter, filling all the etherial regions, and agitated with ſuch a prodigious motion, could not fail to diſturb the planets and comets, in their revolutions about the ſun. But, paſſing by this great difficulty, and admitting, alſo, that particles of matter can move with the amazing velocity of light, is it poſſible that they ſhould penetrate tranſparent ſubſtances with ſo much eaſe? In whatever manner we ſuppoſe them to be tranſmitted, thoſe bodies muſt have pores, diſpoſed in right lines, and in all poſſible directions, in order to form canals for the paſſage of the rays. But, ſays he, ſuch a ſtructure muſt take away all ſolid matter from thoſe bodies, and all coherence among their parts, if they do contain any ſolid matter.*

Mr. Euler likewiſe advanced a new notion with reſpect to the origin of colours in opaque bodies, which is intirely inconſiſtent with the principal part of Sir Iſaac Newton's doctrine. He ſuppoſed that coloured bodies reflect the ſun's incident white light from their anterior ſurface, but that the particular *ſpecies* of light, by which they appear of different colours, is emitted by the parts of the body. For inſtance, he imagines that vermillion does not appear red by a more copious reflexion of red than of other incident rays, but by the new emiſſion of red rays, from the particular velocity of vibration, which its elaſtic parts are capable of being thrown into, by the impulſe of the incident light.*

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* Ac. Berl. 1752, p. 271.

In anſwer to theſe, and ſome other objections, I ſhall preſent my reader with ſome pertinent remarks of Mr. Melville, a perſon who died at the age of 27, after leaving us ſome ingenious thoughts on the ſubject of light and colours; and from the attention which he propoſed to give to this ſubject, ſomething farther might have been expected from him, if he had lived longer.

It is, ſays he, a ſufficient refutation of M. Euler's theory of colours, that no phenomena prove, or require its exiſtence; whereas Newton's theory not only ſolves the phenomena, but is directly drawn from a multitude of experiments. According to M. Euler's hypotheſis, a body of one colour placed in the homogeneous light of another, ought not to appear of the colour of the light, but of a middle one between that and its own natural colour, which is contrary to experience.

The chief, or the only fact, which ſeems to have led M. Euler into this opinion is, that there are many coloured bodies, ſuch as metals, which are capable of receiving a fine poliſh, and therefore of reflecting, regularly, the images of other objects, and at the ſame time retain their proper colour, by which they are ſeen in all poſitions. That light by which we ſee in them the images of their objects, he acknowledges to be incident light, properly reflected, but the other he ſuppoſes to be emitted from the colorific parts of the body. But what neceſſity, ſays Mr. Melville, is there of recurring to this ſuppoſition, when we know previouſly, that the component parts of all opaque bodies are tranſparent, that from every tranſparent body there is a double reflexion, part of the incident light being reflected at the firſt ſurface, and a part of what paſſes through the firſt at the ſecond, and when we know, farther, that very thin bodies, as ſoap bubbles, and air between two lenſes, while they reflect ſome rays of all colours from the firſt ſurface, reflect only particular colours at the ſecond. Do not theſe facts, ſays he, lead us naturally to ſuppoſe the firſt ſort of light to be only a part of the incident light, reflected from the firſt ſurface of the body, and the ſecond a part of what had paſſed on, reflected from the poſterior ſurfaces of the ſuperficial particles. *

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* Edinb. Eſſays, Vol. [illegible], p. 39.

CHAPTER II.

OF THE PROPERTY OF SOME SUBSTANCES TO IMBIBE AND EMIT LIGHT, ESPECIALLY THE BOLOGNIAN PHOSPHORUS.

THE hypotheſis of the materiality of light is peculiarly agreeable to the phenomena of the *Bolognian ſtone*, which has the remarkable property of imbibing light, of retaining it for ſometime, and of emitting it again; and more eſpecially by its emitting it more copiouſly, according to the degree of heat applied to it. By this hypotheſis alſo, it is eaſieſt to account for ſome facts, by which it appears that the colour and internal texture of bodies are changed by light only, excluſive of heat, or any other circumſtance. Of theſe curious diſcoveries I ſhall give a ſuccinct account in this ſection, and in the order in which I have mentioned them.

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* Edin. Eſſays, Vol. 2, p. 38.

Never was philoſophy under more obligation to what we call *chance*, than with reſpect to the diſcovery of the property of imbibing light, which was once thought to be peculiar to a certain foſſil found in the neighbourhood of Bologna, but is now found in many other ſubſtances. Not only was a ſingle and leading fact, but a whole ſeries of facts the reſult of caſual obſervation.

The perſon who led the way in this new branch of philoſophy was Vincenzo Caſcariolo, a ſhoemaker of Bologna; * who being in queſt of ſome chymical ſecret, among other things, tryed a calcination of the ſtone above-mentioned, which is found towards the bottom of mount Peterno in that neighbourhood, and caſually obſerved, that, whenever this ſubſtance was in a dark place, after having been expoſed to the light, it was plainly viſible by light iſſuing from itſelf. This diſcovery was made, as nearly as I can collect from circumſtances, about the year 1630.

So remarkable a fact, as ſoon as it became known, could not fail to engage the particular attention of philoſophers. Accordingly we find ſpecimens of the preparation in the hands of perſons in many parts of Europe, and ſeveral treatiſes were written upon the ſubject. Among others we find the names of F. Licetus, a profeſſor of philoſophy at Bologna, Potterius, Celias, and

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* Hooke's Experiments by Derham, p. 178.

Athanaſius Kircher, whom no ſubject of philoſophy eſcaped.[*] He gives a particular deſcription of this foſſil, and deſcribes the manner in which it was then calcined, namely by being beaten to a fine powder, together with white of egg, water, or linſeed oil. A furnace, he ſays, was conſtructed for the purpoſe, and there was often occaſion to repeat the proceſs ſeveral times, in order to ſucceed perfectly. He ſays, however, that this ſtone was not peculiar to the neighbourhood of Bologna; for that he himſelf had diſcovered it in ſome alum mines at Tolpha.

We are not ſurpriſed to find that this remarkable experiment was explained by ſome perſons on the hypotheſis of the materiality of light, firſt imbibed by this ſubſtance, and after being detained for ſome time within its pores, emitted again; but it appears from Kircher, that this was not the opinion of the generality of philoſophers at that time. He himſelf takes pains to refute it, and ſubſtitutes another of his own; ſuppoſing that the air abounds with a ſubtle vapour, which is very eaſily illuminated, and that this ſubſtance imbibes that vapour.[†]

Among others who wrote upon this ſubject was Count Marſilus of Bologna, whoſe treatiſe was printed at Leipſic in 1698. This gentleman, in order to verify his obſervations, and aſcertain ſome particular facts, viſited mount Peterno above-mentioned in September 1711, in company with Laurentius Galeatius and Beccarius, who afterwards ſo greatly diſtinguiſhed himſelf by his obſervations on this ſubject.[§] Theſe gentlemen took a great deal of pains with the chymical analyſis of this foſſil, by which they thought they diſcovered in it ſome ſulphur, and alſo an alkaline ſalt.* And afterwards they proceeded to examine many of its properties, particularly thoſe concerning which there had been any diſpute among philoſophers.

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[*] Schott's Magia, Vol. 6, p. 94. Ac. Par. 1730, M. p. 749.

[†] Ars Magna, p. 18.

[§] Com. Bonon, Vol. 6, p. 189.

M. Lemery,† and Marſilius himſelf, had advanced that the Bolognian ſtone imbibed leſs light from the direct than from the reflected beams of the ſun; but the experiments they now made induced them to think otherwiſe, and Marſilius openly retracted his opinion.§ They could illuminate the phoſphorus very well with the light of a candle, but not with that of the moon, or light emitted from other phoſphori.‡ They alſo obſerved that when one part of a piece of phoſphorus was illuminated, the light was not propagated to another part.|| They found that expoſing ſome of their beſt ſpecimens to the light for one ſecond, was ſufficient to make them viſible, but they did not ſhine near ſo bright as after they had been expoſed two ſeconds, which was ſufficient to give them all the light they could receive. After continuing four minutes, the light was conſiderably diminiſhed, but ſome of them were viſible above thirty minutes.** The leaſt degree of light that was capable of illuminating their phoſphori, was that, in which the ſmalleſt print was juſt legible. In general, the illuminated phoſphorus was red, but if the light was weak, as when it had been received through ſeveral pieces of white paper, it was of a pale white.†† Laſtly, they found that thoſe ſtones which had been often uſed, imbibed the light better than others, or than they had done at firſt.

Thus terminated the inquiries and the joint labours of theſe philoſophers. Afterwards, namely about 1728, the ſubject was re-

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[*] Com. Bonon, Vol. 6, p. 186. [†] Ac. Par. 1730, M. p. 757. [§] Ib. p. 188. [‡] Ib. p. 190. [||] Ib. p. 191. [**] Ib. [††] Ib. p. 193.

ſumed by Galeatius, and Zanottus, Secretary to the Inſtitute at Bologna. Theſe gentlemen found that, in an exhauſted receiver, the phoſphorus received ſomewhat leſs light, and did not retain it quite ſo long; but this might be owing to the interpoſition of the glaſs, though they did not attend to it. At the ſame time it occurred to Zanottus, that this phoſphorus might be the means of deciding the controverſy, which was then warmly agitated, between the Carteſians and Newtonians, concerning the nature of light; and alſo of determining whether the phoſphorus only emitted the light which it had received, or light of its own, which was kindled, as it were, by foreign light. They, therefore, in company with Algarotti, a great partizan of Newton's, expoſed ſeveral pieces of their beſt phoſphorus in differently coloured light, produced by a priſm in a darkened room; but though they got ſome very good priſms from England, they could not perceive any certain difference between the light imbibed from the different colours. Indeed, they were all very faintly illuminated in this manner. They alſo viewed the illuminated phoſphorus through a priſm, but they could perceive no difference of colour in it. The appearance, they ſay, was the ſame as when they looked through the priſm at coals burning not very bright. *

Upon the whole, Zanottus, who writes the account, concludes that the experiments were very conſiſtent with both the hypotheſes above mentioned, as he endeavours to ſhew at large. But he ſays, that ſince the phoſphorus exhibits only *light* in general, and not the particular *colour* of the light to which it is expoſed, it is evident that it ſhines by its own native light, which is only kindled by foreign light. It has, however, been very lately obſerved by Father Beccaria of Turin, that pieces of artificial phoſphorus, much ſuperior to the calcined Bolognian ſtone, incloſed in tubes, into which the light is admitted through pieces of coloured glaſs, exhibit the ſame colour only. In theſe circumſtances, the phoſphorus is much more ſtrongly illuminated with any particular kind of light, than it could be in the priſmatic image of the ſun. This, therefore, ſeems to be deciſive in favour of the opinion, that the phoſphorus emitts the very ſame light that it receives, and no other; and conſequently, that light conſiſts of real particles of matter, capable of being thus imbibed, retained, and emitted; and I doubt not, but that Zanottus, if he be now living, will retract his concluſion.

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* Ac. Par. 1730, M. p. 205.

It was an accident, likewiſe, that diſcovered the property of imbibing and emitting light in other ſubſtances beſides the Bolognian ſtone. Van Helmont, indeed, aſſerted that he had a flint ſtone, prepared in ſuch a manner, as that it would ſhine in the dark, after being expoſed to the light. But whether his aſſertion was not credited, or whatever elſe was the reaſon, no perſon ſeems to have given any attention to it. At length, however, a little before the year 1677, G. A. Baldwin, Bailiff of ſome place in Miſnia, when he was diſſolving ſome chalk in aqua fortis, in his purſuit of the philoſopher's ſtone, found that the reſiduum, after diſtillation, would imbibe the light, and emit it again in the dark, exactly like the celebrated Bolognian phoſphorus.* This diſcovery was peculiarly acceptable to the curious, as it ſeems that the beſt method of preparing the Bolognian ſtone had been kept a ſecret in the Zagonian family, all

* Miſ. Berol, Vol. 1, p. 91.

of whom had died without revealing it. This phoſphorus of Baldwin, however, was, in no reſpect, ſo good as the Bolognian. It was not ſo luminous, it did not retain the light ſo long, it ſoon loſt its virtue, and never recovered it again.* It is not certain that what afterwards went by the name of Baldwin's phoſphorus, was the ſame preparation that this philoſopher hit upon.

Beſides this phoſphorus of Baldwin, the ſame property was found in a talcky ſubſtance, dug in the neighbourhood of Berne in Switzerland, a ſpecimen of which, accompanied with a memoir on the ſubject, was ſent by M. Bourguet to the members of the Royal Academy at Paris. This ſubſtance was referred to the examination of M. Du. Fay, who is ſo famous for his diſcoveries in electricity. His account of it may be ſeen in the memoirs of the academy for the year 1724.

No other phoſphorus, ſimilar to theſe above mentioned, was diſcovered by any perſon till the year 1730, when M. Du Fay obſerved the ſame property in all ſubſtances that could be reduced to a calx by burning only, or after ſolution in the nitrous acid.† This philoſopher was led to theſe diſcoveries when he was making experiments, with other views, on ſome precious ſtones. At this time he obſerved, that the common topaz, when calcined, had all the properties of the Bolognian phoſphorus; and, purſuing this diſcovery, he found the ſame property, in a great degree, in the belemnites, gypſum, limeſtone, and marble. Some very hard ſubſtances of this kind he was obliged to diſſolve in acids, before calcination would produce this change in them; and with ſome ſubſtances he could not ſucceed even by this means, eſpecially with flint ſtones, river ſand, jaſpers, agates, and rock cryſtal.*

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* Ac. Par. 1693, p. 271.

† Baccarii Comment. p. 10.

The ſame philoſopher, in 1734, likewiſe diſcovered that ſome diamonds, an emerald, and many other precious ſtones were poſſeſſed of this property, without any chymical preparation.† He could find no rule to diſtinguiſh thoſe diamonds which had this property, from thoſe which had it not. Among other experiments, he found that, after enduring the greateſt degrees of heat, both the ſorts remained, juſt as they were before.§ When M. Du Fay diſcovered this property in diamonds, he was endeavouring to find whether they gave light in conſequence of being heated; and he expoſed them to the ſun with no other view than to warm them. On this occaſion he could not help expreſſing his ſurprize that Mr. Boyle, and other perſons, among the various methods they made uſe of to communicate heat to diamonds, ſhould not have thought of this, which could hardly have failed to diſcover to them this property of imbibing light.

M. Du Fay obſerved, that all theſe phoſphori loſt their virtue by being a long time in the open day light; and that ſome of them would preſerve their beauty a long time, though they were plunged in water; which was known to be in the end deſtructive of this peculiar property. He alſo took notice that ſome of theſe ſubſtances ſhone with unuſual ſplendour while they were diſſolving, and thereby heated, in water; but this did not continue long;‡ and he alſo took notice that ſome of his diamonds would ſhine after they had been buried in black wax ſix hours.‖ The cauſe of the light that was given in theſe circumſtances was not known to M. Du Fay, but a very ſatisfactory account will be given of it before the cloſe of this ſection. Beſides common water, this philoſopher endeavoured to find what other ſubſtances would, by their contact with theſe phoſphori, deſtroy their property of imbibing light; but he did not complete the courſe of experiments which he began with this view.*

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* Ac. Par. 1730, H. p. 69. † Beccarii Comment. p. 12. § Ac. Par. 1735, H. p. 3. ‡ Ib. 1730, H. p. 70. ‖ Ib. 1735, H. p. 4.

That ſome diamonds have the property of the Bolognian phoſphorus was alſo diſcovered by Beccarius, about the ſame time that it occurred to M. Du Fay. He was viſiting a lady after her lying in, when, the room being pretty dark, ſhe caſually obſerved, that a diamond which he wore in a ring appeared exceedingly bright. This putting him upon examining it with attention, he found that it was indeed poſſeſſed of the true property of imbibing and emitting light, and accordingly he gave an account of it to the academy of the Inſtitute in November 1734.†

Beccarius, having thus caſually found that this diamond of his was a genuine natural phoſphorus, applied himſelf diligently to the examination of all kinds of ſubſtances; and having contrived a portable ſeat, in which he could keep himſelf perfectly in the dark, and in which he could, with no loſs of time, view any ſubſtance immediately after its being expoſed to the light of the ſun, he found that almoſt every thing in nature imbibed more or leſs light, and emitted it again in the dark, and with a great deal of labour he diſtinguiſhed natural bodies into ſeveral claſſes, as they were phoſphori with or without preparation.

Almoſt all vegetable and animal ſubſtances, when thoroughly dry, had this property, and paper in a very great degree.§

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* Ac. Par. 1730, H. p. 70.

† Com. Bonon, Vol. 2. p 276.

§ Phil. Tranſ. ab. Vol. 10, p. 557. Beccarii Comment. p. 44.

Among ſome of the more remarkable obſervations of Beccarius, made with more accuracy and attention than he had applied at the firſt; and of which he made a ſecond publication, ſome years after his firſt treatiſe on this ſubject, are the following. In his firſt experiments the yolk of an egg, when thoroughly dry, eaſily became a phoſphorus, but the white of the egg not at all.* This fact had puzzled our author very much, but it ſhould ſeem that when he had improved his method of making obſervations, he had found it to imbibe light in a ſmall degree, as he does not mention it in his laſt exceptions. Every kind of earth, without excepting even the black and the red, with which he had no ſucceſs at firſt, were phoſphoreal.† Among a hundred agates, one only, and that a very black one, was found, that was not, in ſome degree, a natural phoſphorus; but this could not be made to imbibe the leaſt light, even from a burning mirror.§ To metals and water he could never communicate the leaſt degree of light,|| which is ſomething remarkable, and deſerving particular notice, as theſe ſubſtances agree in the property of being conductors of electricity. I have alſo found agates to be no bad conductors, and I ſhould be inclined to conjecture that ſuch pieces of charcoal as are good conductors could not be made to imbibe light, though Beccarius does not mention his having made any trial of charcoal. He found, however, that very few ores could be made phoſphoreal, nor any vitriols, except in the ſlighteſt degree.** At the ſame time he found that oils had this property, but only in a ſmall degree;†† and yet it muſt be obſerved that they are very imperfect conductors of electricity. Light was viſibly retained by the ſkins of ſeveral living animals, and by his own fingers, but not always.* At one time, when it was very cold, his hand ſhone uncommonly bright.†

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* Beccarii Comment. p. 52. † Ib. p. 91. § Ib. p. 95. || Ib. p. 96. ** Ib. p. 98. †† Ib. p. 104.

Turning his thoughts, as M. Du Fay had done before him, to thoſe circumſtances which obſtructed this phoſphoreal virtue, he found that, though water could not be made to ſhine by imbibed light, ice did exceedingly well, and eſpecially ſnow.§ Having made three cakes of clay, one of which he baked pretty hard, while he kept another moiſt, and ſet the laſt of them to freeze; he found the firſt and laſt, but eſpecially the firſt, to imbibe light plentifully, but the middle, or moiſt one, very poorly.‡

That excellent chymiſt M. Margraaf, undertook the analyſis of the Bolognian ſtone, and by his proceſs it appears, that both the contact of the fuel and the free acceſs of the air are neceſſary in the calcination of it; || but theſe circumſtances do not ſeem to be material in the preparation of artificial phoſphorus, as it is made by Mr. Canton, and which will be mentioned preſently. M. Margraaf gives a liſt of all the ſubſtances which he could find in Germany, that, being calcined, have the ſame property with the Bolognian ſtone; and after analizing them, he concludes that they all contain a vitriolic acid, united to an alkaline, or calcarious earth.** This will appear to be ſufficiently agreeable to the principles of Mr. Canton's preparation.

In the next place I ſhall preſent my reader with an account of this compoſition of Mr. Canton's which makes an artificial phoſphorus greatly ſuperior to any ſingle natural ſubſtance, and has the advantage of being very eaſily and cheaply prepared; by which means it is now in every perſon's power to amuſe himſelf and his friends with theſe curious experiments. His receipt for making it is as follows: Calcine, ſome common oyſter ſhells, by keeping them in a good coal-fire for half an hour, and let the pureſt part of the calx be pulverized, and ſifted. Mix with three parts of this powder one part of the flowers of ſulphur; let this mixture be rammed into a crucible, of about an inch and a half in depth, till it be almoſt full, and let it be placed in the middle of the fire, where it muſt be kept red hot, for one hour at leaſt, and then ſet by to cool. When it is cold, turn it out of the crucible, and cutting, or breaking it to pieces, ſcrape off, upon trial, the brighteſt parts; which, if good phoſphorus, will be a white powder, and may be preſerved by keeping it in a dry phial with a ground ſtopple.

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* Beccarii Comment. p. 108. † Ib. p. 110. § Com. Bonon, Vol. 5, p. 106.
‡ Ib. p. 107. || Ac. Berl. 1749, p. 56. ** Ib. 1750, p. 145.

The quantity of light which a little of this phoſphorus gives, when firſt brought into a dark room, after it has been expoſed for a few ſeconds on the outſide of a window, to the common light of the day, is ſufficient to diſcover the time by a watch, if the eyes have been ſhut, or in the dark, for two or three minutes before.

By this phoſphorus, celeſtial objects may be very well repreſented, as Saturn and his ring, the phaſes of the moon, &c. if the figures of them, made of wood, be wetted with the white of an egg, and then covered with the phoſphorus. And theſe figures appear to be as ſtrongly illuminated in the night, by the flaſh from a near diſcharge of an electrified bottle, as by the light of the day.*

With this artificial phoſphorus Mr. Canton made a variety of curious experiments, which throw a great additional light upon

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* Phil. Tranſ. Vol. 58, p. 337.

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this remarkable property of bodies; ſome of which are contrary to the aſſertion of Lemery, that expoſing it to the light of the ſun for a long time injures it; for having put ſome of the ſame parcel of the phoſphorus into two glaſs balls, and ſealing them hermetically, he placed one of them on the outſide of a window facing the South, that it might be very much expoſed to the direct rays of the ſun, where it remained from the Twenty-fifth of December 1764, to the Twenty-fifth of December 1765. The other was kept during the ſame time in darkneſs. After this they were both expoſed to the light, and carried into a dark room together, where the phoſphorus in each appeared equally bright.* Mr. Canton, therefore, ſuppoſes that what Lemery imputed to the light of the ſun, was cauſed by the moiſture of the air. It will appear, however, from ſome experiments of Beccarius, which will be recited hereafter, that ſome of the more delicate kinds of phoſphorus were really injured by the rays of the ſun, excluſive of every other circumſtance.

This phoſphorus could not be ſo ſafely expoſed to moiſture; for ſome of it finely powdered, being put into a glaſs ball, with as much water as would make it adhere to the glaſs, ſo as to cover the inſide of the ball, which was hermetically ſealed, was found gradually to loſe its property of imbibing and emitting light; but faſter in ſummer than in winter, ſo that, at the end of the firſt year, it could not in the leaſt be perceived to ſhine, when taken from the ſtrongeſt day light, and carried into a dark room. It was alſo obſerved to loſe its whiteneſs by degrees, and to become of a very dark colour, eſpecially on that ſide of it next to the glaſs. Some of the phoſphorus which was made to ſtick to the inſide of a glaſs ball hermetically ſealed, by means of common ſpirit of wine, was found, after one year, to be a little impaired; but ſome made to ſtick, by means of an ætherial ſpirit, was found not to be impaired at all.*

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* Phil. Tranſ. Vol. 58, p. 338.

Trying the contact of æther and ſpirit of wine, he found it to be more affected by the latter than by the former. For mixing a ſmall quantity of the phoſphorus with a conſiderable quantity of ſpirit of wine in one glaſs ball, and with æther in another, and ſealing them hermetically; when the balls were ſhook each of the fluids appeared like milk, but the phoſphorus would ſoon ſubſide when the balls were at reſt, and leave the ſpirit of wine and æther quite clear. After ſome months, the ſpirit of wine was found to be tinged with yellow, but the æther remained unaltered to December, 1768, when this account was read at the Royal Society. By ſhaking the balls, while they were expoſed to the light, the whole of the fluid in each appeared luminous when carried into a dark room; and the æther gave as much light then as it did at firſt, but the ſpirit of wine a little leſs.†

That *heat* promotes the expulſion of the light which has been formerly imbibed by theſe phoſphori is greatly and manifeſtly in favour of the materiality of light, and the effects of this property were obſerved very early, though the complete diſcovery of it was not made till long afterwards.

Mentzel, who wrote preſently after the diſcovery of Baldwin's phoſphorus, which he compares with the Bolognian, aſſerted that it had the property of becoming luminous by heat only; § and M. Du Fay, we have ſeen, obſerved that his diamond emitted light after having been buried for a long time in black ſealing wax, which was, no doubt, a fact of the ſame kind; but the diſcovery of tne principle on which they depended was made by Beccarius, M. Margraaf, and Mr. Canton, independent of one another.

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* Phil. Tranſ. p. 339. † Ib. § Ac. Berl. 1749, p. 70.

Beccarius, endeavouring, like M. Du Fay, to confine the light in his phoſphoreal diamond, accidentally obſerved, that heat would make it emit the light which had lain dormant in it a conſiderable time, and particularly after having been kept very cold. Having plunged an illuminated diamond in cold water, where he kept it two days, he found that by putting it in his mouth, after that time, it gave as much light as when it was put into the water. Immediately dipping it in the water again, the light became extinct; but once more warming it again in his mouth, where it was equally moiſt, the light revived. This experiment he repeated, till no more light appeared, notwithſtanding any degree of heat that he could communicate to the diamond.* Beccarius was firſt of opinion, with Mentzel, that this light was *produced* by heat; but finding, by repeated trials, that, without previous expoſing to the light, heat had no effect; and eſpecially that the application of heat made the phoſphori ceaſe to ſhine ſooner than they would otherwiſe have done, he gave up that opinion. Among other deciſive experiments to this purpoſe, he filled a glaſs tube with pounded Bolognian phoſphorus, and after expoſing it to the light, he plunged one half of it in warm water, and obſerved that it gave light more copiouſly than that part which was in the open air; but that this part continued to be luminous, long after that which was under water had ceaſed to give light at all.†

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* Com. Bonon. Vol. 2, p. 284. † Vol. 5, p. 112.

M. Margraaf made the ſame obſervation, and even fell into the ſame miſtake with Beccarius, imagining that heat produced the light;* and he corrected his miſtake in a ſubſequent memoir,† in conſequence of obſerving, that the phoſphorus would not ſhine by being placed upon a hot furnace, unleſs it had been expoſed to the light two or three days before. Upon the whole, he concludes that the light is held in this ſubſtance by attraction, and afterwards expelled by heat; for he alſo obſerved, that if the phoſphorus remained a long time upon the furnace, the light was intirely exhauſted.

Mr. Canton ſeems not to have been acquainted with the obſervations of Beccarius, or M. Margraaf, when he made the ſame diſcovery that they had done; and though his was made ſubſequent to theirs; yet as his experiments are more particular, were made with a better kind of phoſphorus, and give us a clearer idea of the degree in which it is poſſeſſed of this power, and alſo of ſome other circumſtances relating to it, I ſhall recite them at ſome length. They were made with that excellent artificial phoſphorus, the compoſition of which has been mentioned already. By a variety of experiments, he found that, when this ſubſtance had imbibed light, and had emitted all that it could in the common ſtate of the atmoſphere, it would emit more, upon the application of heat, in a variety of methods; but, ſtill, that a continuance of the ſame degree of heat would only make it luminous for a certain time. He therefore concludes, that there is a ſtrong attraction between light and the particles of natural bodies, and that the ſtrong vibrations into which heat throws them compels them, as it were, to quit their hold of each other.

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* Ac. Berl. 1749, p. 70.

† Ib. 1750, p. 160.

The light which this phoſphorus gives, by being heated to a certain degree, appears, as he obſerves, to be cauſed by its throwing off adventitious particles, and not by any of its own; ſince its light will decreaſe, and be intirely gone, before the phoſphorus be hot enough to ſhine of itſelf, or to emit particles of light from its own body.

He expoſed his phoſphorus in a dry ſtate, in a glaſs ball, ſealed hermetically, to the light of the day, by holding it on the outſide of a north window, about half a minute; after which it was kept in the dark for two days and a half, and was then found to ſhine, by putting the glaſs ball which contained it into a baſon of boiling water. On the morrow it was expoſed to the light again, and after it had been kept four days and a half in the dark, it gave light when put into boiling water, though not ſo much as before. In ſummer he found that it would not give any light by the heat of boiling water, after keeping it fifteen days, but in winter it would afford a little after keeping it a month.

Parcels of phoſphorus incloſed in two of thoſe glaſs balls were illuminated at the ſame time, and to the ſame degree, and then carried into a dark room, where one of the balls was immediately put into a baſon of boiling water, when the phoſphorus in it became much brighter than that in the other, and continued ſo for a ſhort time, but it parted with its light ſo faſt, than in leſs than ten minutes it was quite dark; whereas the other phoſphorus, ſtill gave a conſiderable degree of light, and remained viſible for more than two hours after, and then even the heat of the hand would plainly increaſe its light.

Bolognian phoſphorus is ſaid by Lemery, and alſo by Muſſchenbroeck, to imbibe leſs light when hot than when cold, as it appears leſs bright when carried into a dark room. But this appearance, our author obſerves, may be cauſed by its parting with the light it has received faſter when in the former ſtate than when in the latter, according to the laſt experiment; as it muſt loſe more when hot than when cold, during the time of conveying it from the place where it takes the light, to a place dark enough to obſerve it in. And this, ſays he, ſeems alſo to be the cauſe why Bolognian phoſphorus never appears ſo bright after it has been illuminated (and conſequently, in ſome meaſure, heated) by the direct beams of the ſun, as after it has only been expoſed in the ſhaded open air, to the common light of the day. I would beg leave to obſerve, that the ſame ſtate of heat, which diſpoſes the phoſphorus to throw off the light after it has been imbibed, muſt likewiſe render it indiſpoſed to receive it; ſo that, in thoſe circumſtances, it will in reality imbibe leſs light, as I doubt not would be found to be the caſe, if the head of the obſerver was cloked up, and the phoſphorus preſented to him immediately after being expoſed to the light.

The balls mentioned in the laſt experiment were kept in the dark for two days afterwards, and then each, at the ſame time was put into a baſon of boiling water, in a dark room; when that which had parted with its light in the hot water before was not viſible, whereas the other appeared luminous for a conſiderable time.

When the phoſphorus has once loſt as much of the light it has received, as the heat of boiling water will cauſe it to part with, it has never been found, he ſays, if kept in the dark, to give any more light by that degree of heat; but if it be expoſed again to the common light of the day, the experiments may be repeated with the ſame ſucceſs as before. This, ſays he, has been frequently done with ſome dry phoſphorus, in glaſs balls, which had been hermetically ſealed about four years, without the leaſt injury to it.

Let one end of a bar of iron of about an inch ſquare, or a poker be made red hot, and laid horizontally in a darkened room, till, by cooling, it ceaſes to ſhine, or is but barely viſible. Then bring a little dry phoſphorus, which has been expoſed to the light in a glaſs ball hermetically ſealed as near the hot iron as poſſible, by holding the ball in contact with it, and the phoſphorus, he ſays, though inviſible before, will, in a few ſeconds, begin to ſhine, and will diſcharge its light ſo very faſt, as to be intirely exhauſted of it in leſs than a minute, and will ſhine no more by the ſame treatment, till after it has been expoſed to the light again. But by this heat, light received from a candle, or even from the moon, may be ſeen ſeveral days after, and phoſphorus that will afford no more light by the heat of boiling water will ſhine again by the heat of hot iron. By means of this heat, phoſphorus which had been kept in darkneſs more than ſix months was found to give a conſiderable degree of light.*

That light is a real ſubſtance, conſiſting of particles of matter emitted from luminous bodies, is farther favoured by thoſe experiments which demonſtrate that the *colour*, and *inward texture* of ſome bodies are changed, in conſequence of their being expoſed to the light. The firſt obſervation of this kind appears to have been made by M. Duhamel, who found that the juice of a certain ſhell fiſh in Provence contracted a fine purple colour when it was expoſed to the light of the ſun, and that the ſtronger was the light, the more ſplendid was the colour. Pieces of cloth dipped in this liquor, and expoſed to the ſun, became red, though they were incloſed in glaſs; but they got none of this colour if, in the ſame expoſure, they were covered with the thinneſt plates of metal. If the liquor was incloſed in veſſels covered with oiled paper, it contracted the colour, eſpecially if the paper itſelf was violet, but it remained colourleſs, if the paper was dry.

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* Phil. Tranſ. Vol. 58, p. 340.

After this, Beccarius obſerved that a quantity of luna cornea, expoſed to the rays of the ſun, became of a violet colour, when part of the ſame compoſition, covered with black paper, remained white.*

It does not appear that Beccarius knew on what ingredient in this compoſition the change of colour depended; but ſome obſervations, which I ſhall here introduce, of M. H. Schulze make it appear to have been the ſilver. This gentleman having put ſome aqua fortis, in which a ſmall quantity of ſilver happened to have been diſſolved, to a quantity of chalk (in order to make Baldwin's phoſphorus) and caſually ſetting the phial which contained it in a window, on which the ſun ſhone very bright, obſerved, after a few minutes, that the colour of it was changed from a dark red to nearly a blue, on the ſide that was next the light; whereas the colour on the other ſide was not changed. Varying the circumſtances of this experiment, he preſently found that this effect was produced not by *heat*, but by the *light*; for when it was covered, it would bear a very great degree of heat without any change of colour, and when he only wrapped a few threads round the phial, the colour of that part of the compoſition which was under them was not changed like the reſt.

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* Com. Bonon. Vol. 4, p. 75.

When he covered the whole phial with white paper, in which letters had been cut, they were legible when the paper was removed, by the difference of their colour. This, he ſays, made a pleaſing experiment, no perſon who ſaw it being able to gueſs in what manner the letters had been made. Trying a piece of the ſaturated chalk, he found it to have the ſame property with the ſolution itſelf; and in all theſe caſes, he ſays, the change of colour will inſtantly appear, if a burning mirror be made uſe of.

In his inquiries into the *cauſe* of this appearance, he found that it did not depend either on the *nitre*, or the *chalk*, but on the ſmall quantity of *ſilver* that happened to be in it. For rectified ſpirit of nitre and chalk only had no ſuch effect, whereas a ſolution of ſilver in aqua fortis underwent the ſame change of colour with his firſt compoſition; and when, in this, he uſed a little more ſilver, the effect was much more conſpicuous. He recommends the uſe of magneſia, burnt harts-horn, and other white ſubſtances inſtead of chalk. He found the experiment to anſwer pretty well with white ceruſs of lead; and he concludes theſe obſervations, with hinting that, perhaps, ſome uſe may be made of them, in attempts to diſcover whether ores, and other ſubſtances, contain any ſilver in them.*

The obſervations of Duhamel, and his own obſervation of the change of colour in the luna cornea, abovementioned, led Beccarius, in a converſation with ſeveral members of the academy at Bologna, on the ſubject of preſerving flowers, to ſuggeſt an attention to the circumſtance of *light* in their experiments; and accordingly, G. Bonzius made ſeveral, which ſeem to prove pretty clearly, that various colours are conſiderably affected by light, excluſive of heat, or any thing elſe. When he had expoſed a number of differently coloured ribbons to the rays of the ſun, for ſeveral days, in the open air, he found that the red, deep green, and blue, had loſt part of their luſtre; but that the violet was ſo much faded, that it could not have been known, if it had not been marked beforehand. The roſe colour alſo had nearly diſappeared, but the yellow and light green were not affected.*

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* Ac. Cæſar, Vol. 1, p. 528, &c.

Expoſing pieces of the ſame ribbons to a much greater degree of heat, in a dark room, none of the colours were affected, except that a ſmall part of their luſtre was loſt. Alſo, no ſenſible change was made in any of them after remaining, for a conſiderable time, in a room that faced the north; except that the roſe colour was leſs vivid.†

That the *air* did not contribute to this change of colour was manifeſt by incloſing them in an exhauſted receiver; for, in this ſituation, they underwent the ſame change of colour, when expoſed to the light, as before. He found, however, that he could produce no change in any of the colours by the light of torches, even when it was collected to a focus, by very large lenſes; but this he attributed to the weakneſs of the light.§

It was conjectured by ſome, that the rays of the ſun diſperſed thoſe parts of the bodies on which the colours depended, but Bonzius obſerved, that when his ribbons were expoſed on white paper, the colours vaniſhed from both their ſides, but that nothing could be found near the places where they had lain.‡

Beccarius himſelf ſuſpected that many other changes in the internal ſtructure of bodies were made by the light of the ſun, and he was ſatisfied that ſeveral ſubſtances which had the property of imbibing light were much injured by them, in that reſpect. He found that paper, after it had been made red hot, and cooled again, was an excellent phoſphorus of this kind; but he was ſatisfied, notwithſtanding his firſt ſuſpicions to the contrary, that it was greatly injured by being expoſed to the light. Having covered one part of the ſame piece of paper, while the other part was expoſed to the rays of the ſun, for ſome hours only; and then expoſing the whole for a very ſhort time, that they might imbibe the light, the part which had been covered was very luminous in the dark, while the other was hardly viſible. That this change did not ariſe from the *heat* which it had acquired by being expoſed to the ſun, was evident by expoſing another piece, part of which was covered with black ſilk, in conſequence of which it was manifeſtly hotter than the part that was uncovered; notwithſtanding which, the reſult of the experiment was the ſame as before.*

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* Com. Bonon. Vol. 6, p. 77. † Ib. p. 78. § Ib. p. 79. ‡ Ib.

The effect was the ſame whatever kind of this phoſphorus he made uſe of; and for this purpoſe he made experiments with a great variety of ſubſtances, ſome mineral, ſome vegetable, and ſome animal. Alſo the ſtronger the light was, and the longer they were expoſed to it, the more injury they received. The difference of an hour was very ſenſible, and a much leſs ſpace of time when the light was collected to a focus by a mirror.* When one piece of paper was laid upon another, both of them being of a moderate thickneſs, the upper was injured, but the lower not; but when he made the experiment with three pieces, of a thinner kind, two of the uppermoſt were injured, but not the loweſt.

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* Com. Bonon. Vol. 6, p. 81. † Ib. p. 83.

The injury which theſe phoſphori received from being expoſed to the rays of the ſun was alſo laſting. He kept one of them a whole year in a dark room, but ſufficiently expoſed to the air, and in all this time it had not recovered its former virtue. He likewiſe waſhed and dried it, and alſo expoſed it to fumes, of every kind that he could think of; but, whatever he did, he could make no improvement in it; and yet it had been a moſt excellent phoſphorus before it had been injured by the ſun.* This ſurpriſed our philoſopher very much, becauſe if any phoſphorus had been injured by heat, it was reſtored by cold; or if it had been hurt by moiſture, it recovered its virtue as ſoon as it was thoroughly dry.

CHAPTER III.

Of the extreme SUBTILTY and MOMENTUM of light, and M. BOSCOVICH's general HYPOTHESIS.

ADMITTING the materiality of light, it muſt, however, be acknowledged, that the particles of which it conſiſts are extremely minute, and, notwithſtanding its amazing velocity, that its *momentum* is very ſmall.

We are obliged to Mr. Melville above mentioned for ſome ingenious illuſtrations concerning the extreme *ſubtilty of light*, or the ſmallneſs of the particles of which it conſiſts, of which few perſons, even of thoſe who admit the hypotheſis, have any tolerable idea.

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* Com. Bonon. Vol. 6, p. 85.

He obſerves that there is, probably, no phyſical point in the viſible horizon, that does not ſend rays to every other point, unleſs where opaque bodies interpoſe. Light, in its paſſage from one ſyſtem to another, often paſſes through torrents of light, iſſuing from other ſuns and ſyſtems, without ever interfering, or being diverted from its courſe, either by it, or by the particles of that elaſtic medium, which ſome phenomena give us reaſon to ſuppoſe to be diffuſed through all the mundane ſpace. To account for this fact, and others ſimilar to it, he concludes that the particles of which light conſiſts muſt be incomparably rare, even when they are the moſt denſe; that is, that the ſemidiameters of two of the neareſt particles, in the ſame, or in different beams, ſoon after their emiſſion, are incomparably leſs than their diſtance from one another. Had M. Euler, he ſays, conſidered this extreme rarity, as well as tenuity of light (which muſt be acknowledged by all who ſuppoſe that its particles are actually projected from the lucid body) he would not have alledged that this opinion is inconſiſtent with the freedom and perpetuity of the celeſtial motions.[*]

This difficulty concerning the non-interference of the particles of light is not ſolved, as he obſerved, by ſuppoſing, with Boſcovich and others, that each particle is endued with an inſuperable impulſive force; becauſe, in that caſe, their ſpheres of impulſion would even be more liable to interfere, and they would, on that account, be more likely to diſturb one another.[*]

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[*] Edinb. Eſſays, Vol. 2, p. 17. [†] Ib. p 18.

Mr. Canton, by means of a very eaſy computation, advances a good deal towards anſwering the objection that has often been made to the Newtonian doctrine of light, from the ſuppoſition that, if it conſiſts of real particles of matter, they muſt frequently impinge upon one another, and upon other particles, when every part of ſpace muſt contain thouſands of them. This difficulty, he obſerves, will nearly vaniſh, if a very ſmall portion of *time* be allowed between the emiſſion of every particle and the next that follows in the ſame direction. Suppoſe, for inſtance, that one lucid point of the ſun's ſurface emits one hundred and fifty particles in one ſecond, which are more than ſufficient to give continual light to the eye, without the leaſt appearance of intermiſſion; yet ſtill the particles of which it conſiſts will, on account of their great velocity, be more than a thouſand miles behind one another, and thereby leave room enough for others to paſs in all directions.*

It would certainly go a great way towards proving the *materiality* of the rays of light, if it could be obſerved that they had any *momentum*, ſo as, by their impulſe, to give motion to light bodies. M. Homberg imagined that he could not only diſperſe pieces of amianthus, and other light ſubſtances, by the impulſe of the ſolar rays, but alſo that, by throwing them upon the end of a kind of lever, connected with the ſpring of a watch, he could make it move ſenſibly quicker, juſt as it did when it was preſſed with the end of a ſtick.† Theſe experiments, the French hiſtorian ſays, agree very well with others which prove the *weight* of the particles of light. This he advances not ironically, but very gravely; and even M. Muſchenbroek mentions theſe experiments of M. Homberg, without intimating any ſuſpicion of their inaccurracy.*

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* Phil. Tranſ. Vol. 58. p. 344.

† Ac. Par. 1708, H. p. 25.

M. Euler's hypotheſis that the tails of comets are vapours driven from the ſun, by the impulſe of thoſe vibrations of the ætherial medium, in which he ſuppoſed the rays of light to conſiſt, engaged M. Mairan to conſider this ſubject, and to make ſome experiments, which induced him to call in queſtion the fact of this impulſe. He was aware that Hartſocker, as well as M. Homberg, had aſcribed extraordinary effects to this power, but he ſhews that they were owing to a current of air, excited by the heat of the burning glaſſes, or to ſome other cauſes which were overlooked by thoſe philoſophers; and in order to make an experiment which might decide this queſtion with more circumſpection, he began with trying the effects of rays collected by lenſes of four and ſix inches in diameter, and thrown upon the needle of a compaſs four or ſix inches long; but the reſult was nothing but ſome tremulous motions, from which he could draw no concluſion. After this, he and M. Du Fay conſtructed a kind of mill of copper, which moved with an exceedingly ſlight impulſe; but though they threw upon it the focus of a lens of ſeven or eight inches in diameter, they were ſtill unable to draw any concluſion from the reſult.

M. Mairan afterwards procured a horizontal wheel of iron, three inches in diameter, having ſix radii, at the extremity of each of which was a ſmall wing fixed obliquely. The axis of the wheel, which was alſo of iron, was ſuſpended by a magnet. The wheel and the axis together did not weigh more than thirty grains; but though a motion was given to this wheel, when the focus of the burning glaſs was thrown upon the extremities of the radii, yet it was ſo irregular, that he could not but conclude that it was occaſioned by the motion of the heated air. He then intended to have made his experiment in vacuo, but he concluded that it was unneceſſary. For, beſides the difficulty of making a vacuum, he was perſuaded that there was a thinner medium mixed with our atmoſphere which freely penetrates even the glaſs itſelf, the exiſtence of which he imagined that he had ſufficiently proved in his treatiſe on the Aurora Borealis.*

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* Introductio, p. 689.

Mr. Michell, ſome years ago, endeavoured to aſcertain the momentum of light in a much more accurate manner than thoſe in which M. Homberg and M. Mairan had attempted it; and though his apparatus was diſordered by the experiment, and, on other accounts, he did not purſue it ſo far as he had intended, it was not wholly without ſucceſs, and the concluſions that may be drawn from it are curious and important.

The inſtrument he made uſe of for this purpoſe conſiſted of a very thin plate of copper, a little more than an inch ſquare, which was faſtened to one end of a ſlender harpſicord wire, about ten inches long. To the middle of this wire was fixed an agate cap, ſuch as is commonly uſed for ſmall mariners compaſſes, after the manner of which it was intended to turn; and at the other end of the wire, was a middling ſized ſhot corn, as a counterpoize to the copper-plate. This inſtrument had alſo fixed to it in the middle, at right angles to the length of the wire, and in an horizontal direction, a ſmall bit of a very ſlender ſewing needle, about one third, or perhaps half an inch long, which was made magnetical. In this ſtate the whole inſtrument might weigh about ten grains. It was placed upon a very ſharp pointed needle, on which the agate cap turned extremely freely; and, to prevent its being diſturbed by any motion of the air, it was included in a box, the lid and front of which were of glaſs. This box was about twelve inches long, ſix or ſeven inches deep, and about as much in width; the needle ſtanding upright in the middle.

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* Ac. Par. 1747, M. p. 630.

At the time of making the experiment, the box was placed in ſuch a manner, that a line drawn from the ſun paſſed at right angles, to the length of it; and the inſtrument was brought to range in the ſame direction with the box, by means of the magnetical bit of a needle abovementioned, and a magnet properly placed on the outſide, which would retain it, though with extreamly little force, in any ſituation. The rays of the ſun were now thrown upon the copper-plate above mentioned, from a concave mirror of about two feet diameter, which, paſſing through the front glaſs of the box, were collected into the focus of the mirror upon the copper plate. In conſequence of this, the copper plate began to move, with a ſlow motion, of about an inch in a ſecond of time, till it had moved through a ſpace of about two inches and a half, when it ſtruck againſt the back of the box. The mirror being removed, the inſtrument returned to its former ſituation, by means of the little needle and magnet; and the rays of the ſun being then again thrown upon it, it again began to move, and ſtruck againſt the back of the box as before; and this was repeated three or four times, with the ſame ſucceſs.

The inſtrument was then placed the contrary way in the box to that in which it had been placed before, ſo that the end, to which the copper-plate was affixed, and which had lain in the former experiment towards the right hand, now lay towards the left; and the rays of the ſun being again thrown upon it, it began to move with a ſlow motion, and ſtruck againſt the back of the box as before; and this was repeated once or twice with the ſame ſucceſs. But by this time the copper-plate began to be ſo much altered in its form, by the extreme heat which it underwent in each experiment, and which brought it very nearly into a ſtate of fuſion, ſo that it became very much bent, and the more ſo, as it had been, unwarily, ſupported by the middle, half of it lying above and half below the wire, to which it was faſtened. By this means it now varied ſo much from the vertical poſition, that it began to act in the ſame manner as the ſail of a windmill, being impelled by the ſtream of heated air, which moved upwards, with a force ſufficient to drive it in oppoſition to the impulſe of the rays of light. Not having a mirror at his own houſe, he did not proſecute the experiment any farther; as it well deſerves to be. There ſeems to be no doubt, however, but that the motion abovementioned is to be aſcribed to the impulſe of the rays of light. The ſeveral weights, meaſures, &c. Mr. Michell gave me only from his memory; but he thought they could not differ much from the truth.

If we impute the motion produced in the above experiment to the impulſe of the rays of light, and ſuppoſe, that the inſtrument weighed ten grains, and acquired a velocity of one inch in a ſecond, we ſhall find that the quantity of matter contained in the rays falling upon the inſtrument in that time, amounted to no more than one twelve hundred millionth part of a grain, the velocity of light exceeding the velocity of one inch in a ſecond, in the proportion of about twelve thouſand millions to one, for it is nearly after the rate of two hundred thouſand miles in a ſecond.

cond. Now the light in the above experiment was collected from a ſurface of about three ſquare feet, which reflecting only about half what falls upon it, the quantity of matter contained in the rays of the ſun, incident upon a ſquare foot and half of ſurface, in one ſecond of time, ought to be no more than the twelve hundred millionth part of a grain, or upon one ſquare foot only, the eighteen hundred millionth part of a grain. But the denſity of the rays of light at the ſurface of the ſun is greater than at the earth in the proportion of forty five thouſand to one; there ought, therefore, to iſſue from one ſquare foot of the ſun's ſurface in one ſecond of time, in order to ſupply the waſte by light, one forty thouſandth part of a grain of matter, that is a little more than two grains in a day, or about four millions ſeven hundred and fifty two thouſand grains, which is about ſix hundred and ſeventy pounds avoirdupois, in ſix thouſand years, a quantity which would have ſhortened the ſun's ſemidiameter no more than about ten feet, if it was formed of matter of the denſity of water only.

The eaſieſt method of ſolving all the difficulties, attending the ſubject of this ſection, and of anſwering M. Euler's objections to the materiality of light, is to adopt the hypotheſis of M. Boſcovich, who ſuppoſes that matter is not impenetrable, as has been, perhaps, univerſally taken for granted, but that it conſiſts of phyſical points only, endued with powers of attraction and repulſion, taking place at different diſtances; that is, ſurrounded with various ſpheres of attraction and repulſion, in the ſame manner as ſolid matter is generally ſuppoſed to be: Provided, therefore, that any body move with a ſufficient degree of velocity, or have ſufficient *momentum*, to overcome any powers of repulſion that it may meet with, it will find no difficulty in making its way through any body whatever; for nothing will interfere, or penetrate one another, but *powers*, ſuch as we know, do, in fact, exiſt in the ſame place, and counterbalance, or overrule one another; a circumſtance which never had the appearance of a contradiction, or even of a difficulty.

If the momentum of ſuch a body in motion be ſufficiently great, M. Boſcovich demonſtrates that the particles of any body through which it paſſes, will not even be moved out of their place by it. With a degree of velocity ſomething leſs than this, they will be conſiderably agitated, and ignition might perhaps be the conſequence, though the progreſs of the body in motion would not be ſenſibly interrupted; and with a ſtill leſs momentum, it might not paſs at all.*

This theory M. Boſcovich has taken a great deal of pains to draw out at full length, and illuſtrate; ſhewing that it is by no means inconſiſtent with any thing that we know concerning the laws of mechanics, or our diſcoveries in natural philoſophy; and that a great variety of phenomena, particularly thoſe which reſpect light, admit of a much eaſier ſolution upon this hypotheſis than any other.

The moſt obvious difficulty, and, indeed, almoſt the only one that attends this hypotheſis, as it ſuppoſes the mutual *penetrability of matter*, ariſes from the idea we have formed of the nature of matter, and the difficulty we meet with in attempting to force two bodies into the ſame place. But it is demonſtrable that the firſt obſtruction ariſes from no actual contact of matter, but from mere powers of repulſion. This difficulty we can overcome;

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* Theoria Philoſophiæ Naturalis, p. 167.

and having got within one ſphere of repulſion, we fancy that we are now impeded by the ſolid matter itſelf. But the very ſame is the apprehenſion of the generality of mankind with reſpect to the firſt obſtruction. Why, therefore, may not the next be only another ſphere of repulſion, which may only require a greater force than we can apply to overcome it, without diſordering the arrangement of the conſtituent particles, but which may be overcome by a body moving with the amazing velocity of light.

This ingenious hypotheſis, at leaſt, one that is the ſame in every thing eſſential, occurred alſo to my friend Mr. Michell, in a very early period of his life, without his having had any communication with M. Boſcovich, or even knowing that there was ſuch a perſon. Theſe two philoſophers had even hit upon the ſame inſtances, to confirm and illuſtrate their hypotheſes, eſpecially thoſe relating to contact, light, and colours.

This ſcheme of the *immateriality of matter*, as it may be called, or rather the *mutual penetration of matter*, firſt occurred to Mr. Michell on reading *Baxter on the immateriality of the ſoul*. He found that this author's idea of matter was, that it conſiſted, as it were, of bricks, cemented together by an immaterial mortar. Theſe bricks, if he would be conſiſtent to his own reaſoning, were again compoſed of leſs bricks, cemented, likewiſe, by an immaterial mortar, and ſo on *ad infinitum*. This putting Mr. Michell upon the conſideration of the ſeveral appearances of nature, he began to perceive that the bricks were ſo covered with this immaterial mortar, that if they had any exiſtence at all, it could not poſſibly be perceived, every *effect* being produced, at leaſt, in nine inſtances in ten certainly, and probably in the tenth alſo, by this immaterial, ſpiritual, and penetrable mortar. Inſtead, therefore, of placing the world upon the giant, the giant upon the tortoiſe, and the tortoiſe upon he could not tell what, he placed the world at once upon itſelf; and finding it ſtill neceſſary, in order to ſolve the appearances of nature, to admit of extended and penetrable immaterial ſubſtance, if he maintained the impenetrability of matter, and obſerving farther, that all we perceive by contact, &c. is this penetrable immaterial ſubſtance, and not the impenetrable one, he began to think he might as well admit of *penetrable material*, as well as *penetrable immaterial ſubſtance*, eſpecially as we know nothing more of the nature of *ſubſtance*, than that it is ſomething which ſupports *properties*, which properties may be whatever we pleaſe, provided they be not inconſiſtent with each other, that is, do not imply the abſence of each other. This by no means ſeemed to be the caſe in ſuppoſing two ſubſtances to be in the ſame place at the ſame time, without excluding each other; the objection to which is only derived from the reſiſtance we meet with to the touch, and is a prejudice that has taken its riſe from that circumſtance, and is not unlike the prejudice againſt the *Antipodes*, derived from the conſtant experience of bodies falling, as we account it, downwards.

I hope I ſhall be excuſed dwelling ſo long on this hypotheſis, on account both of the *novelty* and *importance* of it, eſpecially with reſpect to the phenomena of light. If I were to make any alteration in it, it would be to ſuppoſe the force of the ſphere of repulſion, next to any of the indiviſible points which conſtitute what we call ſolid bodies, not to be abſolutely *infinite*, but ſuch as may be overcome by the momentum of light, which will obviate the objection of Mr. Melville above mentioned. If, however, we conſider that M. Boſcovich makes this neareſt power of repulſion not to extend to any real ſpace, but to be confined to the indiviſible point itſelf, it may appear to be ſufficient for the purpoſe; ſince the chance of ſuch points impinging upon one another is ſo little, that it needs not to be conſidered at all.

SECTION II.

Observations concerning the VELOCITY OF LIGHT.

THIS period of my hiſtory produced a remarkable confirmation of the diſcovery of M. Roemer, concerning the ſucceſſive propagation of light, and likewiſe a proof that the velocity of light is the ſame after reflexion as after its firſt emiſſion. Like many other great diſcoveries, this was made when the author was in purſuit of ſomething elſe, and a thing of a very different nature. It is alſo pleaſing to obſerve the ſucceſſion of hypotheſes that occurred to the ingenious diſcoverer in the courſe of this inquiry.

Dr. Bradley, aſtronomer royal, and his friend Mr. Molyneux, thinking to verify ſome obſervations of Dr. Hooke, concerning the parallax of the fixed ſtars, obſerved the ſtar γ. *Draconis*, at Kew, on the 20th of December 1725, and found that it appeared more ſoutherly than it had done at the beginning of the ſame month and the month preceding. This ſurpriſed them, and the more ſo, as it was the contrary way from what it would have been, had it proceeded from an annual parallax. But being pretty well ſatisfied that it could not be intirely owing to the want of exactneſs in the obſervations, and having no notion of any thing elſe that could cauſe ſuch an apparent motion as this in the ſtar, they began to think that ſome change in the materials, &c. of the inſtrument itſelf might have occaſioned it. In this perſuaſion they remained ſome time; but being at length fully convinced, by ſeveral trials, of the great exactneſs of the inſtrument; and finding, by the gradual increaſe of the diſtance of the ſtar from the pole, that there muſt be ſome regular cauſe that produced it, they took care to examine nicely, at the time of each obſervation, how much it was; * and, after ſeveral obſervations for that purpoſe, the firſt hypotheſis that occurred them was, that it was occaſioned by the nutation of the earth's axis; but this was ſoon found to be inſufficient. They alſo found that it did not depend upon any variety of the ſeaſons of the year, meaning probably the variation of the ſtate of the atmoſphere; and comparing their obſervations upon two ſtars together, it appeared that the apparent differences of declination from the maxima vere always nearly proportional to the verſed ſine of the ſun's diſtance from the equinoctial points. This was an inducement to think that the cauſe, whatever it was, had ſome relation to the ſituation of the ſun with reſpect to thoſe points. But not being able to frame any hypotheſis, at that time, ſufficient to ſolve all the phænomena,

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* Phil. Tranſ. ab. Vol. 6, p. 168.

na, and being very deſirous to ſearch a little farther into this matter, Dr. Bradley began to think of erecting an inſtrument for himſelf at Wanſted; that, having it always at hand, he might, with the more eaſe and certainty, inquire into the laws of this new motion.

He had not been long in obſerving, before he perceived that the notion he had before entertained, of the ſtars being fartheſt north and ſouth, when the ſun was about the equinoxes, was only true of thoſe that were near the ſolſtitial colure; and after he had continued his obſervations a few months, he diſcovered what he then apprehended to be a general law, obſerved by all the ſtars, viz. that each of them became ſtationary, or was fartheſt North or South when they paſſed over his zenith, at ſix of the clock, either in the morning or the evening. He perceived, likewiſe, that whatever ſituation the ſtars were in, with reſpect to the cardinal points of the ecliptic, the apparent motion of every one tended the ſame way, when they paſſed his inſtrument about the ſame hour of the day or night; for they all moved ſouthward while they paſſed in the day, and northward in the night; ſo that each was fartheſt North, when it came about ſix of the clock in the evening, and fartheſt South when it came about ſix in the morning.

Examining the matter more particularly, he found that the greateſt alteration of declination in theſe ſtars was as the ſine of the latitude of each reſpectively. This made him ſuſpect that there might be the like proportion between the maxima of other ſtars; but finding that the obſervations of ſome of them would not perfectly correſpond with ſuch an hypotheſis, and not knowing whether the ſmall difference he met with might not be owing to the uncertainty and error of the obſervations, he deferred the farther examination into the truth of this hypotheſis, till he ſhould be furniſhed with a ſeries of obſervations, made in all parts of the year; which might enable him not only to determine what errors the obſervations were liable to, or how far they might be ſafely depended upon, but alſo to judge whether there had been any ſenſible change in the parts of the inſtrument itſelf.

Upon theſe conſiderations he laid aſide all thoughts at that time about the cauſe of the fore-mentioned phænomena; hoping that he ſhould the more eaſily diſcover it, when he was better provided with proper means to determine more preciſely what they were.

When the year was completed, he began to examine and compare his obſervations; and having pretty well ſatisfied himſelf as to the general laws of the phenomena, he endeavoured to find out the cauſe of them. He was already convinced that the apparent motion of the ſtars was not owing to a nutation of the earth's axis. The next thing that offered itſelf was an alteration in the direction of the plumb line with which the inſtrument was conſtantly rectified, but this, upon trial, proved inſufficient. Then he conſidered what refraction might do, but here alſo nothing ſatisfactory occurred. At laſt he conjectured that all the phenomena hitherto mentioned proceeded from the progreſſive motion of light, and the earth's annual motion in its orbit. For he perceived that if light was propagated in time, the apparent place of a fixed object would not be the ſame when the eye is at reſt, as when it is moving in any other direction than that of the line paſſing through the eye and the object, and that when the eye is moving in different directions, the apparent place of the object would be different.

He conſidered this matter in the following manner: He imagined CA, fig. 103, to be a ray of light falling perpendicularly upon the line BD; that, if the eye is at reſt at A, the object muſt appear in the direction AC, whether light be propagated in time or in an inſtant. But if the eye is moving from B towards A, and light is propagated in time, with a velocity that is to the velocity of the eye, as CA to BA, then light, moving from C to A, whilſt the eye moves from B to A, that particle of it by which the object will be diſcerned when the eye comes to A, is at C when the eye is at B. Joining the points B,C, he ſuppoſed the line CB to be a tube, inclined to the line BD in the angle DBC, of ſuch a diameter as to admit but one particle of light. Then it was eaſy to conceive, that the particle of light at C, by which the object muſt be ſeen, when the eye, as it moves along, arrives at A, would paſs through the tube BC, if it is inclined to BD in the angle DBC, and accompanies the eye in its motion from B to A; and that it could not come to the eye placed behind ſuch a tube, if it had any other inclination to the line BD. If, inſtead of ſuppoſing CB ſo ſmall a tube, we imagine it to be the axis of a larger; then, for the ſame reaſon, the particle of light at C would not paſs through that axis, unleſs it is inclined to BD in the angle CBD. In like manner, if the eye moved the contrary way, from D towards A, with the ſame velocity, then the tube muſt be inclined in the angle BDC. Although, therefore, the true, or *real* place of an object is perpendicular to the line in which the eye is moving, yet the *viſible* place will not be ſo, ſince that, no doubt, muſt be in the direction of the tube; but the difference between the true and apparent place will be, cœteris paribus, greater or leſs, according to the different proportion between the velocity of light and that of the eye. So that if we could ſuppoſe that light was propagated in an inſtant, then there would be no difference between the real and viſible place of an object, although the eye were in motion; for in that caſe, AC being infinite with reſpect to AB, the angle ACB, the difference between the true and viſible place, vaniſhes. But if light be propagated in time, it is evident, from the foregoing conſiderations, that there will be always a difference between the real and viſible place of an object, unleſs the eye is moving either directly towards or from the object. And in all caſes the ſine of the difference, between the real and viſible place of the object will be to the ſine of the viſible inclination of the object to the line in which the eye is moving, as the velocity of the eye is to the velocity of light.

He then ſhews, that if the earth revolve round the ſun annually, and the velocity of light be to the velocity of the earth's motion in its orbit, as one thouſand to one, that a ſtar really placed in the very pole of the ecliptic would, to an eye carried along with the earth, ſeem to change its place continually; and neglecting the ſmall difference on the account of the earth's diurnal revolution on its axis, would ſeem to deſcribe a circle round that pole every way diſtant from it $3' \frac{1}{2}$; ſo that its longitude would be varied through all the points of the ecliptic every year, but its latitude would always remain the ſame. Its right aſcenſion would alſo change, and its declination, according to the different ſituation of the ſun with reſpect to the equinoctial points, and its apparent diſtance from the north pole of the equator, would be $7'$ leſs at the autumnal than at the vernal equinox.

The greateſt alteration of the place of a ſtar in the pole of the ecliptic, or which, in effect, amounts to the ſame thing, the proportion between the velocity of light and the earth's motion in its orbit being known, it will not be difficult, he obſerves, to find what would be the difference, upon this account, between the true and apparent place of any other ſtar at any time; and, on the contrary, the difference between the true and apparent place being given, the proportion between the velocity of light and the earth's motion in its orbit may be found.

Now ſince the apparent declination of γ Draconis, on account of the ſucceſſive propagation of light, would be to the diameter of the little circle which a ſtar would ſeem to deſcribe about the pole of the ecliptic as 39″ to 40″, 4; the half of this is the angle ACB. This, therefore, being 20″, 2, AC will be to AB, that is the velocity of light will be to the velocity of the eye (which in this caſe may be ſuppoſed the ſame as the velocity of the earth's annual motion in its orbit) as 10210 to one; from whence it will follow that light moves, as far as from the ſun to the earth in 8′ 12″. This, Dr. Bradley obſerves, is very probably the truth, becauſe it is a medium between 7 and 11, which were the times which it had before been ſuppoſed to take up, according to different obſervations of the eclipſes of Jupiter's ſatellites. Comparing his obſervations on other ſtars, he afterwards concluded that light is propagated from the ſun to the earth in 8′ 13″ and the near agreement of his obſervations induced him to think that this ſuppoſition could not differ ſo much as a ſecond of a degree from the truth; ſo that the time which light ſpends in paſſing from the ſun to us may be determined by theſe obſervations within 5″ or 10″, which is ſuch a degree of exactneſs as we can never hope to attain from the eclipſes of Jupiter's ſatellites.

Theſe different methods thus agreeing in the reſult, he thought he might reaſonably conclude, not only that theſe phenomena are owing to the cauſes to which he had aſcribed them, but alſo that, in the ſame medium, light is propagated with the ſame velocity after it has been reflected as before. For this will be the conſequence, if it be allowed that the light of the ſun is propagated with the ſame velocity before it is reflected as the light of the fixed ſtars; which will hardly be queſtioned, if it can be made to appear that the velocity of the light of all the fixed ſtars is equal, and that their light moves through equal ſpaces in equal times, at all diſtances from them; and both theſe he thought were ſufficiently proved from the apparent alteration of the declination of ſtars of different luſtre; for that is not ſenſibly different in ſuch ſtars as ſeem near together, though they appear of very different magnitudes. Whatever their ſituations were, he found that, according to the foregoing hypotheſis, the velocity of light from ſtars of the fifth or ſixth magnitude, was the ſame as from thoſe of the ſecond or third, which, in all probability, are placed at very different diſtances from us.

Mr. Melville thought it probable, that differently coloured rays might be affected with different velocities at their emiſſion from the luminous body, and that this hypotheſis was on ſeveral accounts, preferable to Newton's ſuppoſition, that they are particles of different ſizes or denſities. Their different refrangibility, he obſerves, will be the very ſame upon this hypotheſis, and their different velocities will be nearly as the ſines of refraction out of air into glaſs, beginning from the extreme red and ending with the extreme violet, viz. as 78000, 77873, 77797, 77663, 77496, 77330, 77220, 77000, the ſine of incidence being 120120.

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This hypotheſis he thought moſt agreeable to Newton's ſuppoſition of the fits of eaſy reflexion and tranſmiſſion being occaſioned by the pulſes of the medium through which they paſs, as theſe pulſes would overtake rays, moving with different velocities, at different times.

To the objection that the different ſenſations excited in the mind cannot ariſe from the different force of the particles of light, ſince the colour of homogeneous rays is not altered by paſſing through different mediums, though their velocity be thereby always increaſed or diminiſhed, he replies that every ray, as it muſt at laſt paſs through the humours of the eye, in order to viſion, falls upon the retina with one given velocity, whatever number of refractions it has previouſly undergone; ſince the velocity of any ray in any one medium is to its velocity in any other medium, in a conſtant and invariable proportion.

Laſtly, this gentleman propoſes a trial of his hypotheſis by an experiment; for ſince, according to this ſuppoſition, the time which the extreme violet rays take to move through any ſpace are to that which the red takes as 78 to 77; the laſt violet light which a ſatellite reflects before its total immerſion into the ſhadow of Jupiter ought to affect the eye 32 ſeconds after the red light, reflected at the ſame time, is gone; and at the emerſion, there would be a contrary ſucceſſion of colours, beginning with red, and ending with white; and the difference of time between the arrival of the different coloured rays being more than half a minute, there might be ſufficient time to make the obſervation.

The Marquis De Courtivron, in a treatiſe publiſhed in 1752, advanced the ſame opinion with Mr. Melville concerning the cauſe of the different refrangibility of the rays of light, and even propoſed the ſame trial of his hypotheſis, viz. by obſerving the eclipſes of Jupiter's ſatellites; but whereas Mr. Melville ſuppoſed that the velocity of the red light exceeds that of the violet only $\frac{1}{77}$ of the whole time of their paſſage, the Marquis, making uſe of a theorem of M. Clairaut (in his inveſtigation of the curve deſcribed by a ray of light on any hypotheſis of the attractive power) found that the difference ought to be as 44 to 45. But ſince, as Mr. Clairaut informs us, Mr. Short's obſervations led him to conclude the phenomena by no means agreed even with M. Melville's hypotheſis, they were ſtill more concluſive againſt that of the Marquis, which ſuppoſes the difference of velocity to be greater.[*]

In the courſe of M. Clairaut's inquiry, concerning Mr. Dolland's improvements in teleſcopes, he examines an hypotheſis which makes the different refrangibility of the rays of light to depend upon their different velocities, but he found that the refractions which would reſult from this principle were very different from thoſe which actually take place in nature.[†]

Mr. Melville alſo queſtions whether light be emitted with the ſame velocity in whatever medium the luminous body be placed, and ſuppoſes that the velocity may be greater in denſer mediums, and that in proportion to their refractive powers. The ſame argument, he ſays, from whence we gather in general the equal velocity of light emitted by all ſorts of luminous bodies ſeems to prove the truth of the latter ſuppoſition. For ſince rays of any one colour, from the ſun and a candle, for inſtance, are equally refracted by a ſurface of glaſs or water, we may conclude that their velocities in air are equal. Whereſore if the denſity of the ſun's atmoſphere contiguous to his ſurface be different from the denſity of our lower air, as may be ſafely preſumed, his rays muſt have been emitted with more or leſs velocity than that of the candle; otherwiſe they could not have the ſame velocity afterwards in any common medium, for the velocity with which any ray is emitted is, by the laws of refraction, to its velocity in any given medium, as the ſine of refraction to the ſine of incidence, when a ray paſſes from the medium of emiſſion into the given medium.

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[*] Phil. Tranſ. Vol. 48. p. 777. [†] Ac. Par. 1756, H. p. 194.

M. Muſchenbroeck argues that, ſince the red rays have a leſs degree of refrangibility they move more ſlowly than the reſt, after their ſeparation from the other colours, though he is compelled, by aſtronomical obſervations, to acknowledge, that when they are mixed with the other rays, in one beam, they move with equal velocity. He does not, however, think that the colour depends upon the velocity, becauſe, ſince this changes with the medium through which the rays are tranſmitted, the colour of the ſame rays would change in thoſe circumſtances. * But he did not conſider, viz. the obſervation above mentioned of Mr. Melville, that whatever mediums any ray may paſs through, ſince it muſt, after all, be tranſmitted through the ſame humours of the eye, it will arrive at the retina with the ſame velocity; and that nothing but a change in the refractive power of the humours of the eye can aſcertain the truth of this hypotheſis.

I ſhall conclude this ſection with obſerving that the experiments of Mr. Dollond, which will be recited hereafter, clearly prove that the different refrangibility of the rays of light depends upon properties that are independent of different velocity;

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* Introductio, Vol. 2, p. 725.

ſince the proportion of it varies according to the nature of the ſubſtance it falls upon. The ſame is alſo manifeſt from the conſideration, that the diſtances at which differently coloured rays are attracted and repelled are different.

SECTION III.

Observations and discoveries concerning the REFLEXION of light.

CHAPTER I.

Observations on the reflexion of light at the surfaces of bodies.

THE moſt valuable materials with which I can preſent my reader on the ſubject of this ſection are furniſhed by M. Bouguer, the ſame perſon that accompanied M. Ulloa, Condamine, and others, to meaſure a degree of latitude upon the equator, and who publiſhed an account of that voyage. No philoſopher ſince the time of Sir Iſaac Newton has given ſo much attention to the ſubject of light as M. Bouguer; and, next to thoſe of that great philoſopher, his labours ſeem to have been the moſt ſuccefsful. The object of his curious and elaborate experiments was to meaſure the degrees of light whether emitted, reflected, or refracted, by different bodies. They were originally occaſioned by an article of M. Mairan's in the memoirs of the French academy for 1721, in which the proportion of the light of the ſun at the two ſolſtices was ſuppoſed to be known, and his laudable attempt to verify what had been before taken for granted, ſuggeſted a variety of new experiments, and opened to him and to the world, a new field of optical knowledge. His firſt production upon this ſubject was a treatiſe intitled *Eſſai d' Optique*, which was received with general approbation. Afterwards, giving more attention to this ſubject, he formed an idea of a much larger work, to which many more experiments were neceſſary; but he was prevented, by a variety of interruptions, from executing his deſign ſo ſoon as he had propoſed; and he had hardly completed it at the time of his death, in 1758; ſo that we are obliged to his friend M. De la Caille, for the care of the publication. At length, however, it was printed at Paris, in 1760, under the title of *Traité d' Optique*.

At the entrance upon this treatiſe, we are induced to form the moſt pleaſing expectations from our author's experiments, by his account of the variety the ſingular accurracy, and circumſpection, with which he made them; whereby he muſt, to all appearance, have guarded againſt every avenue to error, and particularly againſt thoſe objections to which the few attempts that had been made, of a ſimilar nature, before him, had been liable. In order to compare different degrees of light, he always contrived to place the bodies from which it proceeded, or other bodies illuminated by them, in ſuch a manner, as that he could view them diſtinctly at the ſame time; and he either varied the diſtances of theſe bodies, or modified their light in ſome other way, till he could perceive no difference between them. Then, conſidering their different diſtances, or the other circumſtances by which their light was affected, he calculated the proportion which they would have borne to each other, at the ſame diſtance, or in the ſame circumſtances.

I ſhall give my reader a ſufficiently clear idea of M. Bouguer's methods of meaſuring reflected light by a deſcription of ſome of them. To aſcertain the quantity of light loſt by reflexion, he placed the mirror, or reflecting ſurface, B fig. 104, on which the experiment was to be made truly upright, and having taken two tablets, of preciſely the ſame colour, or of an equal degree of whiteneſs, he placed them exactly parallel to one another at E and D, and threw light upon them, by means of a lamp or candle, P, placed in a right line between them. He then placed himſelf ſo that, with his eye at A, he could ſee the tablet E, and the image of the tablet D, reflected from the mirror B, at the ſame time; making them, as it were, to touch one another. He then moved the candle along the line ED, ſo as to throw more or leſs light upon either of them, till he could perceive no difference in the ſtrength of the light that came to his eye from them. After this, he had nothing more to do than to meaſure the diſtances EP and DP; for the ſquares of thoſe diſtances expreſſed the degree in which the reflexion of the mirror diminiſhed the quantity of light. It is evident that if the mirror reflected all the rays it received, the candle P muſt have been placed at C, at an equal diſtance from each of the tablets, in order to make them appear equally illuminated; but becauſe much of the light is loſt in reflexion, they can only be made to appear equally bright by placing the candle nearer the tablet D, which is ſeen by reflexion only.

To find how much light is loſt by oblique reflexion, he took two equally poliſhed plates D and E, fig. 105, and cauſed them to be enlightened by the candle P; and while one of them, D, was ſeen at A, by reflexion from B, placed in a poſition oblique to the eye, the other, E, was ſo placed, as to appear contiguous to it; and removing the plate E, till the light which it reflected was no ſtronger than that which came from the image of D, ſeen by reflexion at B, he eſtimated the quantity of light that was loſt by this oblique reflexion, by the ſquares of the diſtances of the two objects from the candle.

I need not add that, in theſe experiments, all foreign light was excluded, that his eye was ſhaded, and that every other precaution was obſerved, in order to make his concluſions unqueſtionable.

In order to aſcertain the quantity of light loſt by reflexion with the greateſt exactneſs, M. Bouguer introduced two beams of light into a darkened room, as by the apertures P and Q, fig. 105, which he had ſo contrived, that he could place them higher or lower, and enlarge or contract them at pleaſure, and the reflecting ſurface (as that of a fluid contained in a veſſel) was placed horizontally at O, from whence the light, coming through the hole P, was reflected to R, upon the ſcreen GH, where it was compared with another beam of light that fell upon S, through the hole Q; which he made ſo much leſs than P, as that the ſpaces S and R were equally illuminated; and by the proportion that the apertures P and Q bore to each other, he calculated what quantity of light was loſt by the reflexion at O.

It is neceſſary, he obſerves, that the two beams of light PO and QS (which he uſually made 7 or 8 feet long) ſhould be exactly parallel, that they might come from two points of the ſky equally elevated above the horizon, and having preciſely the ſame intenſity of light. It was alſo neceſſary that the hole Q ſhould be a little higher than P, in order that the two images ſhould be at the ſame heighth, and near one another. It is no leſs neceſſary, he ſays, that the ſcreen GH be exactly vertical, in order that the direct and reflected beams may fall upon it with the ſame inclination; ſince, otherwiſe, though the two lights were perfectly equal, they would not illuminate the ſcreen equally. This diſpoſition, he ſays, ſerves to anſwer another important condition in theſe experiments; for the direct ray QS muſt be of the ſame length with the ſum of the incident and reflected rays, PO and OR, in order that the quantity of light introduced into the room may be ſenſibly proportional to the ſizes of the apertures.*

I ſhall now proceed to recite the reſult of the experiments which he made to meaſure the quantity of light that is loſt by reflexion in a great variety of circumſtances; but I ſhall introduce them by the recital of ſome which were made previous to them on the diminution of light by reflexion, and the tranſmiſſion of it to conſiderable diſtances through the air, by M. Buffon, at the time that he was conſtructing his machine to burn at great diſtances mentioned above.

Receiving the light of the ſun in a dark place, and comparing it with the ſame light of the ſun reflected by a mirror, he found that, at ſmall diſtances, as four or five feet, about one half was

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[*] Traité d' Optique, p. 28.

loſt by reflexion; as he judged by throwing two reflected beams upon the ſame place, and comparing them with a beam of direct light; for then the intenſity of them both ſeemed to be the ſame.

Having received the light at greater diſtances as at 100, 200, and 300 feet, he could hardly perceive that it loſt any of its intenſity by being tranſmitted through ſuch a ſpace of air.*

He afterwards made the ſame experiments with candles, in the following manner. He placed himſelf oppoſite to a looking glaſs, with a book in his hand, in a room perfectly dark; and having one candle lighted in the next room, at the diſtance of about 40 feet, he had it brought nearer to him by degrees, till he was juſt able to diſtinguiſh the letters of the book, which was then twenty-four feet from the candle. He then received the light of the candle, reflected by the looking glaſs, upon his book, carefully excluding all the light that was reflected from any thing elſe; and he found that the diſtance of the book from the candle, including the diſtance from the book to the looking glaſs (which was only half a foot) was in all fifteen feet. He repeated the experiment ſeveral times, and always with nearly the ſame reſult; and therefore concluded, that the quantity of direct light is to that of reflected as 576 to 225, ſo that the light of five candles reflected from a plane mirror is about equal to that of two candles.

From theſe experiments it appeared that more light was loſt by reflexion of the candles than of the ſun, which M. Buffon thought was owing to this circumſtance, that the light iſſuing from the candle diverges, and therefore falls more obliquely upon on the mirror than the light of the ſun, the rays of which are nearly parallel.[*]

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* Ac. Par. 1747, M. p. 122.

Theſe experiments and obſervations of M. Buffon are curious, though, it will be ſeen, that they fall far ſhort of thoſe of M. Bouguer, both in extent and accuracy. I ſhall begin my account of this gentleman's experiments with thoſe which he made to aſcertain the difference in the quantity of light reflected by glaſs and poliſhed metal. Uſing a ſmooth piece of glaſs one line in thickneſs, he found that when it was placed at an angle of fifteen degrees with the incident rays, it reflected 628 parts of 1000 which fell upon it; at the ſame time that a metallic mirror, which he tried in the ſame circumſtances, reflected only 561 of them. At a leſs angle of incidence much more light was reflected, ſo that at an angle of three degrees the glaſs reflected 700 parts, and the metal ſomething leſs, as in the former caſe.[†]

Trying the reflexion of bodies that were *not poliſhed*, he found that a piece of white plaiſter, placed at an angle of 75°, with the incident rays, reflected $\frac{1}{119}$ part of the light it received from a candle, nine inches from it. White paper, in the ſame circumſtances, reflected in the ſame proportion; but at the diſtance of three inches, they both reflected 150 parts of 1000 that were incident.[§]

Proceeding to make farther obſervations on the ſubject of reflected light, he premiſes the two following theorems, which he demonſtrates geometrically, 1. When the luminous body is at an infinite diſtance, and its light is received by a globe, the ſurface of which has a perfect poliſh, and abſorbs no light, it reflects the light equally in all directions, provided it be received at a conſiderable diſtance. He only excepts the place where the ſhadow of the globe falls; but this, he ſays, is no more than a ſingle point, with reſpect to the immenſity of the ſpherical ſurface which receives its light.*

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[*] Ac. Par. 1747, M. p. 123.

[†] Bouguer's Traité d' Optique, p. 57.

[§] Ib. p. 69.

2. The quantity of light reflected in one certain direction will always be exactly the ſame, whether it be reflected by a very great number of ſmall poliſhed hemiſpheres, by a leſs number of larger hemiſpheres, or by a ſingle hemiſphere, provided they occupy the ſame baſe, or cover the ſame ground plan.†

The uſe he propoſes to make of theſe theorems is to aſſiſt him in diſtinguiſhing whether the light reflected from bodies be owing to the extinction of it within them, or whether the roughneſs or eminences which cover them have not the ſame effect with the ſmall poliſhed hemiſpheres above mentioned.

He begins with obſerving that, of the light reflected from mercury, ¼ at leaſt is loſt, and that probably no ſubſtances reflect more than this. The rays were received at an angle of 11 ½ degrees of incidence, that is meaſured from the ſurface of the reflecting body, and not from the perpendicular, which, he ſays, is what we are, from this place, to underſtand whenever he mentions the angle of incidence.§

The moſt ſtriking obſervations which he made with reſpect to this ſubject, are thoſe which relate to the very great difference in the quantity of light reflected at different angles of incidence, of which my reader will have been in ſome meaſure apprized, by the reſult of the introductory experiment I have already mentioned.

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* Traité d' Optique, p. 109. † Ib. p. 113. § Ib. p. 124.

tioned.* In general, he ſays, that reflexion is ſtronger at ſmall angles of incidence, and weaker at large ones. The difference is exceſſive when the rays ſtrike the ſurface of tranſparent ſubſtances, with different degrees of obliquity; but it is almoſt as great in ſome opaque ſubſtances, and it was always more or leſs ſo in every thing that he tried. He found the greateſt inequality in black marble; in which he was aſtoniſhed to find that, with an angle of 3° 35′ of incidence, though not perfectly poliſhed, yet it reflected almoſt as well as quick-ſilver. Of 1000 rays which it received, it returned 600; but when the angle of incidence was fifteen degrees, it reflected only 156; when it was 30, it reflected 51; and when it was 80, it reflected only 23.†

Similar experiments, made with metallic mirrors, always gave the differences much leſs conſiderable. The greateſt was hardly ever an 8th or a 9th part of it, but they were always in the ſame way.

The great difference between the quantity of light reflected from the ſurface of water, at different angles of incidence, is truly ſurpriſing; but our author obſerves, that this difference was greater, when the ſmalleſt inclinations were compared with thoſe which were near to a right angle. He ſometimes ſuſpected that, at very ſmall angles of incidence, the reflexion from water was even greater than from quick-ſilver. All things conſidered, he thought it was not quite ſo great, though it was very difficult to determine the preciſe difference between them. In very ſmall angles, he ſays, that water reflects nearly $\frac{3}{4}$ of the direct light.

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* M. Boſcovich obſerves, that the reaſon of the greater quantity of light being reflected at great degrees of obliquity, in its incidence upon any ſurface, is that, in thoſe circumſtances, the perpendicular velocity is leſs; ſo that the force which reſides at the ſurface can more eaſily, and with reſpect to more particles, deſtroy that velocity, and thereby determine the rays to be reflected. Theoria, p. 329.

† Traité d' Optique, 125.

There is no perſon, he ſays, but has ſometimes felt the force of this ſtrong reflexion from water, when he has been walking in ſtill weather, on the brink of a lake, oppoſite to the ſun. In this caſe, the reflected light is $\frac{1}{3}$, $\frac{1}{2}$, or ſometimes a greater proportion of the light that comes directly from the ſun, which is an addition to the direct rays of the ſun that cannot fail to be very ſenſible. The direct light of the ſun diminiſhes gradually, as it approaches the horizon, while the reflected light, at the ſame time, grows ſtronger: ſo that there is a certain elevation of the ſun, in which the united force of the direct and reflected light will be the greateſt poſſible, and this he ſays is 12 or 13 degrees.

On the other hand, the light reflected from water, at great angles of incidence, is extremely ſmall. Our author was aſſured that, when the light was perpendicular, it reflected no more than the 37th part that quick-ſilver does in the ſame circumſtances; for it did not appear, from all his obſervations, that water reflects more than the 60th, or rather the 55th part of perpendicular light. When the angle of incidence was 50 degrees, the light reflected from the ſurface of water was about the 32d part of that which mercury reflected; and as the reflexion from water increaſes with the diminution of the angle of incidence, it was twice as ſtrong in proportion at 39 degrees; for it was then the 16th part of the quantity that mercury reflected.*

In order to procure a common ſtandard, by which to meaſure the proportion of light reflected from various fluid ſubſtances, he pitched upon water as the moſt commodious, and partly by obſervation,

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* Theoria, p. 133.

ſervation, and partly by calculation, which he always found to agree with his obſervations, he drew up the following table, of the quantity of light reflected from the ſurface of water, at different angles with the ſurface.

Angles of incidence.	Rays reflected of 1000.	Angles of incidence.	Rays reflected of 1000.	Angles of incidence.	Rays reflected of 1000	Angles of incidence.	Rays reflected of 1000.
½	721	5	501	17½	178	50	22
1	692	7½	409	20	145	60	19
1½	669	10	333	25	97	70	18
2	639	12½	271	30	65	80	18
2½	614	15	211	40	34	90	18

In the ſame manner, he drew up the following table, of the quantity of light reflected from the looking glaſs not quick-ſilvered.*

Angles of incidence.	Rays reflected of 1000.	Angles of incidence.	Rays reflected of 1000.	Angles of incidence.	Rays reflected of 1000.
2½	584	15	299	50	34
5	543	20	222	60	27
7½	474	25	157	70	25
10	412	30	112	80	25
12½	356	40	57	90	25

Pouring a quantity of water into a veſſel containing quick-ſilver, it is evident that there will be two images of any objects ſeen by reflexion from them, one at the ſurface of the water, and the other at that of the quick-ſilver. In the largeſt angles of incidence, the image at the ſurface of the water will diſappear, which will happen when it is about a 60th, or an 80th part leſs luminous than the image at the ſurface of the quick-ſilver. Depreſſing the eye, the image on the water will grow ſtronger, and that on the quick-ſilver weaker, in proportion; till, at laſt, the latter will be incomparably weaker than the former, and at an angle of about 10 degrees they will be equally luminous.* According to the table, $\frac{333}{1000}$ of the incident rays are reflected from the water at this angle of 10 degrees. At the ſurface of the mercury they were reduced to 500; and of theſe, part being reflected back upon it from the under ſurface of the water, only 333 remained, to make the image from the mercury.

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* Theoria, p. 137.

It has been obſerved by ſeveral perſons, particularly by Mr. Edwards, ſee Phil. Tranſ. Vol. 53, p. 229; that there is a remarkably ſtrong reflexion into water, with reſpect to rays iſſuing from the water; and perſons under water have ſeen images of things in the water in a manner peculiarly diſtinct and beautiful; but this fact had not been obſerved with a ſufficient degree of attention, till it came into M. Bouguer's way to do it, and he acknowledges it to be very remarkable. In this caſe, he ſays, that from the ſmalleſt angles of incidence, to a certain number of degrees, the greateſt part of the rays are reflected, perhaps in as great a proportion as at the ſurface of metallic mirrors, or of quick-ſilver,† while the other part, which does not eſcape into the air, is extinguiſhed, or abſorbed; ſo that the ſurface of the tranſparent body appears opaque on the inſide. If the angle of incidence be increaſed only a few degrees, the ſtrong reflexion ceaſes altogether, a great number of rays eſcape into the air, and very few are abſorbed or extinguiſhed. In proportion as the angle of incidence is farther increaſed, the quantity of the light reflected becomes leſs and leſs; and when it is near 90 degrees, almoſt all the rays eſcape out of the tranſparent body, its ſurface loſing almoſt all its power of reflexion, and becoming almoſt as tranſparent as it is in other caſes, or when the light falls upon it from without.*

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* Theoria, p. 139.

† This muſt neceſſarily be the caſe where the ſine of the angle of incidence, out of water into air, bears a greater proportion to the radius than that of 3 to 4, which is the proportion of the ſine of the angle of incidence to the ſine of the angle of refraction out of water into air. Otherwiſe, the ſine of the angle of refraction, after the ray had paſſed out of the water into the air, would be greater than the radius, which is impoſſible. In this inſtance, therefore, all the rays muſt be reflected, except thoſe that are abſorbed.

This property belonging to the ſurfaces of tranſparent bodies, of abſorbing or extinguiſhing the rays of light, is truly remarkable, and as there is reaſon to believe, had not been noticed by any perſon before M. Bouguer. It has been already mentioned, as the conjecture of Sir Iſaac Newton, that rays of light become extinct only by impinging upon the ſolid parts of bodies; but theſe obſervations of M. Bouguer ſhew that the fact is quite otherwiſe, and that this effect is to be attributed not to the ſolid parts of bodies, which are certainly more numerous in a long tract of water than juſt in the paſſage out of water into air, but to ſome power lodged at the ſurfaces of bodies only, and therefore, probably, the ſame with that which reflects, refracts, and inflects the light.†

One of the above mentioned obſervations, viz. all the light being reflected at certain angles of incidence from air into denſer ſubſtances, had frequently been obſerved, eſpecially in glaſs priſms, ſo that Newton made uſe of one of them inſtead of a reflecting mirror, in the conſtruction of his teleſcope. If a beam of light fall upon the air from within theſe priſms, at an angle of 10, 20, or 30 degrees, the effect will be nearly the ſame as at the ſurface of quick-ſilver, a fourth or a third of the rays being extinguiſhed and $\frac{2}{3}$ or $\frac{3}{4}$ths reflected. This property retains its full force as far as an incidence of 49° 49′ (ſuppoſing the proportion of the ſines of refraction to be 31 and 20 for the mean refrangible rays) but if the angle of incidence be increaſed but one degree, the quantity of light reflected inwards decreaſes ſuddenly, and a great part of the rays eſcape out of the glaſs, ſo that the ſurface becomes ſuddenly tranſparent.

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* Theoria, p. 145.

† Additional proofs will be given of this property of the ſurfaces of bodies, to abſorb or extinguiſh light, in the next ſection, on the reflexion of tranſparent ſubſtances.

All tranſparent bodies have the ſame property, with this difference, that the angle of incidence at which the ſtrong reflexion ceaſes, and at which the light which is not reflected is extinguiſhed, is greater in ſome than in others. In water this angle is about 41° 32′; and in every medium it depends ſo much on the invariable proportion of the ſine of the angle of refraction, to the ſine of the angle of incidence, that this law alone is ſufficient to determine all the phenomena of this new circumſtance, at leaſt as to this accidental opacity of the ſurface.*

When our author proceeded to meaſure the quantity of light reflected by theſe internal ſurfaces at great angles of incidence, he found many difficulties, eſpecially on account of the many alterations which the light underwent before it came to his eye; but at length, uſing a plate of cryſtal,† he found that, at an angle of 75 degrees, this internal reflexion diminiſhed the light 27 or 28 times, and as the external reflexion at the ſame angle diminiſhed the light only 26 times, it follows that the internal reflxeion is a little ſtronger than the other.

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* Theora, p. 146.

† By cryſtal this writer means not rock cryſtal, but a particular kind of glaſs.

Repeating theſe experiments with the ſame, and different pieces of cryſtal, he ſometimes found the two reflexions to be equally ſtrong; but, in general, the internal was the ſtronger. Alſo, the image reflected internally was always a little redder than an object which was ſeen directly through the plate of cryſtal.*

Reſuming his obſervations on the diminution of light, occaſioned by the reflexion of opaque bodies, obliquely ſituated, he compared it with the appearances of ſimilar ſubſtances which reflected the light perpendicularly. Uſing pieces of ſilver made very white, he found that, when one of them was placed at an angle of 75 degrees with reſpect to the light, it reflected only 640 parts out of 1000. He then varied the angle, and alſo uſed white plaiſter and fine Dutch paper, and drew up the following table of the proportion of the light reflected from each of thoſe ſubſtances at certain angles.†

INTENSITY OF LIGHT reflected from

Angles of Incidence.	Silver.	Plaiſter.	Dutch Paper.
90	1000	1000	1000
75	802	762	971
60	640	640	743
45	455	529	507
30	319	352	332
15	209	194	203

Suppoſing the aſperities of opaque bodies to conſiſt of very ſmall planes, it appears from theſe obſervations, that there are fewer of them in theſe bodies which reflect the light at ſmall angles of incidence than at greater; and our author ſays, that the caſe was nearly the ſame with reſpect to all the opaque bodies that he tried. None of them had their roughneſs equivalent to ſmall hemiſpheres, which would have diſperſed the light equally in all directions; and, from the *data* in the preceding table, he deduces, mathematically, the number of the little planes that compoſe thoſe ſurfaces, and that are inclined to the general ſurface at the angles above mentioned, ſuppoſing that the whole ſurface contains 1000 of them that are parallel to itſelf, ſo as to reflect the light perpendicularly, when the luminous body is ſituated at right angles with reſpect to it. His concluſions reduced to a table, correſponding to the preceding, are as follows.*

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* Theoria, p. 155. Ib. p. 165.

Inclinations of the ſmall ſurfaces with reſpect to the large one.	The diſtribution of the ſmall planes that conſtitute the aſperities of the opaque ſurface in the		
	Silver.	Plaiſter.	Paper.
0	1000	1000	1000
15	777	736	937
30	554	554	545
45	333	374	358
60	161	176	166
75	53	50	52

Theſe variations in the number of little planes, or ſurfaces, he expreſſes in the form of a curve, and afterwards he ſhews, geometrically, what would be the effect, if the bodies were enlightened in one direction, and viewed in another; upon which ſubject he has ſeveral curious theorems and problems; as, the poſition of the eye being given, to find the angle at which the luminous body muſt be placed, in order to its reflecting the moſt light; [*] or, the ſituation of the luminous body being given, to find a proper ſituation for the eye, in order to ſee it the moſt enlightened, &c.[†] But it would carry me too far into the buſineſs of geometry to follow our author through all theſe diſquiſitions.

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* Theoria, p. 168.

Since the planets, as this accurate obſerver takes notice, are more luminous at their edges than at their centers, he concludes, from the above-mentioned principles, that the bodies which form them are conſtituted in a manner different from ours; particularly that their opaque ſurfaces conſiſt of ſmall planes, more of which are inclined to the general ſurface than they are in terreſtrial ſubſtances; [§] and that there are in them an infinity of points, which have exactly the ſame ſplendor. [‡]

Our philoſopher and geometrician next proceeds to aſcertain the quantity of ſurface occupied by the ſmall planes of each particular inclination, from conſidering the quantity of light reflected by each, allowing thoſe that have a greater inclination to the common ſurface to take up proportionably leſs ſpace than thoſe which are parallel to it. And comparing the quantity of light that would be reflected by ſmall planes thus diſpoſed, with the quantity of light that was actually reflected by the three ſubſtances above-mentioned, he found that plaiſter, notwithſtanding its extreme whiteneſs, abſorbs much light; for that, of 1000 rays that fall upon it, of which 166, or 167 ought to be reflected, at an angle of 77 degrees, only 67 are, in fact, returned; ſo that 100 out of 167 were extinguiſhed, that is about three fifths. [‖]

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[*] Theoria, p. 178. [†] Ib. p. 183. [§] Ib. p. 174. [‡] Ib. p. 195. [‖] Ib. p. 210.

With reſpect to the planets, our author concludes that of 300000 rays which the moon receives, 172000 are abſorbed or perhaps 204100.*

Having conſidered the ſurfaces of bodies as conſiſting of planes only, he thus explains himſelf. Each ſmall ſurface, ſeparately taken, is extremely irregular, and ſome of them are really concave, and others convex; but, in reducing them to a middle ſtate, they are to be conſidered as planes. Nevertheleſs he conſiders them as planes only with reſpect to the reception of the rays; for as they are almoſt all curves, and as, beſides this, many of thoſe, whoſe ſituation is different from others, contribute to the ſame effect, the rays always iſſue from an actual or imaginary focus, and after reflexion always diverge from one another.†

If it be aſked what becomes of thoſe rays that are reflected from one aſperity to another, he ſhews that very few of the rays can be in thoſe circumſtances; ſince they muſt fall upon planes which have more than 45 degrees of obliquity to the ſurface, of which there are very few in natural bodies. Theſe rays muſt, alſo, fall at the bottom of thoſe planes, and muſt meet with other planes ſimilarly ſituated to receive them; and conſidering the great irregularity of the ſurfaces of opaque bodies, it may be concluded, that very few of the rays are thus reflected upon the body itſelf, and that the little that is ſo reflected is, probably, loſt to the ſpectators, being extinguiſhed in the body.§

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* In comparing the light of the moon with that of the ſun, M. Bouguer, by a miſtake in the calculation, makes the quantity of light that comes from the ſun to that which is reflected from the moon (upon the ſuppoſition of the moon reflecting all the light which falls upon it) juſt double of what it ought to be, as will be ſhewn hereafter.

† Ib. p. 225. § Ib. p. 228.

CHAPTER III.

Observations on the reflexion of light within transparent substances.

THERE are no ſubſtances that we are acquainted with ſo perfectly tranſparent, but they contain within them particles which intercept and reflect the light, ſo that part of the beam which enters on one ſide is prevented from reaching the other. This is alſo a cauſe of the reflexion of light, to which M. Bouguer, with his uſual circumſpection, applied his meaſures; and the following are the principal obſervations which his treatiſe contains upon this ſubject.

The manner, in which he meaſured the loſs of light is repreſented in fig. 106, in which B is a tranſparent ſubſtance, upon which is placed the candle P, which enlightens at the ſame time, and almoſt perpendicularly, the two tablets D and E. The former of theſe is ſeen through the tranſparent ſubſtance on which the experiment is made, and the other with the naked eye, at A, and both at the ſame time. To bring the lights which come to the eye to an equality, it is neceſſary to remove the tablet E to a much greater diſtance than the other, and the ſquares of the two diſtances from the candle give the proportion required.

In this manner, tranſmitting the light through ſixteen pieces of common window glaſs, in all about 9½ lines in thickneſs, he found that it was diminiſhed 247 times. Trying ſix pieces of looking glaſs, in all $11\frac{1}{2}$ lines thick, the light was weakened in the proportion of 27 to 100; but a piece three inches thick not divided into plates, hardly diminiſhed the light one half.[*]

Getting the cleareſt ſea water that he could at Croiſic, where he reſided, he found that 115 inches of it tranſmitted $\frac{5}{14}$, or more accurately $\frac{64}{169}$ of the light that fell upon it; but he conjectures that near the torrid zone, where the water of the ſea is much clearer, ſo that the bottom may be ſeen at 100 or 120 feet deep, ten feet of it would not prevent the tranſmiſſion of more than $\frac{3}{4}$ or $\frac{7}{10}$ of the light.[†]

Our author cloſes his remarks on this ſubject with obſerving that, though we are much indebted to the tranſparency of the air, we derive no leſs benefit from its not being perfectly tranſparent; as, by this means, it reflects to us the light of the ſun, when his body is not viſible to us; and by the light reflected from it enables us to view objects on all ſides, and not on that only which is turned towards that luminous body.

Looking at an object through one piece of cryſtal, and at the ſame time through four other pieces, all of which put together were of a thickneſs equal to that of the one piece, and each of which was placed at an angle of 75 degrees, he not only found that leſs light was tranſmitted through the four pieces than through the one, but that even more light was loſt than the rules which he had already demonſtrated concerning the quantity reflected and tranſmitted would account for; ſo that he was obliged to have recourſe to another cauſe of the diminution of light; and upon the whole he concluded that $\frac{1}{12}$ part of the light was abſolutely abſorbed, at each of the interruptions of the cryſtal; for whereas the reflexions ought to have reduced the 1000 to 877, it was in fact reduced to 396. One cannot, he ſays, help attributing this diminution to the ſecond ſurface, which acts as if it was not perfectly tranſparent. It extinguiſhed at all moderate incidences (viz. from 0 to 49° 49′) the 3d or the 4th part of the rays. Beyond this inclination it abſorbed but a very ſmall part of the rays, though a few were loſt even when they fell perpendicularly.

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[*] Traité d' Optique, p. 60.

[†] Ib. p. 65.

He repeated the ſame obſervations upon other pieces of cryſtal, and though the reſults were not always the ſame, yet they were nearly ſo, and proved a real extinction of light. He ſometimes thought that the light which was thus abſorbed was about a 24th part, or equal to that which was reflected internally. He could not tell whether theſe differences were the effect of the different pieces of cryſtal, or were to be aſcribed to the errors that are unavoidable in theſe obſervations. Perhaps, ſays he, both may have contributed to them. Upon the whole, however, he preferred the laſt mentioned reſults. Thus, while he took a 36th part for the loſs occaſioned by the external reflexion at great angles of incidence, and the 27th or 28th part for the internal reflexion, he thought that the extinction of the rays at the ſame time was as great as in the laſt proportion.* Theſe obſervations are an additional proof of the power of ſurfaces, not diſcovered by any perſon before our author, of acting upon the rays of light in a manner different from reflexion, refraction, or inflexion, viz. a power of *extinguiſhing* the rays, which Newton had aſcribed to the ſolid parts of bodies only.

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* Traité d' Optique, p. 160.

In the ſecond ſection of our author's third book, we find ſome curious problems concerning the tranſparency of bodies; but as they contain no new optical diſcoveries, I ſhall content myſelf with referring the geometrical reader to the original, only quoting two examples of the laſt problem, which is to determine the thickneſs of any tranſparent medium, that will render it quite opaque; which is done by firſt finding what proportion of the light is ſtopped in traverſing a given ſpace in any medium, and thereby calculating the diſtance at which the light, being conſtantly diminiſhed in the ſame proportion, will be ſo weak as not to affect the eye.

Sea water, he obſerves, would be perfectly opaque at the thickneſs of 679 feet, and the air of our atmoſphere would ceaſe to be tranſparent, if the light had 518385 toiſes of it to traverſe. In this caſe, no light of the heavenly bodies could ever reach us; ſo that, notwithſtanding the ſplendor of the ſun, and the ſeeming thinneſs of the air, we ſhould for ever be involved in the blackeſt darkneſs, except ſo far as we ſhould be relieved by terreſtrial fires.

In the next place our author conſiders the diminution of light in paſſing through a medium varying in its denſity, with a view to the phenomena of the atmoſphere, which is ſuch a medium. In theſe obſervations he does not conſider the aſtronomical refraction, which, he ſays, is certainly too ſmall to make the ſines of incidence and refraction agreeable to the denſity of the air, and muſt, therefore, he ſays, as ſome have ſuſpected, depend upon a particular kind of matter diſperſed through the atmoſphere. At the cloſe of his calculations, he gives us the following table of the thickneſſes of the air contained in the atmoſphere,

phere, and the force which the light of the heavenly bodies retain after traverſing thoſe thickneſſes.

Apparent height of the heavenly bodies.	Thickneſſes of the air equivalent to that of our lower atmoſphere.	The quantity of light that reaches us of 10000 parts that fall upon the air.	Apparent height of the heavenly bodies.	Thickneſſes of the air equivalent to that of our lower atmoſphere.	The quantity of light that reaches us of 10000 parts that fall upon the air.
Degrees.	Toiſes.		Degrees.	Toiſes.	
90	3911	8123			
80	3971	8098	15	14880	4535
70	4162	8016	14	15880	4301
66° 11′	4275	7968	13	17012	4050
65	4315	7951	12	18344	3773
60	4516	7866			
55	4776	7759	11	19908	3472
50	5104	7624	10	21745	3149
45	5530	7454	9	23975	2797
40	6086	7237	8	26672	2423
35	6813	6963	7	29996	2031
30	7784	6613	6	34300	1616
25	9191	6136	5	39893	1201
20	11341	5474	4	47480	802
19° 16′	11744	5358	3	58182	454
19	11890	5316	2	74429	192
18	12515	5143	1	100930	47
17	13220	4954	0	138823	6
16	14000	4753			

It muſt be obſerved that this table is nearly the ſame with that which was publiſhed in the firſt ſketch of our author's work in 1729, and was not calculated over again, after ſome new lights, which he had acquired upon the ſubject. The difference, however, would not have been very great.

In our author's obſervations on the light reflected by the interior parts of tranſparent bodies, he took a piece of cryſtal, which, being pretty thick, diminiſhed the light in the proportion of nine to four. Then having placed two tablets of plaiſter near one another, he looked at them through this medium, but in places of it ſo diſtant from one another, that he could ſee one of them ſtrongly enlightened, while the other was in perfect darkneſs. This circumſtance made a conſiderable difference in the diſtinctneſs of the viſion. The obſcure part of the piece of cryſtal was ſenſibly more tranſparent; whereas the light of the other half, far from uniting with that of the object, in part effaced it, and what he thought would have been an addition of light to the tablet placed at a diſtance, became, on the contrary, a diminution of it; and the more perfectly he ſaw the inſide of the tranſparent cryſtal, the leſs clearly could he perceive the object that was placed beyond it. The light which fell upon one half of the cryſtal was about 31 times ſtronger than that which enlightened the two tablets of plaiſter; and to make theſe two objects appear equally enlightened, he was obliged to increaſe the light of that which was ſeen through the enlightened half of the cryſtal about one ſeventh part.

From this he calculated that the ſmall particles of the cryſtal, inſtead of reflecting all the rays which they intercept, reflect only the 235th part. It is true, however, he ſays, that we cannot be abſolutely certain but that a greater number of reflected rays may fall upon other parts of the tranſparent body, and be thence reflected to the eye; but the effect of theſe ſecondary reflexions muſt be very weak, as well by the loſs of rays at each place of reflexion, as by the greater diſperſion of the rays which take different directions. He is, therefore, content with ſtating the fact, and with concluding that the internal parts of the cryſtal, and, probably, thoſe of all other tranſparent mediums, reflect but a very ſmall part of the light which they ſtop in its paſſage.*

In making an eſtimate of the light that is reflected by the internal parts of a medium, when it is not of the ſame denſity throughout, and enlightened with different degrees of light, he ſuppoſes the ſun to be in the horizon, and he ſhews that the light reflected by the whole atmoſphere will be $\frac{784}{10000}$, while unity expreſſes the ſtrength of the ſun's light reaching us in that ſituation. And, not ſatisfying himſelf with calculating the light reflected by the whole atmoſphere, he alſo calculates it for different thickneſſes of air, and has expreſſed the reſults in the following table, in which the diſtances are expreſſed in great leagues of 20 to a degree.

Diſtances in leagues.	Thickneſſes of air, in toiſes of an uniform denſity.	Intenſity of the light reflected from the air.	Diſtances in leagues.	Thickneſſes of air, in toiſes of an uniform denſity.	Intenſity of the light reflected from the air.
$2\frac{1}{2}$	7115	$\frac{778}{10000}q$	30	79307	$\frac{2525}{10000}q$
5	14231	$\frac{1289}{10000}q$	45	109099	$\frac{2565}{10000}q$
10	28257	$\frac{1967}{10000}q$	60	130548	$\frac{2572}{10000}q$
15	41931	$\frac{2268}{10000}q$	90	153128	$\frac{2575}{10000}q$
20	55096	$\frac{2410}{10000}q$	180	169518	$\frac{2175}{10000}q$

N. B. *q*. Signifies the number of times that the light is weakened by the reflexion of each particle of the medium.†

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[*] Traité d' Optique. p. 346.

[†] There is ſomething rather obſcure in this article, but I have given the beſt account of it that I could, without entering too far into our author's long and intricate calculations on the ſubject. The reſult was too curious and important not to be recited, and this was neceſſary in order to introduce it.

It is ſufficient, he ſays, to caſt our eyes upon the numbers of this ſmall table, to ſee the reaſon of the two different manners in which we loſe ſight of large terreſtrial objects when they are very diſtant from us. If a very large mountain be covered with wood, and be 29 or 30 leagues from us, it will ſend but very few rays of light to our eye; for, beſides that the green of foreſts reflect a very faint light, the leaves of the trees are mixed with ſhadows, which reflect no light at all. But if the atmoſphere be very ſerene, the mountain will be illuminated by all the aerial light, reflected by the whole maſs of air, that lies between the object and the eye of the obſerver; and this colour will be expreſſed by $\frac{2525}{10000}q$, while that of all the atmoſphere, or that of the heavens, which can be ſeen at the ſide of it, will be expreſſed by $\frac{2575}{10000}q$; and though the difference be no more than a 51ſt part, it will, in general, be ſtrong enough to be perceived, provided a perſon be attentive to it. If the mountain were ſtill nearer, there would be a greater difference between the ſhades of the aerial colour, that is, the light reflected from the air, and the object, always preſerving the ſame ſhape, would appear like a thicker cloud, and its colour would appear a little deeper, with reſpect to that of the heavens; whereas, when it is juſt about to diſappear, as in the caſe mentioned above, it will aſſume the appearance of a very light and tranſparent cloud; or, which is the ſame thing, the azure of the ſky will ſeem to be covered in the place of the mountain with a light gauze.

If the mountain was only 18000 or 20000 toiſes diſtant, ſomething of its green colour might be ſeen. Its light would then be weakened, in the proportion of 1000 to 346, through a defect of tranſparency of the air; and it is this defect of tranſparency, which, together with the aerial colour, leſſens the ſplendor of objects. At the ſame time that the mountain would reflect a greater number of rays, the aerial colour would be weaker, and the two lights would reciprocally leſſen one another.

Thoſe objects which are very near diſappearing, on account of their diſtance, are ſeen, as one may ſay, in a *negative* manner, but others, though more diſtant, are ſeen in a *poſitive* manner, when the light which they ſend to us is ſtronger than that which comes from the neighbouring ſky. If a dark mountain be 45 or 50 leagues off, the azure colour with which it will be enlightened will be expreſſed by $\frac{2565}{10000}q$, or $\frac{2567}{10000}q$, which differs ſo little from $\frac{2575}{10000}q$, (which is the aerial colour of the ſky) that the difference would be abſolutely inſenſible. But if the mountain be covered with white ſand, or ſnow, it may be very viſible. Some perſons pretend that very large objects may be ſeen at a greater diſtance, but our author queſtions the fact, and chuſes to fix this limit at 45 leagues; becauſe at that diſtance from the gulf of Guayaquil, Chimboraco, one of the higheſt mountains in Peru, and perhaps in the world, and which has every advantage to make it conſpicuous could but juſt be ſeen. Upon the whole, he calculates, that the particles of the air reflect only the 363d part of the rays which they intercept; and he thinks that there is reaſon to ſuppoſe that they reflect ſtill leſs, which ought to make an object more viſible, when, as in the caſe of this mountain, it is ſeen by its own proper light. He acknowledges, however, that this calculation is ſubject to many errors, and adviſes thoſe philoſophers who would meaſure the intenſity of the aerial colours to compare them immediately with the light of the moon. He alſo points out ſome methods in which it might be compared with the light of the ſun, and concludes with ſeveral obſervations, that may be of uſe for that purpoſe. *

It muſt be obſerved that the diſtance at which objects can be ſeen muſt depend both upon the intenſity of their light, and the goodneſs of the eye, as well as the tranſparency of the interpoſed medium. The following facts may furniſh ſome light on this ſubject.

The inhabitants of Noeſſa, one of the Molucca Iſlands, in a clear day, can ſee a candle burning in the gardens of the Iſland Magindanao, which is twenty three miles diſtant.

M. Muſchenbroeck knew a perſon, he ſays, who could with his naked eyes, diſtinguiſh the ſatellites of Jupiter, as well as he himſelf could ſee them, at the ſame time, with a teleſcope of twelve feet; and he quotes Cicero for an inſtance of a perſon, who could ſee objects at the diſtance of [redacted] 1800 ſtadia, and Pliny, who gives an account of a perſon, who, in the ſecond Punic war, told the number of the Carthagenian fleet coming out of their harbour, when he was at Lilibæum in Sicily. But this ſtory is certainly incredible, if not impoſſible, on account of the curvative of the earth.†

A very great variety of experiments on the power of differently *coloured* mediums to intercept the light was made by M. Muſchenbroeck, in which it appears, that bodies very tranſparent and thin, yet if they be of various colours, often effectually prevent the tranſmiſſion of light. When he took pieces of glaſs, of each of the ſeven colours, though all of them put together were not half an inch thick; yet no appearance of the ſun could be ſeen through them. To judge of the ſeparate effect of each of the coloured pieces, he uſed them in a great variety of combinations, the reſult of which, with reſpect both to the quantity and the colour of the tranſmitted light, may be ſeen in his *Introductio ad Philoſophiam Naturalem, Vol.* 2, *p.* 800. From the whole he concluded, that the red rays of the ſun were in great quantity in a white beam of light, that they were eaſily tranſmitted through the glaſſes that were red, orange, and yellow, but in a ſmall quantity through that which was green, ſo that the green, the blue, the purple, and violet rays were ſeparated from the white beam of light when it was intercepted by the red glaſs. It appeared, therefore, that when the red rays had been tranſmitted through pieces of red, orange, and yellow glaſs, there remained but few rays that could paſs through the piece of green glaſs laid behind them. For no green rays would arrive there, nor were there any blue rays to arrive at the piece of blue glaſs, placed behind them; ſo that all the pieces put together made an opaque maſs.

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* Traité d' Optique, p. 365.

† Introductio, Vol. 2, p. 773.

In making theſe experiments, he could not find what became of the red light, which, after having paſſed through the red, orange, and yellow glaſs, was intercepted by the green; but he ſeemed to imagine, that it might either be reflected back again into the red glaſs, or be diſperſed in all directions within the green.*

The ſame philoſopher alſo made obſervations on the appearance of the ſun through various numbers of plates of the ſame colour, and found that when a beam of light was tranſmitted through a number of blue glaſſes, objects placed in the tranſmitted light appeared of a deeper and deeper blue, till they became purple. Through five of the blue plates of glaſs the ſun appeared white, but when he uſed ſix, it began to appear of a purple colour, which grew deeper by the addition of every plate, till with fifteen of thoſe plates, which were an inch in thickneſs, nothing of the ſun could be ſeen.*

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* Introductio, Vol. 2, p, 801.

In the courſe of this, and the preceding ſection, it has appeared that, according to the obſervations of M. Bouguer, light is abſorbed, or extinguiſhed, in conſequence of its falling with a certain angle of incidence on the ſurfaces of bodies, and not by impinging on their ſolid parts, as Newton ſuppoſed, and that when light is tranſmitted through ſeveral tranſparent ſubſtances, the thickneſs of them has little effect in compariſon with the number of ſurfaces. But ſome experiments which Mr. Canton has lately made with his artificial phoſphorus ſeem to ſhow that, in ſome caſes, the tranſmiſſion of light is very conſiderably impeded by the thickneſs of the moſt tranſparent mediums.

He took two pieces of the fineſt flint glaſs, equally tranſparent, and having their ſides ground truly parallel, but one of them ten times as thick as the other. Theſe he placed in ſuch a manner, that the light of an electrical exploſion might paſs through them, and fall upon pieces of his phoſphorus; and notwithſtanding there were no more than two ſurfaces in each caſe, and the difference of the loſs of light in the intervals between them muſt, according to M. Bouguer's experiments, have been very inconſiderable, there was a very great and manifeſt difference in the light tranſmitted through them; the phoſphorus not being near ſo ſtrongly illuminated by the light of exactly equal exploſions through the thick glaſs, as through the thin one.

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* Introductio, Vol. 2, p. 801.

I have myſelf repeated theſe experiments with a good deal of attention, and have found that the phoſphorus is more ſtrongly illuminated through ſeven pieces of very thin glaſs, than through one piece only, equally tranſparent, that was a quarter of an inch thick, though there were ſeveral circumſtances that favoured the tranſmiſſion of the light through the thick glaſs more than through the thin ones; ſo that I have no doubt but that, in this manner, more light might be tranſmitted through twelve or twenty pieces of very thin glaſs than through one of a quarter of an inch thick.

It is remarkable, however, that when the phoſphorus is illuminated by the light of the ſun through theſe glaſſes, no ſuch difference can be perceived, the phoſphorus being, to appearance, as much illuminated (by a beam of the ſun in a darkened room) through the thick glaſs, as through a thin one; though, when I made the phoſphorus paſs as quickly as I poſſibly could through the beam of light, I imagined that more went through the thin glaſs than the thick one, but if they remained in the light for the leaſt ſenſible ſpace of time, even leſs than a quarter of a ſecond, the difference vaniſhed.

It is to be obſerved that the emiſſion of light in an electrical exploſion is *momentary*, and that it is hardly poſſible to paſs the phoſphorus through the ſun beam ſo quick, but that, with reſpect to the electrical exploſion, it may be ſaid to be a conſiderable time expoſed to the light, and that the particles of light act upon it in *ſucceſſion*. But why this circumſtance ſhould make ſo great a difference in this caſe, I own, I am not able to explain. The fact, however, is very remarkable, and well deſerves to be examined with attention.

CHAPTER IV.

OBSERVATIONS ON THE BLUE SHADOWS OF BODIES, THE BLUE COLOUR OF THE SKY, AND THE RED COLOUR OF THE MORNING AND EVENING CLOUDS.

A VARIETY of natural phenomena demonſtrate that a great proportion of the fainter coloured rays are ſtopped in their paſſage through the atmoſphere, and thence reflected upon other bodies, while the red and the orange coloured rays are tranſmitted to greater diſtances. This circumſtance explains the obſervations that have been made on the blue ſhadows of bodies, the blue colour of the ſky, and the red colour of the clouds when the ſun is near the horizon. The two laſt are facts that have eſcaped the notice of no perſon, but the other was firſt obſerved by the famous Otto Guericke, and his obſervation being overlooked, it was again noticed by M. Buffon. As the fact is exceedingly curious, and was afterwards ingeniouſly diverſified and completely explained by later philoſophers, I ſhall give the ſubject rather more room than it might otherwiſe deſerve.

The opinions of early writers on the ſubject of the blue colour of the ſky are hardly worth reciting. Fromondus thought that it aroſe from a mixture of light, and the black appearance of the ſpace beyond the atmoſphere. Faber, who was much diſſatisfied with the opinion of Fromondus, maintained that it ariſes from the reflexion of the light by particles floating in the air; and Funccius, who wrote an intire treatiſe on the ſubject, again ſupported an opinion not much different from that of Fromondus, aſſerting that the blue colour of the ſky is a mixture of much ſhade and a little light. The opinion of Fromondus prevailed almoſt unanimouſly till the preſent period, and has had many advocates even in modern times, eſpecially the celebrated Wolf, and Muſchenbroek. Dr. Eberhard, who recites theſe opinions, thinks that the air has a proper colour, in conſequence of *refracting* the blue rays more than others. * But this by no means accounts for the ſeparation of all the blue from all the red rays to an eye placed within the atmoſphere. Beſides, the *reflexion* of the blue rays, and the tranſmiſſion of the red ones, which was advanced by M. Bouguer and others, before Dr. Eberhard wrote, gives an eaſy ſolution of the phenomenon.

It was the opinion of Otto Guericke, as well as of all his cotemporaries, that the blue colour of the ſky is nothing but a compound of light and ſhade, or white and black, and he aſſerts that ſuch a mixture will make a blue. As a proof of this, he ſays, that if, in the morning, the light of a candle be ſhaded, ſo that it cannot but fall upon a piece of white paper, the ſhadow will be perfectly blue, and not black.† One would think that if he had actually mixed white and black powders, he could not but have been ſenſible that the reſult was not blue, and therefore that the blue in this caſe muſt have proceeded from ſome other cauſe. But Otto Guericke is not the only great man who have overlooked ed the moſt obvious facts and obſervations, when they have not been favourable to a particular hypotheſis. We ſhall not wonder at a philoſopher of ſo early an age not underſtanding this phenomenon, when we find both M. Buffon and M. Mazeas, who gave a good deal of attention to it, were ignorant of the cauſe of it. It is rather remarkable, however, that ſo curious an appearance as this of *blue ſhadows* ſhould paſs unnoticed near a century, and ſhould then be hit upon by mere accident, as at the firſt. For it was not till the month of July 1742, that M. Buffon, as he was buſy upon the ſubject of *accidental colours*, of which an account will be given in its proper place (when he was waiting to ſee the ſun near the horizon, that he might look upon it without any injury to his eye, and might thereby be enabled to obſerve the colours, and the changes of colour made by the impreſſion of light upon his eye,) obſerved that the ſhadows of trees which fell upon a white wall were green. He was at that time ſtanding upon an eminence, and the ſun was ſetting in the cleft of a mountain, ſo that he appeared conſiderably lower than the horizon. The ſky was clear, excepting in the Weſt, which, though free from clouds, was lightly ſhaded with vapours, of a yellow colour, inclining to red. The ſun itſelf was exceedingly red, and was ſeemingly, at leaſt, four times as large as he appears to be at mid-day. In theſe circumſtances he ſaw very diſtinctly the ſhadows of the trees, which were 30 or 40 feet from the white wall, coloured with a light green, inclining to blue. The ſhadow of an arbour, which was three feet from the wall, was exactly drawn upon it, and looked as if it had been newly painted with verdegris. This appearance laſted near five minutes, after which it grew fainter, and vaniſhed at the ſame time with the light of the ſun.

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* Acta Nova. Cæſ. Tom. 2. App. p. 263.

† Experimenta Magdeburgica, p. 142.

The next morning, at ſun riſe, he went to obſerve other ſhadows, upon another white wall; but inſtead of finding them *green*, as he expected, he obſerved that they were *blue*, or rather of the colour of lively indigo. The ſky was ſerene, except a ſlight covering of yellowiſh vapours in the Eaſt, and the ſun aroſe behind a hill, ſo that it was elevated above his horizon. In theſe circumſtances the blue ſhadows were only viſible three minutes; after which they appeared black, and in the evening of the ſame day he obſerved the green ſhadows exactly as before. Six days paſſed without his being able to repeat his obſervations, on account of clouds; but the 7th day, at ſun ſet, the ſhadows were not green, but of a beautiful ſky blue. He alſo obſerved, that the ſky was, in a great meaſure, free from vapours at that time, and that the ſun ſet behind a rock, ſo that it diſappeared before it came to his horizon. Afterwards he often obſerved the ſhadows both at ſun riſe and ſun ſet, but always obſerved them to be blue, though with a great variety of ſhades of that colour. He ſhewed this phenomenon to many of his friends who were as much ſurpriſed at it as he himſelf had been, but he ſays that any perſon may ſee a blue ſhadow, if he will only hold his finger before a piece of white paper at ſun riſe or ſun ſet.*

The firſt perſon who attempted to explain this phenomenon was the Abbé Mazeas, in a memoir of the ſociety in Berlin for the year 1752. He obſerved that when an opaque body was illuminated by the moon, and a candle at the ſame time, and the two ſhadows were caſt upon the ſame white wall, that which was enlightened by the candle was reddiſh, and that which was enlightened by the moon was blue. But, without attending to any other circumſtances, he ſuppoſed the change of colour to be occaſioned by the diminution of the light; but M. Melville, and M. Bouguer, both independent of one another, ſeem to have hit upon the true cauſe of this curious appearance, and which I have already hinted at. The former of theſe gentlemen, in his attempts to explain the blue colour of the ſky, obſerves, that, ſince it is certain that no body aſſumes any particular colour, but becauſe it reflects one ſort of rays more abundantly than the reſt; and ſince it cannot be ſuppoſed that the conſtituent parts of pure air are groſs enough to ſeparate any colours of themſelves, we muſt conclude with Sir Iſaac Newton, that the violet and blue making rays are reflected more copiouſly than the reſt, by the finer vapours diffuſed through the atmoſphere, whoſe parts are not big enough to give them the appearance of viſible opaque clouds. And without referring to Otto Guericke, or M. Buffon, and probably without knowing what had been obſerved by them, he ſhews that, in proper circumſtances, the bluiſh colour of the ſky light may be actually ſeen on bodies illuminated by it, as, he ſays, it is objected ſhould always happen upon this hypotheſis. For that if, on a clear cloudleſs day, a ſheet of white paper be expoſed to the ſun's beams, when any opaque body is placed upon it, the ſhadow which is illuminated by the ſky only will appear remarkably bluiſh compared with the reſt of the paper, which receives the ſun's direct rays.*

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* Ac. Par. 1743, M. p. 217.

M. Bouguer had no recourſe to the vapours diffuſed through the atmoſphere to account for the reflexion of the blue making rays; but ſeems rather to ſuppoſe, that it ariſes from the conſtitution of the air itſelf, whereby thoſe fainter coloured rays are

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[*] Edinb. Eſſays, Vol. 2, p. 75.

incapable of making their way through any conſiderable tract of it. He, therefore, ſays that the colour of the air is properly blue, though probably mixed with other rays that are eaſily reflected; and that it furniſhes an explanation of a remarkable phenomenon, which had not eſcaped the notice of painters, and occaſioned a memoir of M. Buffon; but of which no perſon that he knew of had aſſigned the true reaſon. The morning and the evening ſhadows, he ſays, are of a lively blue, and a candle produces nearly the ſame effect. This phenomenon, he obſerves, is cauſed by the aerial colour of the atmoſphere, which enlightens theſe ſhadows, and in which the blue rays prevail, while the red rays are not reflected ſo ſoon, but paſs on to the remoter regions of the atmoſphere.*

I muſt alſo add that Dr. Smith obſerves that the air chiefly affects the blue making rays, and accounts for the red appearance of the ſun and moon near the horizon upon this principle.†

M. Beguelin, who has taken the moſt pains with this ſubject, obſerves that, as M. Buffon mentions the ſhadows appearing green only twice, and that at all other times they are blue, this is the colour which they regularly have, and that the blue was changed into green by ſome accidental circumſtance. Green, he ſays, is only a compoſition of blue and yellow, ſo that this accidental change may have ariſen from the mixture of ſome yellow rays in the blue ſhadow; and that perhaps the wall might have had that tinge, ſo that the blue is the only colour for which a general reaſon is required. And this he ſays, muſt be derived from the colour of pure air, which always appears blue, and which always reflects that colour, upon all objects without diſtinction; but which is too faint to be perceived when our eyes are ſtrongly affected by the light of the ſun, reflected from other objects around us.

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* Traité d' Optique, p. 368.

† Opticks. Remarks, p. 63.

To confirm this hypotheſis, he adds ſome curious obſervations of his own, in which this appearance is agreeably diverſified, and for the ſake of which I have introduced his remarks. Being at the village of Boucholtz in July 1764, he obſerved the ſhadows projected on the white paper of his pocket book, when the ſky was clear. At half an hour paſt ſix in the evening, when the ſun was about four degrees high, he obſerved that the ſhadow of his finger was of a dark grey, while he held the paper oppoſite to the ſun; but when he inclined it almoſt horizontally, the paper had a bluiſh caſt, and the ſhadow upon it was of beautiful bright blue.

When his eye was placed between the ſun and the paper laid horizontally, it always appeared of a bluiſh caſt; but when he held the paper, thus inclined, between his eye and the ſun, he could diſtinguiſh, upon every little eminence occaſioned by the inequality of the ſurface of the paper, the principal of the priſmatick colours. He alſo perceived them upon his nails, and upon the ſkin of his hand. This multitude of coloured points red, yellow, green, and blue, almoſt effaced the natural colour of the objects.

At three quarters paſt ſix, the ſhadows began to be blue, even when the rays of the ſun fell perpendicularly. The colour was the moſt lively when the rays fell upon it at an angle of forty-five degrees, but, with a leſs inclination of the paper, he could diſtinctly perceive that the blue ſhadow had a border of a ſtronger blue, on that ſide which looked towards the ſky, and a red border on that ſide which was turned towards the earth. To ſee theſe borders, the body that made the ſhadow was obliged to be placed very near the paper; and the nearer it was, the more ſenſible was the red border. At the diſtance of three inches, the whole ſhadow was blue. At every obſervation, after having held the paper towards the ſky, he turned it towards the earth, which was covered with verdure; holding it in ſuch a manner, that the ſun might ſhine upon it while it received the ſhadows of various bodies, but, in this poſition, he could never perceive the ſhadow to be blue or green at any inclination with reſpect to the ſun's rays.

At ſeven o'clock, the ſun being ſtill about two degrees high, the ſhadows were of a bright blue, even when the rays fell perpendicularly upon the paper, but were the brighteſt when it was inclined at an angle of forty-five degrees, At this time he was ſurpriſed to obſerve that a large tract of ſky was not favourable to this blue colour, and that the ſhadow falling upon the paper placed horizontally was not coloured, or, at leaſt, the blue was very faint. This ſingularity he concluded aroſe from the ſmall difference between the light of that part of the paper which received the rays of the ſun, and that which was in the ſhade in this ſituation. In a ſituation preciſely horizontal, the difference would vaniſh, and there could be no ſhadow. Thus too much or too little of the ſun's light produced, but for different reaſons, the ſame effect; for they both made the blue light reflected from the ſky to become inſenſible. This gentleman never ſaw any green ſhadows, but when he made them fall on yellow paper. But he does not abſolutely ſay that green ſhadows cannot be produced in any other manner, and ſuppoſes that if it was on the ſame wall that M. Buffon ſaw the blue ſhadows, ſeven days after having ſeen the green ones, he thinks that the cauſe of it might be the mixture of yellow rays, reflected from the vapours, which he obſerves were of that colour.

Theſe blue ſhadows, our author obſerves, are not confined to the times of ſun riſing and ſun ſetting; for on the nineteenth of July, when the ſun has the greateſt force, he obſerved them at three o'clock in the afternoon, but the ſun ſhone through a miſt at that time.

If the ſky is clear, the ſhadows begin to be blue, when if they be projected horizontally, they are eight times as long as the height of the body that produces them, that is, when the center of the ſun is 7° 8′ above the horizon. This obſervation he ſays was made in the beginning of Auguſt.

Beſides theſe coloured ſhadows, which are produced by the interception of the direct rays of the ſun, our author obſerved others ſimilar to them at every hour of the day, in rooms into which the light of the ſun was reflected from ſome white body, if any part of the clear ſky could be ſeen from the place, and all unneceſſary light was excluded as much as poſſible. Obſerving theſe precautions, he ſays that the blue ſhadows may be ſeen at any hour of the day, even with the direct light of the ſun, and that this colour will diſappear in all thoſe places of the ſhadow from which the blue ſky cannot be ſeen.

All the obſervations that our author made upon the yellow or reddiſh borders of ſhadows, above mentioned, led him to conclude that they were occaſioned by the interception of the ſky light, whereby part of the ſhadow was illuminated either by the red rays reflected from the clouds, when the ſun is near the horizon, or from ſome terreſtrial bodies in the neighbourhood.

This conjecture is favoured by the neceſſity he was under of placing any body near the paper, in order to produce this bordered ſhadow, as he ſays, it is eaſily demonſtrated, that the interception of the ſky light can only take place when the breadth of the opaque body is to its diſtance from the white ground on which the ſhadow falls, as twice the ſine of half the amplitude of the ſky to its coſine.

At the concluſion of his obſervations on theſe blue ſhadows, he gives a ſhort account of another kind of them, which, he does not doubt, have the ſame origin. Theſe he often ſaw early in the ſpring when he was reading by the light of a candle in the morning, and conſequently the twilight mixed with that of his candle. In theſe circumſtances the ſhadow that was made by intercepting the light of his candle, at the diſtance of about ſix feet, was of a beautiful and clear blue, which became deeper as the opaque body which made the ſhadow was brought nearer to the wall, and was exceedingly deep at the diſtance of a few inches only. But wherever the day light did not come, the ſhadows were all black, without the leaſt mixture of blue.*

Mr. Melville objects to the opinion of Sir Iſaac Newton, and the generality of philoſophers, concerning the cauſe of the various *colours reflected by the clouds*, and gives an explication of this phenomenon agreeably to his obſervations concerning the blue ſhadows; theſe being cauſed by the blue coloured rays reflected from the atmoſphere, and the other by the red and orange, which alone can reach the clouds after the ſun beams have been deprived of the blue and fainter colours, in their paſſage through a long tract of air.

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* Ac. Berlin, 1767, p. 27.

It was the opinion of Sir Iſaac Newton, that the different colour of the clouds depends upon the different ſize of the globules, into which the vapours are condenſed, and in ſo tranſparent a ſubſtance as water, he owns, that he can ſee no other reaſon for the production of theſe colours.* But, ſays Mr. Melville, why ſhould the particles of the clouds become at that particular time, and never at any other, of ſuch a magnitude as to ſeparate theſe colours? And why are they rarely, if ever, ſeen tinctured with blue and green, as well as red, orange, or yellow? Is it not more credible, that the ſeparation of rays is made in paſſing through the horizontal atmoſphere, and that the clouds only reflect and tranſmit the ſun's light, as any half tranſparent colourleſs body would do in their place? For ſince the atmoſphere reflects a greater quantity of blue and violet rays than of the reſt, the ſun's light tranſmitted through it ought to incline towards yellow, orange, or red; eſpecially when it paſſes through the greateſt tract of air. Accordingly, every one muſt have remarked, that the ſuns horizontal light is ſometimes ſo deeply tinctured, that objects directly illuminated by it appear of a high orange, or even red. At that inſtant is it any wonder, ſays he, that the colourleſs clouds reflect the ſame rays in a more bright and lively manner?

It is obſervable, ſays our author, that the clouds do not commonly aſſume their brighter dies till the ſun is ſome minutes ſet, and that they paſs from yellow to a flaming gold colour, and thence, by degrees, to red, which turns deeper and deeper, though fainter, till the ſun leaves them altogether. Now it is plain that the clouds at that time receive the ſun's light through a much longer tract of air than they do at the inſtant of his ſetting, perhaps by the difference of a 100 miles, or more, as may be computed from their height, or the duration of their colours. Is it not, therefore, natural to imagine, that, as the ſuns light becomes always ſomewhat yellowiſh, or orange, in paſſing through the depth of the atmoſphere horizontally, it ought to incline more and more from orange towards red, by paſſing through a ſtill greater tract of air; ſo that the clouds, according to their different altitudes, may aſſume all the variety of colours at ſun riſing and ſetting, by barely reflecting the ſun's incident light as they receive it?

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* Opticks, p. 228.

Our author was confirmed in this opinion by frequently obſerving, when he was in Switzerland, that the ſnowy ſummits of the alps turned more and more reddiſh after ſun ſet, in the ſame manner as the clouds, and he obſerves that what makes the ſame colours much more rich and copious in the clouds is their ſemi-tranſparency, joined with the obliquity of their ſituation.

Does it not greatly confirm this explication, he ſays, that theſe coloured clouds immediately reſume that dark leaden hue, which they receive from the ſky, as ſoon as the ſun's direct rays ceaſe to ſtrike upon them? For if their gaudy colours aroſe, like thoſe of the ſoap bubble, from the particular ſize of their parts, they then would preſerve nearly the ſame colours, though much fainter, when illuminated only by the atmoſphere. About the time of ſun ſet, or a little after, the lower part of the ſky, to ſome diſtance, on each ſide from the place of his ſetting, ſeems to incline to a faint ſea green, by the mixture of his tranſmitted beams, which are then yellowiſh, with the ætherial blue. At greater diſtances, this faint green gradually changes into a reddiſh brown, becauſe the ſun's rays, by paſſing through more air, begin to incline to orange. At the ſame time, on the oppoſite ſide of the hemiſphere, the colour of the horizontal ſky inclines ſenſibly to purple, becauſe the tranſmitted light, which mixes with the azure, by paſſing through a ſtill greater length of air, becomes reddiſh.

To underſtand diſtinctly why the ſun's rays, by paſſing through a greater and greater quantity of air, change, by degrees, from white to yellow, from thence to orange, and laſtly to red, our author obſerves, that we have only to apply to the atmoſphere what Sir Iſaac ſays concerning the colour of tranſparent liquors in general, and I would add that of the infuſion of lignum nephriticum in particular.

Our author concludes theſe judicious obſervations, with a conjecture, that it is the ſame coloured light, of the riſing and ſetting ſun, which tinctures the clouds, that, being thrown by the refraction of the atmoſphere into the ſhadow of the earth, ſometimes gives the moon, in total eclipſes, the obſcure reddiſh colour of brick. He alſo obſerves that, as the rays, which paſs through the greateſt tract of air become reddiſh, thoſe which paſs through the leaſt yellowiſh, and the intermediate ones orange, the red muſt converge the faſteſt into the ſhadow, after them the orange, and laſtly the yellow; ſo that the whole ſpace of the earth's ſhadow being filled with a faint light, the colour of which verges always more towards red, in approaching the earth, the colour of the moon in total eclipſes muſt vary likewiſe, according to her diſtance from the earth at the time of obſervation; and, he imagined, would always be more inclined to red at entering and leaving the ſhadow than in the middle.

Let aſtronomers, ſays he, determine whether the phenomena agree with this theory.*

CHAPTER IV.

MISCELLANEOUS OBSERVATIONS RELATING TO THE REFLEXION OF LIGHT.

§ 1. OBSERVATIONS *on the manner in which* heat *is excited by the reflexion of light.*

It is well known that one effect of light, eſpecially when it is denſe, as in the ſun's rays, is the production of heat, and we are obliged to Mr. Melville for ſome ingenious obſervations on the manner in which bodies are heated by light. He obſerves, that, as each colorific particle of an opaque body muſt be ſome what moved by the reaction of the particles of light, when it is reflected backwards and forwards between the ſame particles, it is manifeſt that they muſt, likewiſe, be agitated with a vibratory motion, and the time of a vibration will be equal to that which light takes up in moving from one particle of a body to another adjoining.† This diſtance, in the moſt ſolid opaque bodies, cannot be ſuppoſed greater than $\frac{1}{12500}$th of an inch, which ſpace a particle of light deſcribes in the $\frac{1}{125000000000000}$th of a ſecond: With ſo rapid a motion, therefore, may the internal parts of bodies be agitated by the influence of light, as to perform 125,000,000,000,000, vibrations, or more, in a ſecond of time.

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* Edinb. Eſſays, Vol. 2, p. 75.

† This is evidently a miſtake in Mr. Melville, as it is by no means neceſſary to ſuppoſe that the velocity of the parts of bodies ſhould be the ſame with that of the light, by the impulſe of which they are put in motion. The vibration of a fiddle ſtring doth not depend upon the time in which the fiddle ſtick paſſes over it.

The arrival of different particles of light at the ſurface of the ſame colorific particle, in the ſame, or different rays, may diſturb the regularity of its vibrations, but will evidently increaſe their frequency, or raiſe ſtill ſmaller vibrations among the parts, which compoſe thoſe particles; by which means the inteſtine motion will become more ſubtle, and more throughly diffuſed. If the quantity of light admitted into the body be increaſed, the vibrations of the particles muſt, likewiſe, increaſe in magnitude and velocity, till at laſt, they may be ſo violent, as to make all the component particles daſh one another to pieces by their mutual colliſion; in which caſe, the colour and texture of the body muſt be deſtroyed.*

Since there is no reflexion of light but at the ſurface of a medium, the ſame perſon obſerves that the greateſt quantity of rays, though crouded into the ſmalleſt ſpace, will not, of themſelves, produce any heat. From hence it follows, that the portion of air which lies in the focus of the moſt potent ſpeculum, is not at all affected by the paſſage of light through it, but continues of the ſame temperature with the ambient air, though any opaque body, or even any tranſparent body, denſer than air, when put in the ſame place, would be intenſely heated in an inſtant.

This conſequence, evidently flowing from the plaineſt and moſt certain principles, not ſeeming to have been rightly underſtood by many philoſophers, and even the ſilence of moſt phyſical writers concerning this paradoxical truth making it probable that they were unacquainted with it, he thought it worth his while to ſay ſomething in explication of it. He obſerves that the eaſieſt way to be ſatisfied of the matter experimentally is to hold a hair, or a piece of down, immediately above the focus of a lens or ſpeculum, or to blow a ſtream of ſmoke from a pipe horizontally over it; for if the air in the focus were hotter than the ſurrounding fluid, it would continually aſcend upon account of its rarefaction, and thereby ſenſibly agitate thoſe ſlender bodies. Or a lens may be ſo placed as to form its focus within a body of water; or ſome other tranſparent ſubſtance, the heat of which may be examined from time to time with a thermometer; but care muſt be taken, in this experiment, to hold the lens as near as poſſible to the tranſparent body, leſt the rays, by falling cloſer than ordinary on its ſurface, ſhould warm it more than the common ſun beams.*

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* Edinb. Eſſays, Vol. 2, p. 20.

To apply theſe obſervations to the explication of natural phenomena, he obſerves that, the atmoſphere is not much warmed by the paſſage of the ſun's light through it, but chiefly by its contact with the heated ſurface of the globe. This he thought furniſhed one very ſimple and plauſible reaſon, why it is coldeſt in all climates on the tops of very high mountains, namely becauſe they are removed to the greateſt diſtance from the general ſurface of the earth. For it is well known that a fluid heated by its contact with a ſolid body decreaſes in heat, in ſome inverſe proportion to the diſtance from the body. He himſelf found, by repeated trials, that the heat of water in deep lakes decreaſes regularly from the ſurface downwards. But to have this queſtion fully determined, the temperature of the air in the valley and on the mountain top muſt be obſerved every hour, both night and day, and carefully compared together. *

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* Ebinb. Eſſays, p. 23-

From this doctrine he thinks it reaſonable to ſuppoſe that the heat produced by a given number of rays, in an opaque body, of a given magnitude, muſt be greater when the rays are more inclined to one another, than when they are leſs ſo; for the direction of the vibrations, raiſed by the action of the light, whether in the colorific particles, or thoſe of an inferior order, will more interfere with one another, from whence the inteſtine ſhocks and colliſions muſt increaſe. Beſides this, the colorific particles of opaque bodies being diſpoſed in various ſituations, perhaps upon the whole, the rays will fall more directly on each, the more they are inclined to one another. Is not this, ſays he, the reaſon of what has been remarked by philoſophers, that the heat of the ſun's light, collected into a cone, increaſes in approaching the focus, in a much higher proportion than according to its denſity? That the difference of the angle in which the rays fall on any particle of a given magnitude, placed at different diſtances from the focus, is but ſmall, is no proof that the phenomenon cannot be aſcribed to it; ſince we know not in what high proportion one, or both the circumſtances now mentioned may operate. However, that it proceeds not from any unknown action of the rays upon one another, as has been inſinuated, is evident from this, that each particular ray, after paſſing through the focus, preſerves its own colour, and its own direction, in the ſame manner as if it were alone.†

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* Edinb. Eſſays, p. 63. † Ib. p. 60.

The attempts of the Abbé Nollet to fire inflammable ſubſtances by the power of the ſolar rays collected in the foci of burning mirrors has ſo near a relation to the ſubject of this chapter, that I have thought proper to give them a place in it. Conſidering the great power of burning mirrors and lenſes, eſpecially thoſe of late conſtruction, it will appear ſurpriſing that this celebrated experimental philoſopher ſhould not be able to fire any liquid ſubſtance. But though he made the trial with all the care imaginable, on the nineteenth of February, 1757, he was not able to do it, either with ſpirit of wine, olive oil, oil of turpentine, or æther; and though he could fire ſulphur, yet he could not ſucceed with ſpaniſh wax, roſin, black pitch, or ſuet. He both threw the focus of theſe mirrors upon the ſubſtances themſelves, and alſo upon the fumes that roſe from them; but all the effect was that the liquor boiled, and was diſperſed in vapour or very ſmall drops, but would not take fire. When linen rags, and other ſolid ſubſtances, were moiſtened with any of theſe inflammable liquids, they would not take fire till the liquid was diſperſed in a copious fume, ſo that rags thus prepared were longer in burning than thoſe that were dry.

M. Beaume, who aſſiſted M. Nollet in ſome of theſe experiments obſerved farther, that the ſame ſubſtances, which were eaſily fired by the flame of burning bodies, could not be ſet on fire by the contact of the hotteſt bodies that did not actually flame. Neither æther, nor ſpirit of wine could be fired with a hot coal, or even red hot iron, unleſs they were of a white heat. From theſe experiments our author concludes that, ſuppoſing the electric matter to be the ſame thing with fire or light, it muſt fire ſpirit of wine by means of ſome other principle. I have already obſerved that the members of the Academy Del Cimento could not fire ſeveral of theſe ſubſtances ; but this was ſo early in the hiſtory of philoſophy, that no body ſeems to have concluded, that, becauſe they failed in this attempt, the thing could not be done. However the Abbé informs us, that he read an account of his experiments to the Royal Academy at Paris ſeveral years before he attended to what had been done by the Italian philoſophers.*

§ 2. By the help of optical principles, and eſpecially obſervations on the reflexion of light, Mr. Melville diſcovered that bodies which ſeem to touch one another are not always in actual contact. It is common, ſays he, to admire the volubility and luſtre of drops of rain that lie on the leaves of colewort, and ſome other vegetables ; but no philoſopher, as far as he knew, had put himſelf to the trouble of explaining this curious phenomenon. Upon inſpecting them narrowly, he found that the luſtre of the drop is produced by a copious reflexion of light from the flattened part of its ſurface, contiguous to the plant. He obſerved, farther, that when the drop rolls along a part which has been wetted, it immediately loſes all its luſtre, the green plant being then ſeen clearly through it ; whereas, in the other caſe, it is hardly to be diſcerned.

From theſe two obſervations put together, he concluded that the drop does not really touch the plant, when it has the mercurial appearance, but is ſuſpended in the air, at ſome diſtance from it, by the force of a repulſive power. For there could not be any copious reflexion of white light from its under ſurface, unleſs there were a real interval between it and the ſurface of the plant.

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* Ac. Par. 1757, M. p. 896.

If that ſurface were perfectly ſmooth, the under ſurface of the drop would be ſo likewiſe, and would therefore ſhew an image of the illuminating body by reflexion, like a piece of poliſhed ſilver; but as it is conſiderably rough and unequal, the under ſurface becomes rough likewiſe, and ſo by reflecting the light copiouſly, in different directions, aſſumes the reſplendent white colour of unpoliſhed ſilver.

It being thus proved, by an optical argument, that the drop is not really in contact with the plant which ſupports it, it may eaſily be conceived whence its volubility ariſes, and why it leaves no moiſture where it rolls.*

§ 3. Before I conclude the obſervations which I have collected concerning the reflexion of light, I muſt not omit to take notice that Baron Alexander Funk, viſiting ſome ſilver mines in Sweden, obſerved that, in a clear day, it was as dark as pitch under ground in the eye of a pit, at ſixty or ſeventy fathoms deep; whereas in a cloudy or rainy day, he could even ſee to read at 106 feet deep. Inquiring of the miners, he was informed that this is always the caſe; and, reflecting upon it, he imagined that it aroſe from this circumſtance, that when the atmoſphere is full of clouds, light is reflected from them into the pit in all directions, and that thereby a conſiderable proportion of the rays are reflected perpendicularly upon the earth; whereas, when the atmoſphere is clear, there are no opaque bodies to reflect the light in this manner, at leaſt in a ſufficient quantity; and rays from the ſun itſelf can never fall perpendicularly in that country.†

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* Edinb. Eſſays, Vol. 2, p. 25.

† Swed. Abhandlungen, Vol. 3, p. 236.

SECTION

SECTION IV.

OBSERVATIONS AND DISCOVERIES RELATING TO THE REFRACTION OF LIGHT.

CHAPTER I.

OF THE DIFFERENT DISPERSION OF THE RAYS OF LIGHT, AS INDEPENDENT OF THEIR DIFFERENT REFRANGIBILITY.

THE moſt important diſcovery that I have to communicate to my reader relating to refraction is that of Mr. Dollond, which led the way to his capital improvement of teleſcopes. Notwithſtanding the great diſcovery of Sir Iſaac Newton, concerning the different refrangibility of the rays of light, he had no idea but that they were all affected in the ſame proportion by every medium; ſo that the refrangibility of the extreme rays might be determined, if that of the mean ones was given. From this it would follow, as Mr. Dollond obſerves, that equal and contrary refractions muſt not only deſtroy each other, but that the divergency of the colours from one refraction would likewiſe be corrected by the other, and there could be no poſſibility of producing any ſuch thing as refraction which would not be affected by the different refrangibility of light; or, in other words, that, however a ray of light might be refracted backwards and forwards by different mediums, as water, glaſs, &c. provided it was ſo done, that the emergent ray ſhould be parallel to the incident one, it would ever after be white; and conſequently, if it ſhould come out inclined to the incident, it would diverge, and ever after be coloured; and from this it was natural to infer, that all ſpherical object glaſſes of teleſcopes muſt be equally affected by the different refrangibility of light, in proportion to their apertures, of whatever materials they may be formed.*

For this reaſon Sir Iſaac Newton, and all other philoſophers and opticians, had deſpaired of bringing refracting teleſcopes to any great degree of perfection, without making them of an immoderate and very inconvenient length. They therefore applied themſelves chiefly to the improvement of the reflecting teleſcope, and the buſineſs of refraction was dropped till about the year 1747, when M. Euler, improving upon a hint of Sir Iſaac Newton's, formed a ſcheme of making object glaſſes of two materials, of different refractive powers; hoping that by this difference the refractions would balance one another, and thereby prevent the diſperſion of the rays that is occaſioned by the difference of refrangibility. Theſe object glaſſes were compoſed of two lenſes of glaſs with water between them. This memoir of M. Euler excited the attention of Mr. Dollond. He carefully went over all M. Euler's calculations, ſubſtituting for his hypothetical laws of refraction thoſe which had been actually aſcertained by the experiments of Newton, and found that, after this neceſſary ſubſtitution, it followed from M. Euler's own principles, that there could be no union of the foci of all kinds of colours, but in a lens infinitely large.

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* Phil. Tranſ. Vol. 50, p. 735.

M. Euler did not pretend to controvert the experiments of Newton; but he ſaid that they were not contrary to his hypotheſis, but in ſo ſmall a degree as might be neglected, and aſſerted that, if they were admitted in all their extent, it would be impoſſible to correct the difference of refrangibility occaſioned by the tranſmiſſion of the rays from one medium into another of different denſity; a correction which, he thought, was very poſſible, ſince he ſuppoſed it to be actually effected in the ſtructure of the eye, which he thought was made to conſiſt of different mediums for that very purpoſe. To this kind of reaſoning Mr. Dollond made no reply; but by appealing to the experiments of Newton, and the great circumſpection with which it was known that he conducted all his inquiries.

In this ſtate of the controverſy, the friends of M. Clairaut ingaged him to attend to it, and it appeared to him that, ſince the experiments of Newton cited by Mr. Dollond could not be queſtioned, the ſpeculations of M. Euler were more ingenious than uſeful.*

The ſame paper of M. Euler was alſo particularly noticed by M. Klingenſtierna of Sweden, who gave a conſiderable degree of attention to the ſubject, and diſcovered that, from Newton's own principles, the reſult of the 8th experiment of the ſecond book of his Opticks could not anſwer his deſcription of it. As this experiment began from this time to be the ſubject of a good deal of debate, I ſhall recite it at full length, with the deductions which the author drew from it.

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* Ac. Par. 1756, H. p. 183.

He found, he ſays, that when light goes out of air through ſeveral contiguous refracting mediums, as through water and glaſs, and thence goes out again into air, whether the refracting ſurfaces be parallel or inclined to one another, that light, as often as, by contrary refractions, it is ſo corrected, that it emerges in lines parallel to thoſe in which it was incident, continues ever after to be white; but if the emergent rays be inclined to the incident, the whiteneſs of the emerging light will, by degrees, in paſſing on from the place of emergence, become tinged at its edges with colours. This he tried by refracting light with priſms of glaſs, placed within a priſmatic veſſel of water.

By theorems deduced from this experiment he infers that the refractions of the rays of every ſort, made out of any medium into air, are known by having the refraction of the rays of any one ſort; and alſo that the refraction out of one medium into another is found as often as we have the refractions out of them both into any third medium.*

On the contrary, the Swediſh philoſopher obſerves, that, in this experiment, the rays of light, after paſſing through the water and the glaſs, though they come out parallel to the incident rays, will be coloured; but that the ſmaller the glaſs priſm is, the nearer will the reſult of it approach to Newton's deſcription.†

This paper of M. Klingenſtierna, being communicated to Mr. Dollond by M. Mallet, made him entertain doubts concerning Newton's report of the reſult of his experiment, and determined him, like a true philoſopher, who reſpects truth and fact more than authorities, to have recourſe to experiment.§

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* Newton's Opticks, p. 112. † Swed. Abhand. Vol. 16, p. 300. § Ac. Par. 1757, M. 854.

He therefore cemented together two plates of parallel glaſs at their edges, ſo as to form a priſmatic veſſel, when ſtopped at the ends or baſes; and the edge being turned downwards, he placed in it a glaſs priſm, with one of its edges upwards; and filled up the vacancy with clear water; ſo that the refraction of the priſm was contrived to be contrary to that of the water, in order that a ray of light, tranſmitted through both theſe refracting mediums, might be effected by the difference only between the two refractions. As he found the water to refract more or leſs than the glaſs priſm, he diminiſhed or increaſed the angle between the glaſs plates, till he found the two contrary refractions to be equal, which he diſcovered by viewing an object through this double priſm. For when it appeared neither raiſed nor depreſſed, he was ſatisfied that the refractions were equal, and that the emergent rays were parallel to the incident.

Now, according to the prevailing opinion, he obſerves that the object ſhould have appeared through this double priſm in its natural colour; for if the difference of refrangibility had been, in all reſpects, equal, in the two equal refractions, they would have rectified each other. But this experiment fully proved the fallacy of the received opinion, by ſhewing the divergency of the light, by the glaſs priſm to be almoſt double of that by the water; for the image of the object, though not at all refracted, was yet as much infected with priſmatic colours, as if it had been ſeen through a glaſs wedge only, whoſe refracting angle was near thirty degrees.

This experiment is the very ſame with that of Sir Iſaac Newton's above mentioned, notwithſtanding the reſult was ſo remarkably different: but Mr. Dollond aſſures us, that he uſed all poſſible precaution and care in his proceſs, and he kept his apparatus by him, that he might evince the truth of what he wrote, whenever he ſhould be properly required to do it.

He plainly ſaw, however, that if the refracting angle of the water veſſel could have admitted of a ſufficient increaſe, the divergency of the coloured rays would have been greatly diminiſhed, or entirely rectified, and that there would have been a very great refraction without colour, as he had already produced a great diſcolouring without refraction; but the inconveniency of ſo large an angle, as that of the priſmatic veſſel muſt have been, to bring the light to an equal divergency with that of the glaſs priſm whoſe angle was about ſixty degrees, made it neceſſary to try ſome experiments of the ſame kind with ſmaller angles.

Accordingly, he got a wedge of plate glaſs, the angle of which was only nine degrees, and uſing it in the ſame circumſtances, he increaſed the angle of the water wedge, in which it was placed, till the divergency of the light by the water was equal to that by the glaſs, that is, till the image of the object, though conſiderably refracted by the exceſs of the refraction of the water, appeared nevertheleſs quite free from any colours proceeding from the different refrangibility of the light; and, as near as he could then meaſure, the refraction by the water was about $\frac{5}{4}$ of that by the glaſs. He acknowledges, indeed, that he was not very exact in taking the meaſures, becauſe his buſineſs was not at that time to determine the exact proportions, ſo much as to ſhew that the divergency of the colours, by different ſubſtances, was by no means in proportion to the refractions, and that there was a poſſibility of refraction without any divergency of the light at all.

As theſe experiments clearly proved that different ſubſtances made the light to diverge very differently, in proportion to their general refractive power, Mr. Dollond began to ſuſpect that ſuch variety might poſſibly be found in different kinds of glaſs, eſpecially as experience had already ſhewn that ſome of the kinds made much better object glaſſes in the uſual way than others; and as no ſatisfactory cauſe had been aſſigned for ſuch difference, he thought there was great reaſon to preſume, that it might be owing to the different divergency of the light in the ſame refractions.

His next buſineſs, therefore, was to grind wedges of different kinds of glaſs, and apply them together; ſo that the refractions might be made in contrary directions, in order to diſcover, as in the above mentioned experiments, whether the refraction and the divergency of the colours would vaniſh together. But a conſiderable time elapſed before he could ſet about that work; for though he was determined to try it at his leiſure, for ſatisfying his own curioſity, he did not expect to meet with a difference ſufficient to give room for any great improvement of teleſcopes, ſo that it was not till the latter end of the year 1757, that he undertook it; but his firſt trials convinced him that the buſineſs deſerved his utmoſt attention and application.

He diſcovered a difference far beyond his hopes in the refractive qualities of different kinds of glaſs, with reſpect to the divergency of colours. The yellow, or ſtraw coloured foreign ſort, commonly called Venice glaſs, and the Engliſh crown glaſs, proved to be very nearly alike in that reſpect, though, in general, the crown glaſs ſeemed to make the light diverge the leſs of the two. The common Engliſh plate glaſs made the light diverge more, and the white cryſtal, or Engliſh flint glaſs, moſt of all.

It was now his buſineſs to examine the particular qualities of every kind of glaſs, that he could come at, not to amuſe himſelf with conjectures about the cauſe of this difference, but to fix upon two ſorts, in which it ſhould be the greateſt; and he ſoon found theſe to be the *crown glaſs*, and the white *flint glaſs*. He therefore ground one wedge of white flint, of about twenty-five degrees, and another of crown glaſs, of about twenty-nine degrees, which refracted very nearly alike, but their power of making the colours diverge was very different. He then ground ſeveral others of crown glaſs, to different angles, till he got one which was equal with reſpect to the divergency of the light, to that in the white flint glaſs. For when they were put together, ſo as to refract in contrary directions, the refracted light was intirely free from colours. Then meaſuring the refraction of each wedge, with theſe different angles, he found that of the white glaſs to be to that of the crown glaſs, nearly as *two* to *three*, and this proportion held very nearly in all ſmall angles; ſo that any two wedges, made in this proportion, and applied together, ſo as to refract in a contrary direction, would refract the light without any diſperſion of the rays.

That the general refractive power of theſe two kinds of glaſs may be more exactly compared with their power of ſeparating the rays of light, I ſhall here obſerve, that Mr. Dollond, in a letter to M. Klingenſtierna, quoted by M. Clairaut,* ſays, that the ſine of incidence in crown glaſs is to that of its general refraction as 1 to 1. 53, and in flint glaſs as 1 to 1. 583.

To apply this knowledge to practice, Mr. Dollond went to work upon the object glaſſes of teleſcopes, not doubting but that, upon the ſame principles on which a refracted colourleſs ray was produced by priſms, it might be done by lenſes alſo, made of ſimilar materials. And he ſucceeded by conſidering that, in order to make two ſpherical glaſſes that ſhould refract the light in contrary directions, the one muſt be concave and the other convex; and as the rays are to converge to a real focus, the exceſs of refraction muſt evidently be in the convex lens. Alſo, as the convex glaſs is to refract the moſt, it appeared from his experiments, that it muſt be made of crown glaſs, and the concave of white flint glaſs. Farther, as the refractions of ſpherical glaſſes are in an inverſe ratio of their focal diſtances, it follows that the focal diſtances of the two glaſſes ſhould be inverſely as the ratios of the refractions of the wedges; for being thus proportioned, every ray of light that paſſes through this combined glaſs, at whatever diſtance it may paſs from its axis, will conſtantly be refracted, by the difference between two contrary refractions, in the proportion required; and therefore the different refrangibility of the light will be intirely removed.

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* Ac. Par. 1757, M. p. 857.

Notwithſtanding our author had theſe clear grounds in theory and experiment to go upon, he found that he had many difficulties to ſtruggle with, when he came to reduce them into actual practice; but with great patience and addreſs, he, at length, got into a ready method of making teleſcopes upon theſe new principles.

His principal difficulties aroſe from the following circumſtances. In the firſt place the focal diſtances, as well as the particular ſurfaces, muſt be very nicely proportioned to the denſities, or refracting powers of the glaſſes, which are very apt to vary in the ſame ſort of glaſs, made at different times. Secondly, The centers of the two glaſſes muſt be placed truly in the common axis of the teleſcope, otherwiſe the deſired effect will be in a great meaſure deſtroyed. Add to theſe, that there are four ſurfaces to be wrought perfectly ſpherical; and any perſon, he ſays, but moderately practiſed in optical operations, will allow, that there muſt be the greateſt accuracy throughout the whole work. At length, however, after numerous trials, and a reſolute perſeverance, he was able to conſtruct refracting teleſcopes, with ſuch apertures and magnifying powers, under limited lengths, as, in the opinion of the beſt judges, far exceeded any thing that had been produced before, repreſenting objects with great diſtinctneſs, and in their true colours.*

It was objected to Mr. Dollond's diſcovery, that the ſmall diſperſion of the rays in crown glaſs is only apparent, owing to the opacity of that kind of glaſs, which does not tranſmit the fainter coloured rays in a ſufficient quantity; but this objection is particularly conſidered, and anſwered by M. Beguelin.§

As Mr. Dollond did not explain the methods which he took in the choice of different ſpheres, proper to deſtroy the effect of the different refrangibility of the rays of light, and gave no hint that he himſelf had any rule to direct himſelf in it; and as the calculation of the diſperſion of the rays, in ſo complicated an affair, is very delicate, M. Clairaut, who had given a good deal of attention to this ſubject, from the beginning of the controverſy, endeavoured to make out a compleat theory of it.

Without ſome aſſiſtance of this kind, it is impoſſible, ſays this author, to conſtruct teleſcopes of equal goodneſs with thoſe of Mr. Dollond, except by a ſervile imitation of his; which, however, on many accounts, would be very unlikely to anſwer. Beſides, Mr. Dollond only gave his proportions in general, and pretty near the truth; whereas the greateſt poſſible preciſion is neceſſary. Alſo the beſt of Mr. Dollond's teleſcopes fell far ſhort of the Newtonian ones;* whereas it might be expected that they ſhould exceed them, if the foci of all the coloured rays could be as perfectly united after refraction through glaſs, as after reflexion from a mirror; ſince there is more light loſt in the latter caſe than in the former.

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* Phil. Tranſ. Vol. 50, p. 743.

§ Ac. Berl. 1762, p. 74, &c.

With a view, therefore, to aſſiſt the artiſt, he endeavoured to aſcertain the refractive power of different kinds of glaſs, and alſo their property of ſeparating the rays of light, by the following exact methods. He made uſe of two priſms placed cloſe to one another, as Mr. Dollond had done, but, inſtead of looking through them, he placed them in a darkened room; and when the image of the ſun, tranſmitted through them, was perfectly white, he concluded that the different refrangibility of the rays was corrected.

In order to aſcertain with more eaſe the true angles that priſms ought to have, to deſtroy the effect of the difference of refrangibility, he conſtructed one which had one of its ſurfaces cylindrical, with ſeveral degrees of amplitude. By this means, without changing his priſms, he had the choice of an infinity of angles; among which, by examining the point of the curve ſurface which, receiving the ſolar ray, gave a white image, he could eaſily find the true one.

He alſo aſcertained the proportion in which different kinds of glaſs ſeparated the rays of light, by meaſuring, with proper precautions, the oblong image of the ſun, made by tranſmitting a beam of light through them. In making theſe experiments, he hit upon an eaſy method of convincing any perſon of the greater refractive power of Engliſh flint glaſs, above the common French glaſs, both with reſpect to the mean refraction, and the different refrangibility of the colours; for having taken two priſms, of theſe two kinds of glaſs, but equal in all other reſpects, and placed them ſo that they received, at the ſame time, two rays of the ſun, with the ſame degree of incidence, he ſaw that, of the two images, that which was produced by the Engliſh flint glaſs was a little higher upon the wall than the other, and longer by more than one half.*

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* It is poſſible that this aſſertion of M. Clairaut might be true at the time that it was made, but it is by no means ſo at preſent.

M. Clairaut was aſſiſted in theſe experiments by M. De Tournieres, and the reſults agreed with Mr. Dollond's in general; but whereas Mr. Dollond had made the diſperſion of the rays in glaſs and in water to be as five to four (acknowledging, however, that he did not pretend to do it with exactneſs) theſe gentlemen, who took more pains, and uſed more precautions, found it to be as three to two.† For the theorems and problems deduced by M. Clairaut from theſe new principles of opticks, with a view to the perfection of teleſcopes, I muſt refer my reader to the memoirs themſelves which I have juſt quoted.

The labours of M. Clairaut were ſucceeded by thoſe of M. D'Alembert, which ſeem to have given the makers of theſe achromatic teleſcopes all the aid that calculations can afford them. This excellent mathematician has likewiſe propoſed a variety of new conſtructions of theſe teleſcopes, the advantages and diſadvantages of which he diſtinctly notes; at the ſame time that he points out ſeveral methods of correcting the errors to which they are liable; as by placing the object glaſſes, in ſome caſes, at a ſmall diſtance from one another, and ſometimes by uſing eye glaſſes of different refractive powers; which is an expedient that ſeems not to have occured to any perſon before him. He even ſhews that teleſcopes may be made to advantage, conſiſting of only one object glaſs, and an eye glaſs of a different refractive power. Some of his conſtructions have two or more eye glaſſes of different kinds of glaſs. This ſubject he conſidered at large in one of the volumes of his *Opuſcules Mathematiques*. We have alſo three memoirs of M. D' Alembert upon this ſubject, among thoſe of the French Academy one in the year 1764, another in 1765, and a third in 1767.

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* Ac. Par. 1756, M. p. 624. † Ib. 1757, M. p. 859.

At the concluſion of his ſecond memoir he ſays, that he does not doubt but, by the different methods he propoſes, achromatic teleſcopes may be made to far greater degrees of perfection than any that have been ſeen hitherto, and even ſuch as is hardly credible; and though the crown glaſs, by its greeniſh colour, may abſorb ſome part of the red or violet rays, which, however, is not found to be the caſe in fact; that objection cannot be made to the common French glaſs, which is white, and which, on this account, he thinks muſt be preferable to the Engliſh crown glaſs.

He alſo obſerves that, from his theorems, a great number of tables might be calculated which would be very uſeful to perſons who apply themſelves to the improvement of achromatic teleſcopes; and from which conſiderable advantage would reſult to the ſciences of opticks, aſtronomy, and navigation.*

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* Ac. Par. 1767, M. p. 107.

Notwithſtanding Meſſrs. Clairaut and D'Alembert ſeemed to have exhauſted the buſineſs of calculation on the ſubject of Mr. Dollond's teleſcopes, no uſe could be made of their labours by foreign artiſts. For ſtill the teleſcopes made in England, according to no exact rule, as foreigners ſuppoſed, were greatly ſuperior to any that could be made elſe where, though under the immediate direction of thoſe able calculators. For this M. Beguelin aſſigned ſeveral reaſons. Among others, he thought that their geometrical theorems were too general, and their calculations too complicated for the uſe of workmen. He alſo thought that, in conſequence of neglecting ſmall quantities, which theſe calculators profeſſedly did, in order to make their algebraical expreſſions more commodious, their concluſions were not ſufficiently exact. But what he thought to be of the moſt conſequence was the want of an exact method of meaſuring the refractive and diſperſing powers of the different kinds of glaſs; and for want of this, the greateſt preciſion in calculation was altogether uſeleſs.*

Theſe conſiderations induced this gentleman to take another view of this ſubject; but ſtill he could not reconcile the actual effect of Mr. Dollond's teleſcopes with his own concluſions; ſo that he imagined, either that he had not the true refraction and diſperſion of the two kinds of glaſs given him, or elſe, that the aberration which ſtill remained, after his calculations, muſt have been deſtroyed by ſome irregularity in the ſurfaces of the lenſes.† He finds ſeveral errors in the calculations both of M. D'Alembert and Clairaut,§ and concludes with expreſſing his deſign to purſue this ſubject much farther.‡

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* Ac. Berl. 1762, p. 343. † Ib. p. 283. § Ib. p. 403, 409. ‡ Ib. p. 416.

M. Euler,

M. Klingenſtierna alſo, who expreſſes great ſatisfaction in the diſcovery which his obſervations on Newton had led Mr. Dollond to make, took the pains to make calculations for the uſe of the makers of theſe teleſcopes, ſimilar to thoſe of M. Clairaut, &c. They may be ſeen in the Swediſh Memoirs, Vol. 22, p. 75.

M. Euler, who firſt gave occaſion to this inquiry, which terminated ſo happily for the advancement of ſcience, being perſuaded both by his reaſoning and calculations, that Mr. Dollond had diſcovered no new principle in opticks, and yet not being able to controvert Mr. Short's teſtimony in favour of the goodneſs of his teleſcopes, concluded that this extraordinary effect was owing, in part, to the crown glaſs not tranſmitting all the red light, which, would otherwiſe have come to a different focus, and have diſtorted the image; but principally to his happening to hit on a juſt curvature of his glaſs, which, he did not doubt, would have produced the ſame effect if his lenſes had all been made of the ſame kind of glaſs* In another place he imagines that the goodneſs of Mr. Dollond's teleſcope might be owing to the eye glaſs.†

Upon the whole, the manner in which Mr. Euler writes upon this ſubject is one of the fineſt examples I have met with, of a ſtruggle between a regard to a theory ſuppoſed to be well eſtabliſhed, and to the teſtimonies of unexceptionable evidence, to facts which overthrow it. If my theory, ſays he, be true, § this diſagreeable conſequence follows, that Mr. Dollond's object glaſſes cannot be exempt from the diſperſion of colours; yet a regard to ſo reſpectable a teſtimony embarreſſes me extremely, it being as difficult to queſtion ſuch expreſs authority, as to abandon a theory which appears to me perfectly well founded; and to embrace an opinion, which is as contrary to all the eſtabliſhed laws of nature, as it is ſtrange and ſeemingly abſurd *(bizarre et revoltant.)* He even appeals to experiments made in a darkened room, in which, he ſays, he is confident, that Mr. Dollond's object glaſſes would appear to have the ſame defects that others are ſubject to.*

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* Ac. Berl. 1762, p. 245, 253. † Ib. p. 261. § Ib. p. 25.

Not doubting, however, but that Mr. Dollond, either by chance, or otherwiſe, had made ſome conſiderable improvement in the conſtruction of teleſcopes, by the combination of glaſſes, he abandoned his former project, in which he had recourſe to different mediums, and confined his attention to the correction of the errors which ariſe from the curvature of lenſes. But while he was proceeding, as he imagined, upon the true principles of opticks, of which, however, he made but little uſe, he could not help expreſſing his ſurprize, that Mr. Dollond ſhould have been led to ſo important a diſcovery by reaſoning, in a manner quite contrary to the nature of things.§ At length, however, M. Euler was convinced of the reality and importance of Mr. Dollond's diſcoveries, and very frankly acknowledges that he ſhould, perhaps, never have been brought to aſſent to it; had not his friend M. Clairaut, aſſured him, that the experiments of the Engliſh Optician might be depended upon. However the experiments of M. Zeiher of Peterſburgh gave him the moſt complete ſatisfaction with reſpect to this new law of refraction.

This gentleman demonſtrated that it is the *lead,* in the compoſition of glaſs, that gives it this remarkable property, that, while the refraction of the mean rays is nearly the ſame, that of the extremes differs conſiderably. And, by increaſing the quantity of lead in the mixture, he produced a kind of glaſs, which occaſioned a much greater ſeparation of the extreme rays than the flint glaſs, which Mr. Dollond had made uſe of. By this evidence M. Euler owns that he was compelled to renounce the principle which, before this time, had been conſidered as inconteſtible, viz. that the diſperſion of the extreme rays depends upon the refraction of the mean, and that the former varies with the quality of the glaſs, while the latter is not affected by it.*

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* Ac. Berl. 1762, p. 260. § Ib. p. 261.

From theſe new principles M. Euler deduces theorems concerning the combination of the lenſes, and in a manner ſimilar to M. Clairaut and D'Alembert, points out methods of conſtructing achromatic teleſcopes.

While he was employed upon this ſubject, he informs us, that he received a letter from M. Zeiher, dated Peterſburgh 30th of January 1764, in which he gives him a particular account of the ſucceſs of his experiments on the compoſition of glaſs, and that having mixed minium and ſand in different proportions, the reſult of the mean refraction and the diſperſion of the rays varied according to the following table.

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* Ac. Berl. 1766, p. 119.

Proportion of Minium to Flint.	Mean refraction from air into glaſs.	Diſperſion of the rays in compariſon of crown glaſs.
I. - - - 3 : 1	2028 : 1000	4800 : 1000
II. - - - 2 : 1	1830 : 1000	3550 : 1000
III. - - - 1 : 1	1787 : 1000	3259 : 1000
IV. - - - $\frac{3}{4}$: 1	1732 : 1000	2207 : 1000
V. - - - $\frac{1}{2}$: 1	1724 : 1000	1800 : 1000
VI. - - - $\frac{1}{4}$: 1	1664 : 1000	1354 : 1000

By this table it is evident that a greater quantity of lead not only occaſions a greater diſperſion of the rays, but alſo conſiderably encreaſes the mean refraction. The firſt of theſe kinds of glaſs, which contains three times as much minium as flint, will appear very extraordinary; ſince, hitherto, no tranſparent ſubſtance has been known, whoſe refractive power exceeded the ratio of two to one, and that the diſperſion occaſioned by this glaſs is almoſt five times as great as that of crown glaſs, which could not be believed by thoſe who entertained any doubt concerning the ſame property in flint glaſs, the effect of which is, three times as great as crown glaſs. One may obſerve, however, in theſe kinds of glaſs ſomething of a proportion between the mean refraction and the diſperſion of the rays, which may enable us to reconcile theſe ſurpriſing effects with other principles already known.

Here, however, M. Euler anounces to us another diſcovery of the ſame M. Zeiher, no leſs ſurpriſing than the former, and which diſconcerted all his ſchemes for reconciling the above mentioned phenomena. As the ſix kinds of glaſs mentioned in the above table were compoſed of nothing but minium and flint,

M. Zeiher happened to think of mixing alkaline ſalts with them, in order to give the glaſs a conſiſtence more proper for dioptric uſes; when he was much ſurpriſed to find this mixture greatly diminiſhed the mean refraction, almoſt without making any change in the diſperſion. After many trials, he, at length, obtained a kind of glaſs greatly ſuperior to the flint glaſs of Mr. Dollond, with reſpect to the conſtruction of teleſcopes, ſince it occaſioned three times as great a diſperſion of the rays as the common glaſs, at the ſame time that the mean refraction was only as 1. 61 to 1.*

M. Euler alſo gives particular inſtructions how to find both the mean and extreme refractive power of different kinds of glaſs, and particularly adviſes to make uſe of priſms with very large refracting angles, not leſs than ſeventy degrees.

Notwithſtanding it evidently appeared, I may ſay to almoſt all philoſophers, that Mr. Dollond had made a real diſcovery of ſomething not comprehended in the optical principles of Sir Iſaac Newton, it did not appear ſo to ſo ſenſible a man, and ſo good a mathematician as Mr. Murdoch is univerſally acknowledged to be. Upon this occaſion he interpoſed in the defence, as he imagined, of Sir Iſaac Newton, maintaining that Mr. Dollond's poſitions, which, he ſays, he knows not by what miſhap have been deemed paradoxes in Sir Iſaac's theory of light, are really the neceſſary conſequences of it. He alſo endeavours to ſhew that Sir Iſaac might not be miſtaken in his account of the experiment above-mentioned; but, admitting all that he advances in this part of his defence, Newton muſt have made uſe of a priſm with a much ſmaller refracting angle than, from his own account of his experiments, we have any reaſon to believe that he ever did make uſe of.*

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* Ac. Berl. 1766, p. 150.

The fact probably was, that Sir Iſaac deceived himſelf in this caſe, by attending to what he imagined to be the clear conſequence of his other experiments; and though the light he ſaw was certainly tinged with colours, and he muſt have ſeen it to be ſo, yet he might imagine that this circumſtance aroſe from ſome imperfection in his priſms, or in the diſpoſition of them, which he did not think it worth his while to examine. It is alſo obſervable that Sir Iſaac is not ſo particular in his deſcription of his priſms, and other parts of his apparatus, in his account of this experiment, as he generally is in other caſes; and therefore, probably, wrote his account of it from his memory only. In reality, it is no reflexion upon Sir Iſaac Newton, who did ſo much, to ſay that he was miſtaken in this particular caſe, and that he did not make the diſcovery that Mr. Dollond did; though it be great praiſe to Mr. Dollond, and all thoſe perſons who contributed to this diſcovery, that they ventured to call in queſtion the authority of ſo great a man.

It is not ſaying too much of Sir Iſaac Newton, to ſay that, having given ſo much of his attention to this ſubject, he muſt have been acquainted with the utmoſt extent of his own principles; and therefore that, if it had been poſſible, by the help of them only, to have made any conſiderable improvement in refracting teleſcopes; he, who thought ſo much about teleſcopes, could not have avoided hitting upon it. But, notwithſtanding all that Mr. Murdoch has ingeniouſly advanced in favour of Sir Iſaac Newton, or rather againſt Mr. Dollond, it is not true, in fact, that ſuch teleſcopes as his could be conſtructed, if the refrangibility of the extreme rays was, in all ſubſtances, in the ſame proportion to that of the mean ones, which it is evident Sir Iſaac Newton took it for granted it was.

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* Phil. Tranſ. Vol. 53, p. 192.

In 1748 Mr. Dollond made a farther improvement in his teleſcopes, by making two object glaſſes of crown glaſs, and one of flint, which his ſon Mr. Peter Dollond compleated, ſo as to finiſh an object glaſs, or rather a ſet of object glaſſes, of five feet focal length, with an aperture of $3\frac{3}{4}$ inches, compoſed of two convex lenſes of crown glaſs, and a concave one of flint glaſs. Afterwards he made one of $3\frac{1}{2}$ feet focal length, with the ſame aperture as the former.*

As it may be uſeful to compare the refractive power of different kinds of glaſs with their ſpecific gravity, I ſhall here take notice that the ſpecific gravities of different kinds of glaſs, of which teleſcopes are uſually made were examined by Mr. Martin, and found by him to be as follows.†

The ſpecific gravity of white flint is as	1	to	3.29
Common plate glaſs - - -	1		2.76
Crown glaſs - - - - -	1		2.52
Yellow plate - - - - -	1		2.52
Brazil pebble - - - - -	1		2.62

Hence, he obſerves, it appears that the refractive powers of theſe ſeveral kinds of glaſs are very nearly, as their ſpecific gravities, which are nearly equal in *crown glaſs*, *yellow glaſs*, and *Brazil pebbles*, and alſo in that which is called *Dutch white* and in *Corniſh diamond*.

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* Phil. Tranſ. Vol. 55, p. 56.

† New Elements of Opticks, p. 69.

CHAPTER II.

Obsevations on the REFRACTIVE POWER of DIFFERENT SUBSTANCES.

M. Euler the elder, having filled two meniſcus glaſſes with water, and found their focus to be eight feet, and that after filling them with ſpirit of wine, it was only five feet, when the difference of the refractive powers of theſe liquors was ſo ſmall as in the proportton of 73 to 75, he thought that this would be the beſt and moſt accurate method of aſcertaining the refractive powers of all tranſparent liquors.*

M. Euler the younger, carried into execution the ſcheme propoſed by his father, with reſpect to ſeveral tranſparent liquors, after firſt demonſtrating the certainty of the method, and ſhewing the manner in which allowance muſt be made for the variation of refraction ariſing from the glaſs of the meniſcuſes, and the form of them.† He was furniſhed with an opportunity of trying theſe experiments by having ſome glaſſes which proved to be very convenient for this purpoſe, though they were ground for object glaſſes to teleſcopes, on the ſcheme mentioned above; which not ſucceeding, they had been laid aſide as uſeleſs.

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* Ac. Berlin. 1756, p. 237.

† Ib. 1762, p. 279.

The deſcription M. Euler gives of the manner in which his experiments were made, ſhews that this method is an exceedingly eaſy one. He had nothing to do but to put his two glaſſes together, when they were plunged in the fluid, the refractive power of which he wanted to determine; for the edges being ground flat, they immediately cohered, ſo that none of the fluid could eſcape, and then they might both together be wiped, and uſed as one object glaſs, in a long tube, which he could lengthen or ſhorten at pleaſure; and applying an eye glaſs to it, all the obſervation he had to make was to meaſure the exact length of his tube, when he could ſee through it, moſt diſtinctly, a turret which was at a conſiderable diſtance from him. Or, when the focal diſtance was leſs than a foot, he only obſerved at what diſtance from a wall the image of an oppoſite window was the moſt diſtinct.*

The refractive power of the fluids which he examined in this manner, he expreſſed in the following tables.

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* Ac. Berl. 1762, p. 302.

A ray of light paſſing from air into	The ſine of the angle of incidence will be to that of refraction as	
Diſtilled water	1,3358	To unity.
Rain water	1,3358	
Well water	1,3366	
French wine	1,3453	
French brandy	1,3603	
Ditto a ſtronger kind	1,3646	
Rectified ſpirit of wine	1,3685	
Ditto more highly rectified	1,3706	
The white of an egg	1,3685	
Diſtilled vinegar	1,3442	To unity.
A ſolution of gum arabic	1,3467	
A ſolution of 2 ſcruples of white ſugar in an oz. of water	1,3457	
A ſolution of 2 ſcruples of rock ſalt, in ditto	1,3477	
A ſolution of 2 ſcruples of ſalt of urine, in ditto	1,3400	
Oil of Provence	1,4651	
Oil of Turpentine	1,4822	

N. B. The rock ſalt, and the ſalt of urine was purified by a double cryſtallization.

By a ſecond pair of meniſcuſes.

Diſtilled water	1,3358	To unity.
Rain water	1,3358	
Well water	1,3362	
French wine	1,3458	
French brandy	1,3600	
Ditto a ſtronger kind	1,3618	
Spirit of wine rectified	1,3683	
Ditto more higher rectified	1,3705	
Tea	1,3376	
Mineral alkali ſaturated (I ſuppoſe with water)	1,3600	To unity.
Spirit of Nitre	1,4025	
A ſolution of 2 ſcruples of Glauber's ſalt in an oz. of water	1,3430	
A ſolution of 2 ſcruples of digeſtive ſalt of Sylvius in do.	1,3454	
A ſolution of 2 ſcruples of Sal ammoniac in ditto	1,3488	
A ſolution of 2 ſcruples of copperas in ditto	1,3395	
Oil of Tartar per deliquium	1,3917	
Oil of Provence	1,4648	
Oil of Turpentine	1,4822	

The refractive power of ſome other fluids found by the ſecond pair of meniſcuſes 18 Auguſt 1761, Reaumur'sThermometer being 31 *degrees above the freezing point.*

A ray of light paſſing from air into	The ſine of the angles of incidence will be to that of refraction as	
Well water	1,3351	
Four different ſolutions of nitre		
viz. 12 grains of depurated nitre to an ounce of water	1,3380	
24 —— —— —— ——	1,3398	
38 —— —— —— ——	1,3450	
to 2 ounces of water	1,3540	
Four different infuſions	1,3351	
Of parſley	1,3355	
Of nutſhells	1,3359	To unity.
Of ſaffron	1,3363	
Of fiſh ſcales	1,3353	
Of Zelter water	1,3353	
Of Eger water	1,3358	
Anodyne liquor	1,3650	
Spirit of camphor	1,3757	
Spirit of Saxon ſope	1,4088	

That the refractive power of theſe fluids, determined in this manner by M. Euler, may be more eaſily compared with Mr. Haukſbee's and Sir Iſaac Newton's experiments, he reduced the reſult of their obſervations into the following tables exactly ſimilar to his own. As the original tables are not large, I imagined that the reader would not be diſpleaſed with ſeeing them again in a different form; and yet might chuſe to have it in his power to conſult them in the form in which their authors diſpoſed them, eſpecially as they are often quoted and referred to in that form.

The refractive power of certain fluids found by Mr. Haukſbee, ſee his Experiments, p. 292.

A ray of light paſſing from air into	The ſine of the angle of incidence will be to that of refraction as	
Water	1,3359	
Spirit of honey	1,3359	
——— ſal ammoniac	1,3377	
Oil of amber	1,3377	
Spirit of hartſhorn	1,3390	
Human urine	1,3419	
White of an egg	1,3511	
Jelly of hortſhorn	1,3541	
French brandy	1,3626	To unity.
Spirit of wine	1,3721	
Diſtilled vinegar	1,3721	
Gum ammoniac	1,3723	
Aqua regia	1,3898	
——— from aqua fortis and ſal ammoniac	1,3964	
Aqua fortis	1,4044	
Spirit of nitre	1,4076	
The cryſtalline humour of an ox's eye	1,4635	
Butter of antimony	1,6831	
Oil of vitriol	1,4262	
Oil of wax	1,4524	
Oil of lavender	1,4690	
Oil of roſemary	1,4719	
Oil of origanum	1,4770	
Oil of ginger	1,4799	
Oil of oranges	1,4833	
Oil of turpentine	1,4833	To unity.
Oil of ſavine	1,4857	
Oil of flowers of muſk	1,4878	
Oil of mint	1,4911	
Oil of amber	1,5010	
Oil of cummin	1,5088	
Oil of fennel	1,5114	
Oil of cloves	1,5136	
Oil of aniſe	1,5191	
Oil of cinnamon	1,5340	
Oil of ſaſſafras	1,5443	

The refractive powers of ſome tranſparent liquors according to the obſervations of Sir Iſaac Newton. *See Opticks*, p. 247.

A ray of light paſſing from air into	The ſine of the angle of incidence will be to that of refraction as	
A yellow pſeudo topaz - - - - -	1,6429	
Air - - - - - - - - - - - -	0,9997	
Glaſs of antimony - - - - -	1,8889	
A ſelenites - - - - - - - -	1,4878	
Glaſs - - - - - - - - - -	1,5500	To unity.
Cryſtal of the rock - - - - -	1,5620	
Iſland cryſtal - - - - - - -	1,6666	
Sal gem - - - - - - - -	1,5455	
Alum - - - - - - - - - -	1,4577	
Borax - - - - - - - - - -	1,4667	
Nitre - - - - - - - - - -	1,5238	
Dantzick vitriol - - - - - -	1,5000	
Oil of vitriol - - - - - - -	1,4285	
Rain water - - - - - - - -	1,3358	
Gum arabic - - - - - - - -	1,4771	To unity.
Spirit of wine well rectified - - -	1,3698	
Camphire - - - - - - - - -	1,5000	
Oil Olive - - - - - - - - -	1,4666	
Linſeed oil - - - - - - - -	1,4814	
Spirit of turpentine - - - - -	1,5625	
Amber - - - - - - - - - -	1,5556	
A diamond - - - - - - - -	2,4390	

M. Euler charges Mr. Haukſbee with inaccuracy, and obſerves that he makes the refractive power of ſpirit of wine conſiderably greater than Newton makes it, though the ſpirit of wine that Newton made uſe of was very highly rectified. He alſo thinks Mr. Haukſbee miſtaken in making the refractive power of the white of egg leſs than that of French brandy; whereas, by repeated experiments, he found it to be equal to that of rectified ſpirit of wine. Mr. Haukſbee alſo made the refractive power of diſtilled vinegar equal to that of ſpirit of wine, whereas M. Euler could only make it to be equal to that of French wine.*

The following obſervations of M. Euler on this ſubject are curious and uſeful. There is no fluid, and probably no tranſparent ſubſtance, of any kind, the refractive power of which is leſs than that of rain water, or diſtilled water. It is ſurpriſing that betwixt air and rain water, there is no ſubſtance that has an intermediate refractive power.

After rain water immediately follows *well-water*, but there are probably as many varieties in its refractive power as there are different wells. We may, however, conclude that the ratio of refraction from air into well-water is contained between the limits of 1,336 to 1, and 1,337 to 1,

Spirituous liquors have a greater refractive power in proportion to their ſtrength, but the ratio of refraction from air into any ſpirituous liquor is never leſs than 1, 34, nor greater than 1, 37.

There is, probably, no kind of ſalt, but what, being diſſolved in water, increaſes its refractive power.

The ſolutions of ſalt of urine and of vitriol have the leaſt refractive powers, and thoſe of rock ſalt and ſal ammoniac the greateſt; but the ratio of refraction from air into any ſaline ſolution, preſerving the proportion of one ounce of ſalt to twelve of water, will be contained between the limits of 1, 34 to 1, and 1,35 to 1.

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* Ac. Berl. 1762, p. 313.

Diſtilled vinegar and the ſolution of gum arabic have nearly the ſame refractive power with common French wine, and the refractive power of white of egg is the ſame as that of rectified ſpirit of wine.

Mineral alkali ſaturated, ſeems to have the ſame refractive power with very ſtrong brandy.

Spirit of nitre and oil of Tartar *per deliquium* have a medium refractive power between ſpirituous liquors and oils.

The refractive powers of oils approach the neareſt to that of glaſs, eſpecially oil of turpentine, which had the greateſt refractive power of all the fluids, on which he had made experiments.*

Sir Iſaac Newton ſuſpected that different *degrees of heat* might have ſome effect on the refractive power of bodies, but his method of determining the general refraction was not ſufficiently accurate to aſcertain this circumſtance; but happily this method of M. Euler's proved to be well adapted to this purpoſe, to which he did not fail to apply it.

1. When a ſingle object glaſs was held in boiling water, till it had acquired the ſame degree of heat, he found that its focal diſtance was ſixteen inches; whereas, when it was cold, it was 16¼. This experiment was made Auguſt 22, 1761, when Reaumur's thermometer (which was at 80 for the heat of boiling water) was at 14; ſo that an increaſe of 66 degrees of heat made a difference of ¼ of an inch in the focal diſtance of the lens.

2. Filling the cavity of two meniſcuſes with boiling water, the focus was 45 inches, and when cold it was 41, 44. In this caſe a difference of 46 degrees of heat made an augmentation of 3. 56 inches in the focal diſtance.

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* Ac. Berl. 1762, p. 310.

3. Holding a ſingle object glaſs over a quantity of boiling water, the difference of 33 degrees of heat leſſened the focal diſtance half an inch. When it was heated to 30 degrees above the freezing point, the focal diſtance meaſured 48. 31 inches. When it was cooled, till it was only 8 degrees above the ſame point, it was increaſed $\frac{1}{4}$ of an inch; and when it was quite cold, which was 3 degrees below the freezing point, it was increaſed $\frac{1}{4}$ of an inch more.

4. Holding two meniſcuſes, filled with well-water, over a quantity of boiling water, till the thermometer (which was held cloſe to it) roſe from 3 degrees below, to 30 above the freezing point, the focal diſtance meaſured 37. 56 inches; at 6 above freezing it was 32. 44; at $\frac{1}{2}$ a degree above freezing it was 31. 44 inches. He then took his apparatus into the open air, when the water froze, and loſt its tranſparency.

5. The ſame meniſcuſes, filled with French brandy, at 3 degrees below freezing, the focal diſtance meaſured 18. 31 inches; but when they were heated to 30 degrees above freezing it increaſed 7. 13 inches.

6. When the meniſcuſes were filled, about the ſame time, with a ſolution of rock ſalt, the focal diſtance meaſured 21. 81 inches; but when they were heated to 30 degrees above freezing, it was increaſed 2. 88 inches.

7. When the meniſcuſes were filled with oil of olives, the thermometer being at 8 below freezing, the focal diſtance was 6. 75 inches; but when they were 50 degrees above freezing, it was 7. 37 inches.

8. On the ſame day, the thermometer being 8 below freezing, the meniſcuſes being filled with Eger water, the focal diſtance

was 36. 56 inches; but being 50 degrees above freezing it was 53 inches, ſo that 58 degrees of heat had increaſed the focal diſtance 16. 15 inches.*

From theſe experiments M. Euler makes the following obſervations. The firſt and third experiment ſhew that the focal diſtance of a ſingle lens of glaſs diminiſhes with the heat communicated to it, and this diminution of the focal diſtance is not owing to the increaſe of bulk in the glaſs by heat; for the effect of this change is both inconſiderable, and of a contrary nature. There can be no doubt, therefore, but that this alteration in the focal diſtance is owing to a change in the refractive power of the glaſs itſelf, which, as well as, probably, that of all other tranſparent ſubſtances, is increaſed by heat, and diminiſhed by cold.

It may ſeem ſurpriſing that the focal diſtance of a ſingle lens ſhould decreaſe with heat, and yet that of the meniſcuſes filled with any fluid, ſhould increaſe with heat; but M. Euler obſerves, that it by no means follows from hence, that theſe fluids are affected by heat in a manner different from glaſs; and, after computing the effect of every circumſtance of this complex experiment, of the two glaſſes and the fluid combined, he concludes that heat increaſes the refractive power of water, and of other fluids as well as that of glaſs.†

He farther obſerves that as 66 degrees of heat diminiſhed the focal diſtance $\frac{1}{65}$ part, 33 degrees ought to have diminiſhed it $\frac{1}{130}$ part; whereas the diminution in this caſe was $\frac{1}{97}$. From hence, ſays he, one may perhaps conclude, that when it is very cold, the ſame change in the thermometer has a greater effect on the refractive power of the glaſs than when it is very hot. But he acknowledges that experiments of this kind are not capable of ſo much preciſion as one could wiſh, and that, perhaps, in reality, the 66 degrees made a change of $\frac{1}{60}$, and the 33 degrees of $\frac{1}{120}$; but he imagined that a great number of experiments, made in different temperatures of the air, might decide this queſtion, eſpecially if object glaſſes of a very great focal diſtance were made uſe of.*

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* Ac. Berl. 1762, p. 328.

† Ib. p. 333, 341.

M. Euler concludes this curious paper with expreſſing his wiſhes, that a greater number of experiments of this kind were made, in order to aſcertain, with more exactneſs than he could pretend to have done it, the quantity of augmentation which the refractive power of glaſs receives by heat. He imagined, alſo, that by purſuing the experiments with the meniſcuſes, not only the change of the refractive power of the liquids contained in them might be determined, but that one could hardly fail to make many diſcoveries concerning the nature of refraction in general, which might be of uſe in perfecting the theory of it.§

The Duke De Chaulnes, not ſatisfied with the methods uſed by Newton and others to determine the refractive power of glaſs, hit upon another which is very ingenious, and, if good inſtruments and great attention be applied, may, perhaps, ſucceed pretty well. He formed the glaſs into plates, the ſurfaces of which were truly plane and parrallel, and having placed ſmall objects on each of them, he found, by means of a compound microſcope, to which he applied the moſt exquiſite micrometers, the different diſtances at which they were diſtinctly viſible, and compared them with the thickneſs of the glaſs. This, he ſays, gives the proportion of the ſines of the angles of incidence and refraction in that kind of glaſs directly. Thus if B and A fig. 107, be at the diſtance of the two ſurfaces, any object at A will be ſeen by rays ſo refracted at D and *d*, as if they had proceeded from C, and the diſtance between C and A will be greater or leſs in proportion to the refractive power of the glaſs.*

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* Ac. Berl. p. 334. § Ib. p. 342.

In this method he aſcertained the mean refractive power of fifteen kinds of glaſs. The real thickneſs of his piece of crown glaſs was 2. 78 of an inch, the thickneſs by refraction was 1. 85, and the proportion of the ſine of incidence and refraction was 0. 665. In flint glaſs the real thickneſs was 2. 42, the thickneſs by refraction 1. 52, and the proportion of the ſines 0. 628.

To aſcertain the proportion in which different kinds of glaſs diſperſed different kinds of rays, he fixed, in the aperture of a camera obſcura, pieces of glaſs of the beſt red and blue that he could procure, with hairs drawn acroſs one another upon them; and, with the greateſt care, he meaſured the diſtances at which the rays were united, after being received, at a known diſtance, by the lens, the mean focus of which he had very accurately found. In this manner he compared the glaſs of St. Gobin with Engliſh flint glaſs, by which it appeared, that the true focus of the red rays in the flint glaſs was one foot 6. $6\frac{1}{16}$ inches, and that of the blue rays one foot 6. $3\frac{3}{16}$ inches; but the focus of the glaſs of St. Gobin was for the red rays one foot 8. $2\frac{7}{16}$ inches, and for the blue one foot 8. $0\frac{10}{16}$ inches; the white focus in the former being one foot 6. $4\frac{15}{16}$ inches, and in the latter one foot 8. $2\frac{3}{16}$ inches; ſo that the diſperſion in the former was to that in the latter as 3 to $1\frac{13}{16}$.§

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* Ac. Par. 1767, M. p. 431.

§ Ib. p. 441.

With the ſame attention the Duke De Chaulnes contrived methods of meaſuring the preciſe degree of curvature proper for glaſſes to be uſed in achromatic teleſcopes, and the diſtances at which they ought to be placed. He alſo took a great deal of pains in applying micrometers to all the inſtruments he made uſe of.

CHAPTER III.

Of the refractive power of the ATMOSPHERE.

OBSERVATIONS on the refractive power of the atmoſphere have been happily applied by M. Lambert, to correct the calculations by which the heights of mountains have been determined, and to demonſtrate a more perfect correſpondence of barometrical obſervations with them.

If AM, fig. 108, repreſents the ſurface of the earth, and Mo the height of a mountain upon it, a ray coming from the top of it to the eye of a ſpectator at A will come in the direction AO. But the true altitude, Mo, cannot be determined till the angle of refraction, OAo, be deducted from the angle of obſervation OAM. Upon theſe principles he calculated the following uſeful table, ſuppoſing E the radius of the trajectory, which the rays of light make through the atmoſphere, to be to the radius of the earth as 7 to 1, the trajectory being ſafely conſidered as a circle, in ſo ſmall a diſtance.

o O	A M	o O	A M	o O	A M
1	6761	34	39427	67	55344
2	9562	35	40002	68	55758
3	11711	36	40570	69	56166
4	13523	37	41130	70	56571
5	15119	38	41686	71	56974
6	16562	39	42226	72	57374
7	17889	40	42764	73	57770
8	19124	41	43296	74	58165
9	20285	42	43820	75	58556
10	21388	43	44338	76	58940
11	22425	44	44851	77	59333
12	23422	45	45357	78	59719
13	24379	46	45860	79	60098
14	25300	47	46354	80	60477
15	26187	48	46845	81	60855
16	27046	49	47331	82	61230
17	27879	50	47812	83	61600
18	28687	51	48287	84	61970
19	29470	52	48758	85	62338
20	30238	53	49224	86	62705
21	30985	54	49687	87	63069
22	31715	55	50144	88	63430
23	32427	56	50599	89	63788
24	33125	57	51049	90	64146
25	33808	58	51495	91	64501
26	34478	59	51937	92	64854
27	35134	60	52375	93	65206
28	35778	61	52810	94	65556
29	36412	62	53240	95	65903
30	37035	63	53668	96	66250
31	37647	64	54092	97	66595
32	38249	65	54514	98	66937
33	38842	66	54931	99	67275
34	39427	67	55344	100	47615

The firſt column of this table, o O, expreſſes the quantity by which the obſerved altitude of a mountain muſt be diminiſhed, and the ſecond column AM, the diſtance that correſponds to it. Taking a medium between the diameter of the equator and the axis of the earth, Mr. Lambert ſuppoſes that, if OM be

15.311 toiſes, o O will be 2.1873 toiſes, from whence the numbers in the table are deduced, o O increaſing or decreaſing, as the ſquare of the diſtance AM.

Applying this correction to the heights of the mountains obſerved by M. Caſſini, he found that ſome of them had been ſuppoſed to be 40 or 50 toiſes too high, and others almoſt as much too low; ſo that it was no wonder that the heights of the barometer could not be made to correſpond to them; whereas, when he had applied this correction, he was ſurpriſed to find the correſpondence to be ſo very exact as it is. He has, therefore, preſented us with the following table of the heights of the barometer anſwering to the true heights of mountains; and, comparing it with the heights that have been actually meaſured, he found the difference to be but a very few toiſes, except where there was reaſon to think that the menſuration had not been exact.

Height of the Barometer.	Height of the Places.	Height of the Barometer.	Height of the Places.	Height of the Barometer.	Height of the Places.
27 : 11	12,0	24 : 8	529,3	21 : 5	1136,4
- - 10	24,1	- - 7	544,4	- - 4	1153,2
- - 9	36,3	- - 6	558,8	- - 3	1170,1
- - 8	48,6	- - 5	573,4	- - 2	1187,1
- - 7	60,9	- - 4	588,0	- - 1	1204,1
- - 6	73,3	- - 3	602,7	21 : 0	1221,2
- - 5	85,7	- - 2	617,3	20 : 11	1238,4
- - 4	98,2	- - 1	632,1	- - 10	1255,6
- - 3	110,8	24 : 0	647,9	- - 9	1272,9
- - 2	123,3	23 : 11	661,8	- - 8	1290,3
- - 1	136,0	- - 10	676,8	- - 7	1307,7
27 : 0	148,7	- - 9	691,8	- - 6	1325,3
26 : 11	161,4	- - 8	706,8	- - 5	1342,7
- - 10	174,4	- - 7	721,9	- - 4	1360,4
- - 9	187,4	- - 6	737,1	- - 3	1378,1
- - 8	200,4	- - 5	752,5	- - 2	1396,1
- - 7	213,4	- - 4	766,6	- - 1	1413,9
- - 6	226,5	- - 3	783,0	20 : 0	1431,8

Height of the Barometer.	Height of the Places.	Height of the Barometer.	Height of the Places.	Height of the Barometer.	Height of the Places.
- - 5	239,7	- - 2	798,4	19 : 11	1449,8
- - 4	252,9	- - 1	813,9	- - 10	1467,9
- - 3	266,2	23 : 0	829,5	- - 9	1486,1
- - 2	279,6	22 : 11	845,0	- - 8	1504,4
- - 1	239,1	- - 10	860,7	- - 7	1522,8
26 : 0	306,6	- - 9	876,4	- - 6	1541,2
25 : 11	320,1	- - 8	892,2	- - 5	1559,7
- - 10	333,7	- - 7	908,0	- - 4	1578,3
- - 9	347,3	- - 6	924,0	- - 3	1597,0
- - 8	361,1	- - 5	940,0	- - 2	1615,7
- - 7	374,8	- - 4	956,1	- - 1	1634,5
- - 6	388,7	- - 3	972,2	19 : 0	1652,5
- - 5	402,5	- - 2	988,3	18 : 6	1768,0
- - 4	416,5	- - 1	1004,4	18 : 0	1887,4
- - 3	430,5	22 : 0	1020,8	17 : 6	2009,3
- - 2	444,6	21 : 11	1037,1	17 : 0	2134,8
- - 1	458,7	- - 10	1053,5	16 : 6	2264,0
25 : 0	472,8	- - 9	1069,9	16 : 0	2397,3
24 : 11	487,0	- - 8	1086,4	15 : 6	2534,9
- - 10	501,2	- - 7	1103,0	15 : 0	2677,0
- - 9	515,5	21 : 6	1119,7	14 : 6	2824,0
				14 : 0	2976,0

The obſervations of Mr. Lambert go upon the ſuppoſition that the refractive power of the atmoſphere is invariable; but this is by no means the caſe and therefore his rules muſt be conſidered as true for the mean ſtate of air only.

A moſt remarkable variety in the refractive power of the atmoſphere was obſerved by Dr. Nettleton, near Halifax in Yorkſhire, which demonſtrates how little we can depend upon the calculated heights of mountains, when the obſervations are made with an inſtrument, and the refractive power of the air is to be allowed for. Being deſirous to learn, by obſervation, how far the mercury would deſcend in the barometer at any given elevation (for which there is the beſt opportunity in that hilly country) he propoſed to take the height of ſome of their higheſt hills; but when he attempted it, he found his obſervation ſo much diſturbed by refraction, that he could come to no certainty. Having meaſured one hill, of a conſiderable height, in a clear day, and obſerved the mercury at the bottom and at the top, he found, according to that eſtimation, that about 90 feet, or more, were required to make the mercury fall $\frac{1}{10}$ of an inch; but afterwards, repeating the experiment on a cloudy day, when the air was rather groſs and hazy, he found the ſmall angles ſo much increaſed by refraction, as to make the hill much higher than before. He afterwards frequently made obſervations at his own houſe, by pointing a quadrant to the tops of ſome neighbouring hills, and obſerved that they would appear higher in the morning before ſun riſe, and alſo late in the evening, than at noon, in a clear day, by ſeveral minutes. In one caſe the elevations of the ſame hill differed more than 30 minutes. From this he infers that obſervations made on very high hills, eſpecially when viewed at a diſtance, and under ſmall angles, as they generally are, are probably uncertain, and not much to be depended upon.*

M. Euler conſidered with great accuracy the refractive power of the atmoſphere, as affected by different degrees of heat and elaſticity, in which he ſhews that its refractive power, to a conſiderable diſtance from the zenith, is ſufficiently near the proportion of the tangent of that diſtance, and that the law of refraction follows the direct ratio of the height of the barometer, and the inverſe ratio of the difference marked by the thermometer; but when ſtars are in the horizon, the changes are in a ratio ſomewhat greater than this, more eſpecially on account of the variation in the heat. §

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* Phil. Tranſ. ab. Vol. 6, p. 161.

§ Ac. Berlin, 1754, p. 132, 138.

As a chief cauſe of the *twinkling of the ſtars* is now generally acknowledged to be the unequal refraction of light, in conſequence of inequalities and undulations in the atmoſphere, I ſhall conclude this ſection with reciting ſuch obſervations as I have met with concerning this ſubject.

Mr. Michell ſuppoſes that the arrival of fewer or more rays at one time, eſpecially from the ſmaller, or the more remote fixed ſtars, may make ſuch an unequal impreſſion upon the eye, as may, at leaſt, have ſome ſhare in producing this effect; ſince it may be ſuppoſed that even a ſingle particle of light is ſufficient to make a ſenſible impreſſion upon the organs of ſight; ſo that very few particles arriving at the eye in a ſecond of time, perhaps not more than three or four, may be ſufficient to make an object conſtantly viſible. For though the impreſſion may be conſidered as momentary, yet the perception occaſioned by it is of ſome duration. Hence, he ſays, it is not improbable, that the number of the particles of light which enter the eye in a ſecond of time, even from Sirius himſelf, (the light of which does not exceed that of the ſmalleſt viſible fixed ſtar, in a greater proportion than that of about 1000 to 1) may not exceed three or four thouſand, and from ſtars of the ſecond magnitude they may, therefore, probably, not exceed an hundred. Now the apparent increaſe and diminution of the light, which we obſerve in the twinkling of the ſtars, ſeems to be repeated at not very unequal intervals, perhaps about four or five times in a ſecond. He, therefore, thought it reaſonable to ſuppoſe that the inequalities which will naturally ariſe from the chance of the rays coming ſometimes a little denſer, and ſometimes a little rarer, in ſo ſmall a number of them, as muſt fall upon the eye in the fourth or fifth part of a ſecond, may be ſufficient to account for this appearance. An addition of two or three particles of light, or perhaps a ſingle one, upon twenty, eſpecially if there ſhould be an equal deficiency out of the next twenty, would, he ſuppoſed, be very ſenſible, as he thought was probable from the very great difference in the appearance of ſtars, the light of which does not differ ſo much as is commonly imagined. The light of the middlemoſt ſtar in the tail of the great bear does not, he thinks, exceed the light of the very ſmall ſtar that is next to it in a greater proportion than that of about ſixteen or twenty to one, and M. Bouguer found that a difference in the light of objects of one part in ſixty-ſix was ſufficiently diſtinguiſhable.

It will perhaps, he ſays, be objected, that the rays coming from Sirius are too numerous to admit of a ſufficient inequality ariſing from the common effect of chance, ſo frequently as would be neceſſary to produce this effect, whatever might happen with reſpect to the ſmaller ſtars; but he obſerves that, till we know what inequality is neceſſary to produce this effect, we can only gueſs at it one way or the other.*

Since theſe obſervations were publiſhed, Mr. Michell has entertained ſome ſuſpicion that the unequal denſity of light does not contribute to this effect in ſo great a degree as he had imagined, eſpecially in conſequence of obſerving that even Venus does ſometimes twinkle. This he once obſerved her to do remarkably when ſhe was about 6 degrees high, though Jupiter, which was then about 16 degrees high, and was ſenſibly leſs luminous, did not twinkle at all. If, notwithſtanding the great number of rays, which, no doubt, come to the eye from ſuch a ſurface as this planet preſents, its appearance be liable to be affected in this manner, it muſt be owing to ſuch undulations in the atmoſphere, as will probably render the effect of every other cauſe altogether inſenſible. The conjecture, however, has ſo much probability in it that it well deſerved to be recited.

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* Phil. Tranſ. Vol. 57, p. 262.

M. Muſchenbroek ſuſpects that the twinkling of the ſtars ariſes from ſome affection of the eye, as well as the ſtate of the atmoſphere. For he ſays that in Holland, when the weather is froſty, and the ſky very clear, the ſtars twinkle moſt manifeſtly to the naked eye, though not in teleſcopes, and ſince he does not ſuppoſe that there is any great exhalation, or dancing of the vapour at that time, he queſtions whether the vivacity of the light affecting the eye may not be concerned in the phenomenon.*

But this philoſopher might very eaſily have ſatisfied himſelf with reſpect to this hypotheſis by looking at the ſtars near the Zenith, when the light traverſes but a ſmall part of the atmoſphere, and therefore might be expected to affect the eye the moſt ſenſibly. For he would not have perceived them to twinkle near ſo much, as they do nearer the horizon, when much more of their light is intercepted by the atmoſphere.

To ſhew my reader that every random conjecture, for the ſolution of natural appearances, has not been confined to the early periods of my hiſtory, I ſhall obſerve that ſome aſtronomers have lately endeavoured to explain the twinkling of the fixed ſtars by the extreme minuteneſs of their apparent diameter; ſo that they ſuppoſe the ſight of them is intercepted by every mote that floates in the air; but Mr. Michell obſerves that no object can hide a ſtar from us that is not large enough to exceed the apparent diameter of the ſtar, by the diameter of the pupil of the eye; ſo that if a ſtar was a mathematical point, the interpoſing object muſt ſtill be equal in ſize to the pupil of the eye.* I would add, that it muſt be large enough to hide the ſtar from both eyes at the ſame time.

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* Introductio, Vol. 2, p. 707.

Beſides a variation in the quantity of light, a momentary *change of colour* has likewiſe been obſerved in ſome of the fixed ſtars. Mr. Melville ſays that, when one looks ſtedfaſtly at Sirius, or any bright ſtar, not much elevated above the horizon, its colour ſeems not to be conſtantly white, but appears tinctured, at every twinkling, with red and blue.§ This obſervation Mr. Melville puts among his *queries*, with reſpect to which he could not intirely ſatisfy himſelf, and he obſerves that the ſeparation of the colours by the refractive power of the atmoſphere is, probably, too ſmall to be perceived. But the ſuppoſition of Mr. Michell above mentioned, will pretty well account for this circumſtance, though it may be thought inadequate to the former caſe. For the red and blue rays being much fewer than thoſe of the intermediate colours, and therefore much more liable to inequalities, from the common effect of chance, a ſmall exceſs or defect in either of them will make a very ſenſible difference in the colour of the ſtars.†

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[*] Introductio, Vol. 2, p. 262. [§] Edinburgh Eſſays, Vol. 2, p. 81.

[†] Phil. Tranſ. Vol. 57, p. 264.

SECTION V.

OBSERVATIONS RELATING TO THE COLOURS OF THIN PLATES, AND OTHERS SIMILAR TO THEM.

THERE are no optical experiments with which Sir Iſaac Newton ſeems to have taken more pains than thoſe relating to the *rings of colours* which appear in *thin plates*; and in all his obſervations and inveſtigations concerning them, he diſcovers the greateſt ſagacity, both as a philoſopher and mathematician; and yet in no ſubject to which he gave his attention does he ſeem to have overlooked more important circumſtances in the appearances he obſerved, or to have been more miſtaken with reſpect to their cauſes. The latter has, I preſume, in ſome meaſure, been made to appear already in the remarks on his hypotheſis of the *fits of eaſy reflexion and tranſmiſſion*, and the former will be evident from the obſervations of thoſe who ſucceeded him in theſe inquires, particularly thoſe of the Abbé Mazeas, to whom we are indebted for a paper upon this ſubject, firſt publiſhed in the Berlin Memoirs for 1752, and afterwards more correctly, and with enlargements in the *Memoires Preſentés*, Vol. 2.

This Gentleman, endeavouring to give a very high poliſh to the flat ſide of an object glaſs, happened to be rubbing it againſt another piece of flat and ſmooth glaſs, when he was ſurprized to find that, after this friction, they adhered very firmly together, till, at laſt, he could not move the one upon the other. But he was much more ſurprized to obſerve the ſame colours between theſe plane glaſſes that Newton obſerved between the convex object glaſs of a teleſcope, and another that was plane. Theſe colours between the plane glaſſes, the Abbé obſerves, were in proportion to their adheſion. *

The reſemblance between theſe colours and thoſe of Newton induced him to give a very particular attention to them, and his obſervations and experiments relating to them are ſuch as I think it my duty to lay them before my reader, with very little abridgement.

If the ſurfaces of the pieces of glaſs be tranſparent, and well poliſhed, ſuch as are uſed for mirrors, and the preſſure be as equal as poſſible on every part of the two ſurfaces, a reſiſtance, he ſays, will ſoon be perceived, when one of them is made to ſlide over the other, ſometimes towards the middle, and ſometimes towards the edges; but wherever the reſiſtance is felt, two or three very fine curve lines will be perceived, ſome of a pale red, and others of a faint green. Continuing the friction, theſe red and green lines increaſe in number at the place of contact, the colours being ſometimes mixed without any order, and ſometimes diſpoſed in a regular manner. In the laſt caſe, the coloured lines are generally concentric circles, or ellipſes, or rather ovals, more or

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* Memoires Preſentes, Vol. 2, p. 28.

leſs elongated, as the ſurfaces are more or leſs united. Theſe figures will not fail to appear if the glaſſes be well wiped, and warmed before the friction.

When the colours are formed, the glaſſes adhere with conſiderable force, and would always continue ſo, without any change in the colours. In the center of all theſe ovals, the longer diameter of which generally exceeds ten lines, there appears a ſmall plate, of the ſame figure, exactly like a plate of gold, interpoſed between the glaſſes; and in the center of it there is often a dark ſpot, which abſorbs all the rays of light, except the violet; for this colour appears very vivid through a priſm.

If the glaſſes be ſeparated ſuddenly, either by ſliding them horizontally over one another, or by the action of fire, as will be explained hereafter, the colours will appear immediately upon their being put together, without the leaſt friction.

Beginning by the ſlighteſt touch, and increaſing the preſſure by inſenſible degrees, there firſt appears an oval plate of a faint red, and in the center of it a ſpot of light green, which enlarges by the preſſure, and becomes a green oval, with a red ſpot in the center; and this, enlarging in its turn, diſcovers a green ſpot in its center. Thus the red and the green ſucceed one another in turns, aſſuming different ſhades, and having other colours mixed with them, which will be diſtinguiſhed preſently.

The greateſt difference between theſe colours exhibited between plane ſurfaces and thoſe by curve ones is, that, in the former caſe, preſſure alone will not produce them, except in the caſe above mentioned. With whatever force he compreſſed them, his attempts to produce the colours were in vain, without previous friction. But the reaſon of this plainly was, that without ſliding one of the glaſſes over the other, they could not be brought to approach near enough for the purpoſe.

Having made theſe obſervations with *plates* of glaſs, the ſides of which were parallel, or nearly ſo, he got two *priſms*, with very ſmall refracting angles; and rubbing them together, when they were ſo joined as to make a parallelopiped, the colours appeared with a ſurpriſing luſtre at the places of contact, owing, he did not doubt, to the ſeparation of the rays of light by the priſm. In this caſe differently coloured ovals appeared, but the plate of gold in them was much whiter, and was yellow only at its edges. This plate, having a black ſpot in its center, was bordered by a deep purple. He could not perceive any violet by his naked eye, but it might be perceived by the help of a lens, with a weak light. It appeared in a very ſmall quantity at the confines of the purple and the blue, and ſeemed to him to be only a mixture of thoſe two colours. It was very viſible in each of the coloured rings, by inclining the glaſſes to the light of the moon. Next to the purple and violet appeared blue, orange, red tinged with purple, light green, and faint purple. The other rings appeared to the naked eye to conſiſt of nothing but faint reds and greens, and they were ſo ſhaded, that it was not eaſy to mark their terminations.

Our author obſerved that theſe colours, notwithſtanding their luſtre, were not primitive ones, that is, ſuch as were ſeparated by the priſm. That he might obſerve the order of their colours, he ſuddenly ſeparated the two priſms, and after joining them again by ſimple appoſition, he raiſed the upper glaſs very gently, as Newton did with his object glaſſes, one of which was plano convex, belonging to a teleſcope of 14 feet, and the other convex on both ſides, belonging to a teleſcope of 50 feet.

That the order of theſe colours may be compared with thoſe of Newton, he gives a view of both in the following table.

Order of the colours of the plane glaſſes.		Order of the colours in Newton's object glaſſes.
Order 1.	Black ſpot Whitiſh oval Yellow border Deep purple	Black Blue White Yellow Red
Order 2.	Blue Orange Purple	Violet Blue Green Yellow Red
Order 3.	Greeniſh blue Yellowiſh green Purpled red	Purple Blue Green Yellow Red
Order 4.	Green Red	Green Red
Order 5.	Faint green Faint red	Greeniſh blue Red
Order 6.	Weak green Light red	Greeniſh blue Red
Order 7.	Very faint green Very faint red	Greeniſh blue Pale red

When theſe coloured glaſſes were ſuſpended over the flame of a candle, the colours diſappeared ſuddenly, though they ſtill adhered to one another, when they were parallel to the horizon. When they were ſuffered to cool, the colours returned by degrees to their former places, in the order of the preceding table.

After this, he took two plates, much thicker than the former, in order to obſerve at his leiſure the action of fire upon the matter which he ſuppoſed to produce the colours, and obſerved that, as they grew warm, the colours retired to the edges of the glaſſes, and there became narrower and narrower, till they were reduced to imperceptible lines. Withdrawing the flame, they returned to their place. This experiment he continued till the glaſſes were bent by the violence of the heat. It was pleaſant, he ſays, to obſerve theſe colours glide over the ſurface of the glaſs, as they were purſued by the flame. Theſe changes muſt have been owing to the glaſſes ſwelling, and the diſtance between them changing by the heat.

At firſt the Abbé Mazeas had no doubt but that theſe colours were owing to a thin plate of air between the glaſſes, to which Newton has aſcribed them; but the remarkable difference in the circumſtances attending theſe produced by the flat plates, and thoſe produced by the object glaſſes of Newton, convinced him that the air was not the cauſe of this appearance. The colours of the flat plates vaniſhed at the approach of flame, but thoſe of the object glaſſes did not. He even heated theſe till that which was next the flame was cracked by the heat, before he could obſerve the leaſt dilation of the coloured rings. This difference was not owing to the plane glaſſes being leſs compreſſed than the convex ones; for though the former were compreſſed ever ſo much by a pair of forceps, it did not in the leaſt hinder the effect of the flame.

Afterwards, he put both the plane glaſſes and the convex ones into the receiver of an air pump, ſuſpending the former by a thread, and keeping the latter compreſſed by two ſtrings; but he obſerved no change in the colours of either of them, in the moſt perfect vacuum that he could make.

Notwithſtanding theſe experiments ſeemed to be concluſive againſt the hypotheſis of theſe colours being cauſed by a plate of air, the Abbé frankly acknowledges, that the air may adhere ſo obſtinately to the ſurface of the glaſſes, as not to be ſeparated from them by the force of the pump; which, indeed, is agreeable to other appearances, but the following experiments of our author make it ſtill more improbable that the air ſhould be the cauſe of theſe colours.

To try the utmoſt effect of heat upon theſe coloured plates, after warming them gradually, he laid them on burning coals; but though they were nearly red, yet when he rubbed them together by means of a rod of iron, he obſerved the ſame coloured circles and ovals as before. When he ceaſed to preſs upon them, the colours ſeemed to vaniſh; but when he repeated the friction they returned, and continued till the pieces of glaſs began to be red hot, and their ſurfaces to be united by the fuſion.

When the outward ſurface of one of his plates of glaſs was quick-ſilvered, none of theſe colours were viſible, though the glaſſes adhered with the ſame force. This he aſcribed to the ſtronger impreſſion made upon the eye by the greater quantity of light reflected by the quick-ſilver.

Judging, from the reſemblance between his experiments and thoſe of Sir Iſaac Newton, that the colours were owing to the thickneſs of ſome matter, whatever it was, interpoſed between the glaſſes, the Abbé, in order to verify the hypotheſis, tried the experiment on thicker ſubſtances. He put between his glaſſes a little ball of ſuet, about a fourth of a line in diameter, and preſſed it between the two ſurfaces, warming them at the ſame time, in order to diſperſe the ſuet; but, though he rubbed them together as before, and likewiſe uſed other ſoft ſubſtances beſides ſuet, his endeavours to produce the colours had no effect. But rubbing them with more violence in a circular manner, he was ſurprized, on looking at a candle through them, to ſee it ſurrounded with two or three concentric greens very broad, and with very lively and delicate colours, namely, a red inclining to yellow, and a green like that of an emerald. At that time he obſerved only thoſe two colours, but continuing the friction, the rings aſſumed the colours of blue, yellow, and violet; eſpecially when he looked through the glaſſes on bodies directly oppoſed to the ſun. If, after having rubbed the glaſſes, the thickneſs was conſiderably diminiſhed, the colours grew weaker by tranſmitted light, but they ſeemed to be much ſtronger by reflexion, and to gain on one ſide what they loſt on the other.

Our author was confirmed in his opinion that there muſt be ſome error in Newton's hypotheſis, by conſidering that, according to his meaſures, the colours of the plates varied with the difference of a millionth part of an inch, whereas he was ſatisfied that there muſt have been much greater differences in the diſtance between his glaſſes when the colours remained unchanged.

If the colour depended upon the thickneſs only, he thought that the matter interpoſed between the glaſſes ought to have given the ſame colour, when it was reduced to a thin plate, by ſimple fuſion, as well as by friction, and that, in rubbing two plates together, warming them at different times, and compreſſing them with a conſiderable force, other colours would have appeared beſides thoſe obſerved before.

Theſe circumſtances made him ſuſpect that the different thickneſſes of the ſubſtance interpoſed between the glaſſes only ſerved to make it more or leſs tranſparent, which was an eſſential condition in the experiment; and he imagined that the friction diffuſed over the ſurface of the thin ſubſtance a kind of matter, on which the colours are formed by reflected light; for when he held the plates of glaſs (which gave the colours when the ſuet was between them) over the flame of a ſmall candle, the colours fled with great precipitation, and returned to their place, without his being able to perceive the leaſt alteration in the ſuet.

He was confirmed in his conjecture by frequently obſerving that when the glaſſes were ſeparated, at the moment that the colours diſappeared, they were covered with the ſame greaſy matter, and that it ſeemed to be in the very ſame ſtate as when they were ſeparated without warming. Beſides, having often repeated the ſame experiment with different kinds of matter, he found that the degree of heat that diſperſed the colours was not always ſufficient to melt it; which difference was more ſenſible, in proportion as the matter interpoſed was made thinner.

Inſtead of the ſuet, he ſometimes made uſe of Spaniſh wax, reſin, common wax, and the ſediment of urine. He began with Spaniſh wax, on account of its remarkable tranſparency in Mr. Haukſbee's electrical experiments; but he had much difficulty in making it ſufficiently thin by friction, being often obliged to warm his glaſſes, to ſeize the moment of fuſion, which continued but a ſhort time, and to hazard the burning of his fingers.

The experiment at length ſucceeding, the Spaniſh wax appeared with its opacity and natural colour when it reflected the light, but they both diſappeared in the tranſmitted light. He obſerved the ſame rings in it as in the ſuet, and indeed he could perceive but little difference between the colours of ſuet, Spaniſh wax, common wax, or reſin, except that this laſt ſubſtance did not make the colours ſo vivid, on account of the too great tranſparency of its particles.

The ſediment of urine had ſomething more particular in its appearance, as its colours were more lively. Holding it above the flame, its colours diſappeared; and keeping it in that ſituation, there were formed upon its ſurface ramifications, like thoſe of the hoar froſt, which diſappeared as the glaſſes grew cold. There were the ſame ramifications both upon the ſuet and the wax, but they were not ſo conſiderable. The glaſſes which had Spaniſh wax and reſin between them adhered with ſo much force, that they could not be ſeparated without the help of fire; and when they began to grow warm, they ſeparated with a noiſe like that of a glaſs breaking in the fire, though the glaſſes were not broken, and the matter between them was not melted.

Separating the glaſſes which he firſt uſed very ſuddenly, he obſerved upon their ſurface very thin vapours, which formed different colours, but preſently vaniſhed all together.

To try the effect of vapour, he breathed upon one of his plates of glaſs, and obſerved that the vapours which adhered to the glaſſes ſometimes formed, before they were intirely diſperſed, a ſurpriſing variety of colours. This experiment, he obſerves, does not always ſucceed at the firſt trial. The glaſs muſt be breathed upon ſeveral times, and care muſt be taken to wipe it every time with ones hand, both to take off the moiſture, which might otherwiſe detain the vapours, and alſo to make upon the glaſs a kind of furrows which contribute very much to the variety of colours, by making inequalities in the thickneſſes of the vapours. It is neceſſary alſo that the glaſſes on which theſe experiments are made have no quick-ſilver upon them.

When the particles of water which formed this vapour were too thick to exhibit theſe colours, he ſtruck them ſeveral times with his pencil, in order to attenuate them; and then he ſaw an infinity of ſmall coloured threads, which ſucceeded one another with great rapidity.

Putting a drop of water between two pieces of common glaſs, he obſerved that the compreſſion of them produced no colour; but if, while they were compreſſed, the water was made to paſs from one place to another, the water left behind it large ſpots, red, yellow, green, purple, &c. and the ſpots aſſumed different colours ſucceſſively, with a ſurpriſing rapidity, and preſented to the eye a moſt beautiful variety of ſhades.

In order to determine with greater certainty whether they were vapours that cauſed the colours in his firſt obſervations, he firſt breathed upon one of his plates of glaſs, and then rubbed them one againſt another, when the colours appeared in the ſame order as before, but darker, and diſperſed in confuſion in the places occupied by the vapours; but when he made uſe of fire to diſperſe the watery particles, the colours reſumed their luſtre.

Newton, having introduced a drop of water between his two object glaſſes, obſerved that, in proportion as the water inſinuated itſelf between the glaſſes, the colours grew fainter, and the rings were contracted; and aſcribing theſe colours to the thickneſs of the plate of water, as he aſcribed the former to the thickneſs of the plate of air, he meaſured the diameters of the coloured rings made by the plate of water, and concluded that the intervals between the glaſſes at the ſimilar rings of theſe two mediums were nearly as three to four, and thence he inferred that, in all caſes, theſe intervals would be as the ſines of the refraction of thoſe mediums.

The Abbé Mazeas, in order to aſſure himſelf whether, agreeably to this rule, the coloured rings of his glaſſes depended upon the thickneſs of the water only, dipped one of the edges of his coloured glaſſes in a veſſel of water, having taken care to wipe and warm them well before he produced the colours by friction. The water was a conſiderable time in riſing as high as the glaſſes, and in proportion as it aſcended, he perceived a very thin plate of water, which ſeemed to paſs over the matter which he thought produced the colours, without mixing with it; for beyond this plate of water, he ſtill perceived the colours in the ſame place and order, but deeper and darker; and holding the glaſſes above the flame of a candle, he ſaw the colours go and come ſeveral times as he moved them nearer to, or farther from the flame. He then moiſtened both the glaſſes more than before, and rubbing them as uſual, he always ſaw the ſame appearance, and ſeizing the moment when the colours had diſappeared, to ſeparate the glaſſes, he always found that they were wet. On this account he thought that it could not be the water on which the colour depended, but ſome ſubſtance much more ſenſible to heat. He alſo thought that theſe coloured rings could not be owing to the compreſſion of the glaſſes, or that, if this circumſtance did contribute any thing to them, it ſerved rather to modify than to generate them.

Our author concludes his ingenious paper with great propriety and modeſty, ſaying that he does not conſider the inductions he has drawn from his experiments as any more than conjectures, and that the theory of light thus reflected from thin plates is too delicate a ſubject to be compleatly aſcertained by a ſmall number of obſervations. *

M. Du Tour gave particular attention to the preceding obſervations of Abbé Mazeas. He repeated the experiments with ſome variation of circumſtances, particularly comparing them with thoſe of Sir Iſaac Newton, and adding ſome obſervations of his own.

He is ſo far from ſuppoſing that a plate of air is neceſſary to the appearance of theſe coloured rings, that he thinks the reaſon of their not appearing between the flat plates of glaſs is the adhering of the air to their ſurfaces, and that mere preſſure is not ſufficient to expel it, except, as the Abbé Mazeas obſerved, the rings had before been made in the ſame place, in which caſe, ſimple appoſition, without friction, is ſufficient; the air, probably, not having had time to apply itſelf ſo cloſely to the ſurface of the glaſs. The contact of ſome other ſubſtances, M. Du Tour obſerves, is not ſo prejudicial in this experiment as that of air; for he found that, if he only gave the plates a ſlight coating of any kind of greaſe, the rings would appear without friction. Alſo, dipping them ſlightly in water, or wiping them with his finger, would anſwer the ſame purpoſe. † Upon this I would obſerve that the greaſe and other ſubſtances being interpoſed between the glaſſes may bring their own powers of attraction and repulſion in the place of thoſe of the glaſs, and theſe may be more eaſily over-

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* Memoires Preſentés, Vol. 2, p. 43. † Ib. Vol. 4, p. 288.

come. When water, or any ſubſtance, is put between the glaſſes, the width of the rings muſt depend upon the difference between the intervals of the ſphere of attraction and repulſion belonging to both ſubſtances.

M. Du Tour verified his conjecture by means of the air pump; for dipping two pieces of glaſs in water, one of which had been wiped, and the other not, the former appeared to have no bubbles adhering to it, when the air was exhauſted, whereas the other had.

When one of the glaſſes is convex, our author obſerves, that the particles of air may more eaſily make their eſcape by preſſure only; whereas their retreat is in a manner cut off when they are compreſſed between two flat ſurfaces. The air pump, he found, was not able to detach theſe particles of air from the ſurfaces to which they adhere; leaving theſe flat plates for a conſiderable time in an exhauſted receiver was not ſufficient to prepare them for the experiment, ſo well as wiping them. *

M. Du Tour made many other obſervations on this ſubject, but hardly any of them are original, or of much value. If the flat glaſſes be obſerved by light tranſmitted through them, the rings, he ſays, appear to be very faintly coloured. If light ſeparated by a priſm be thrown upon the coloured flat plates, and afterwards reflected upon a piece of paſteboard, the white and the red rings will appear very bright; whereas the green and the blue, as well as the dark ſpot, will be very faint, and this will be the caſe whatever ſeparate rays be thrown upon them. If the coloured light be tranſmitted through them, the rings that appeared red by reflected light, were ſufficiently diſtinct; but

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* Memoires Preſentés, Vol. 4, p. 289.

thoſe which exhibited the fainter colours were not near ſo well defined as in the former obſervations.

This Gentleman had a very good contrivance to throw light upon the coloured plates at any given angle; and when no light fell upon them but in a certain angle, the colours were only viſible in an angle of reflexion equal to that of incidence; ſo that the rays that formed them are parallel to one another, having no divergence after reflexion; but they were viſible in every angle of incidence, though the colour of the ſame place changed with the angle.

The following obſervations of M. Muſchenbroeck, upon this ſubject of the colours of thin plates, well deſerve to be attended to, as they clearly ſhew that this remarkable appearance does not depend upon the air. If a little water was put betwixt the teleſcopic lenſes, when the coloured rings appeared between them, the outermoſt diſappeared, the reſt ſhrunk into a narrower ſpace, but by varying the preſſure of the lenſes, more rings made their appearance in various places, but the colours were not very diſcernible; becauſe, being formed in the water, the refraction of the rays iſſuing from the glaſs, he ſays is, leſs than from glaſs into air. Alſo the water being very ſtrongly attracted by the glaſs is often the reaſon why the plate is too thick for the rings to appear in it. But he ſays if a large drop of ſpirit of wine be firſt laid upon one of the lenſes, and then the other placed upon it, that when they are ſtrongly preſſed on both ſides with two fingers, ſo that the drop is much dilated, and the lenſes held obliquely with reſpect to the horizon, the preſſure of the upper fingers being relaxed, and that of the lower increaſed; then the ſpirit being ſtrongly attracted to the place where the interval between the glaſſes is the ſmalleſt, and deſcending, but not entirely leaving the ſurfaces, which it had before moiſtened, will leave many broad coloured rings, three inches in diameter. Of theſe rings there was not the leaſt trace when the drop was in the middle place between the lenſes; but by varying the preſſure of the upper and lower fingers, by which the intervals between the lenſes were ſometimes larger and ſometimes leſs, more rings came in view. Similar broad coloured rings, he ſays, ariſe, if a *lixivium* of black ſoap be poured between the upper edges of the lenſes, and then, when it has arrived at the middle place, the glaſſes be preſſed alternately by the upper and lower fingers. For where the lenſes are fartheſt aſunder, the lixivium adhering to the ſurfaces, the rings appear, while the reſt of the lixivium is flowing to the place where the preſſure is the greateſt.

It ſometimes happens that when the lenſes belong to teleſcopes, of an hundred feet in length, which have lain long dry, and one upon another, the coloured rings will not appear; but if they be well waſhed with ſpirit of wine, and then wiped with a linen cloth, the colours immediately appear in the place of contact. That the coloured rings did not appear without this waſhing and wiping, M. Muſchenbroeck imagined to ariſe from ſome impurities in the atmoſphere, which, in length of time, adhere to the ſurface of the glaſs; partly becauſe the ſalts which are uſed in the compoſition of the glaſs effloreſce a little, and make ſpecks, which can only be diſcovered by a very cloſe attention. On this account the ſurfaces of the two glaſſes have a covering, which keeps them too far aſunder for the appearance of theſe colours.* As to the cauſe of theſe coloured rings, he acknowledges, that he could not ſatisfy himſelf about it.†

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* Memoires Preſentés, Vol. 4, p. 724.

† Introductio, p. 738.

Mr. Mel-

Mr. Melville queries whether it be not poſſible to prove by experiment, what Sir Iſaac Newton takes for granted as a reaſonable ſuppoſition, that thin tranſparent plates of any uniform colour, divided into ſmaller fragments, would compoſe a powder of like colour. Would not this tend, he ſays, to ſtrengthen the analogy between the colours of ſuch plates, and thoſe of natural bodies. For this purpoſe he tried to freeze ſoap bubbles, but could never make any ſtand, till they were turned to ice, except ſuch as were too thick to have lively colours. However he did not doubt but that with due care, the thing might be done, eſpecially if the ſoap water, inſtead of being blown with a pipe into bubbles, was drawn out into a thin plate upon any wooden or metalline frame; for the ſides of a plane ſurface, bearing a greater proportion to its area than a baſe of the ſpherical ſegment to its ſurface, the froſt would be ſooner communicated to the whole water in the former caſe than in the latter. There is this advantage too, he obſerves, in uſing the plane ſurface of ſoap water, that before it freezes the obſerver may draw out any particular colour, or ſeries of colours, which he chuſes, to a greater breadth, by ſtroaking it along with a wet finger. For this reaſon amongſt others, he found it a more convenient ſubject for examining the various orders of colours than ſpherical bubbles, adhering to a plane. He thought melted reſin might be drawn out into a thin coloured plate before it hardens, for he often blew it into bubbles with a tobacco pipe, till it became coloured. He knew no other ways in which the various orders of colours could be preſerved for deliberate inſpection, but either in a frozen plate of water, or reſin, or in the permanent *ſcoriæ* that appear on heated metals. * He counted on the ſide of a clean poliſhed copper tea kettle the ſix firſt orders of colours, diſtinctly and regularly ranged, in the ſame ſucceſſion in which they appear in the ſoap bubbles, the firſt order being formed on that part of the kettle which had been leaſt heated.*

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* Perhaps glaſs blown ſo thin as to exhibit colours might have anſwered Mr. Melville's purpoſe better than any thing that he here mentions.

Has not gold, he ſays, been reduced by beating to a degree of thinneſs little exceeding that which muſt be aſcribed to its colorific parts, according to Sir Iſaac Newton's theory; but how can it cohere into a continuous leaf, ſo as to leave no viſible pores, unleſs there be many of its component particles contained in its thickneſs.†

Beſides the obſervations on the colours of *thin plates*, it has been ſeen that Sir Iſaac Newton imagined he could account for the colours exhibited by thick plates, in ſome caſes, in a ſimilar manner, particularly in thoſe curious experiments, in which he admitted a beam of light through a hole in a piece of paſteboard, and obſerved the rings of colours reflected back upon it, by a concave glaſs mirror of an equal thickneſs in all places. Theſe experiments were reſumed, and happily purſued by the Duke De Chaulnes, who aſcribed theſe colours to the inflection of light. As his obſervations on this ſubject are ingenious and plauſible, I ſhall preſent my reader with an account of them, and eſpecially with his drawings, as they may alſo be of uſe in giving a clearer idea of the experiments of Newton before recited, and which he did not illuſtrate in this manner.

Chance led the Duke de Chaulnes to obſerve that, when the nearer ſurface of the glaſs mirror was clouded, by breathing upon it, ſo as lightly to tarniſh it, a white, diffuſed, and vivid light was ſeen upon the paſteboard, and all the colours of the rings became much ſtronger and more diſtinct. This appearance he made conſtant by moiſtening the ſurface of the mirror with a little milk and water, and ſuffering it to dry upon it.

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* Introductio, p. 66.

† Edinb. Eſſays, Vol. 2, p. 81. This objection, and the doctrine of it, ſeem to be ſuperſeded by M. Boſcovich's hypotheſis of the penetrability of matter explained before.

In all his experiments upon this ſubject, he found that when the rays fell converging on the ſurface of the mirror, the rings were hardly viſible; when they fell parallel upon it, as they muſt have done in all the experiments of Newton, they appeared ſufficiently diſtinct; but when, by means of a convex lens placed in the hole of the window, they were made to diverge from the center of the ſphere to which the mirror was ground, ſo that they fell perpendicularly upon the ſurface of the mirror, the colours were as vivid as he could make them. In this caſe he could remove the reflected image to a great diſtance from the hole, without making the rings diſappear, and he could plainly perceive them to ariſe from their central ſpots, which changed their colours ſeveral times.

The effect of tarniſhing the mirror convinced the Duke De Chaulnes that theſe coloured rings depended upon the firſt ſurface of the mirror, and that the ſecond ſurface, or that which reflected them, after they had paſſed the firſt, only ſerved to collect them, and throw them upon the paſteboard in a quantity ſufficient to make them viſible, and he was confirmed in his ſuppoſition by the following experiments.

He took a plano-convex glaſs, of ſix feet focus, and placed it ſix feet from the paſteboard, with its convex ſide towards it, as in fig. 109. By this means the rays which fell upon that ſurface, after being refracted there, were tranſmitted through the thickneſs of the glaſs, parallel to one another, and fell perpendicularly upon the plane ſurface that reflected them, and in their return would be collected upon the paſteboard. In theſe circumſtances the rings appeared very diſtinct, after he had tarniſhed the convex ſurface, which, in this poſition, was next to the light.

Turning the ſame glaſs the contrary way, ſo that the plane ſurface was towards the paſteboard, he could perceive none of the rings, at the ſame diſtance of ſix feet; but they were viſible at the diſtance of three feet, becauſe, at that diſtance, the ſecond ſurface reflected the rays, by its concavity, directly towards the paſteboard, as in fig. 110.

Theſe two experiments demonſtrate the uſe of the ſecond ſurface of the mirror, and ſhew the manner of placing it to the moſt advantage. Thoſe that follow ſhew the uſe of the firſt ſurface, with reſpect to theſe rings, and he was led to make them by the caſual obſervation above mentioned.

Newton, he obſerves, had remarked that, when he made uſe of a mirror of the ſame focus with the firſt that he had uſed, but of twice the thickneſs, he found the diameter of the rings much ſmaller than before. This obſervation the Duke thought to be favourable to his own concluſions; for if theſe rings depend upon the firſt ſurface, the nearer it is to the ſecond, which only reflects the rays tranſmitted from it, the larger they ought to appear upon the paſteboard.

To aſcertain this fact, he thought of making uſe of two movable ſurfaces, and to make uſe of a micrometer, to meaſure the diſtance between them with exactneſs. For this purpoſe he took a metallic mirror, belonging to a reflecting teleſcope, being part of a ſphere of ten feet radius, and he fixed it firm upon a foot, in which was a groove, that carried a light frame, to which was faſtened a thin piece of talc, tarniſhed with milk and water. A view of this apparatus may be ſeen in fig. 111. The frame that ſupported the piece of talc could either be brought into contact with the mirror, or be removed to the diſtance of eight or nine inches from it, and the micrometer ſhewed, to the utmoſt exactneſs, the leaſt motion of the frame.

Having placed this mirror ten feet from the paſteboard, that is at the diſtance of the radius of its own ſphere, he obſerved the rings to appear very diſtinct, the form of his mirror being very true; but the diameter of the rings upon the paſteboard varied with the diſtance of the talc from the mirror, ſo that they were very large when the talc was near the mirror, and very ſmall when it was placed at the diſtance of ſeven or eight inches.

Theſe experiments proved that the rings were formed by the firſt ſurface, and reflected by the ſecond; but it ſtill remained to be determined in what manner they were formed. He imagined that the ſmall pencils of rays that were tranſmitted through the pores of the glaſs, or any other tranſparent ſubſtance, might ſuffer a kind of inflection, which might change the cylinder which they formed into a truncated cone, either by means of their different degrees of inflexibility, or by the different diſtances at which they paſs by the edges of the ſmall hole through which they are tranſmitted. Purſuing this idea, he thought of making uſe of ſome body, the pores of which were of a known and determinate ſhape. Inſtead, therefore, of the piece of talc, he placed a piece of fine muſlin in the above mentioned frame, ſtretching it as even as poſſible, to make the pores formed by the threads more exact and more permeable to the light; and he ſoon found, with great pleaſure, that his conjecture was verified; for inſtead of the circular rings which he had before, they were now manifeſtly ſquare, though their angles were a little rounded, and they were coloured as the others, though the light was not very vivid, on account of the quantity that was ſtopped by the muſlin.

When, inſtead of the muſlin, he ſtretched a croſs his frame fine ſilver wires, exactly parallel, at the diſtance of about three quarters of a line, or a whole line from one another, without any other wires acroſs them; inſtead of the rings, which he had ſeen before, there was nothing upon the paſteboard but a gleam of white light, divided by many ſmall ſtreaks, coloured in a very vivid manner, and in the ſame order as in the rings.

Laſtly, in order to ſimplify the experiment, and to bring it into a nearer compariſon with thoſe of Newton on the inflection of light, inſtead of the frame above-mentioned, he fixed nothing but the edge of a knife; and he had the ſame appearance as with the ſilver wires, except that the light was much leſs intenſe, though ſufficient to prove the indentity of the effect.

One of the moſt difficult things to account for in theſe experiments of the Duke De Chaulnes, is how the tarniſhing of the firſt ſurface ſhould make the colours ſo much more vivid, and the rings more numerous; and to this he thought it contributed on two accounts; firſt by diſperſing a part of the light which the firſt ſurface would have reflected, and which would have tended to efface the luſtre of the rings; and ſecondly by ſupplying, either by the ſmall particles of water, globules of milk, or ſome other ſimilar cauſe, a greater number of regular pores.

As he ſuppoſes that the ſecond ſurface only collects and reflects the light that is firſt inflected at the firſt ſurface, it might be thought that this purpoſe might be better anſwered by a convex lens, which would bring them to a focus on the other ſide; but he obſerves that, as the lens conſiſts of refracting materials, the colours that conſtitute the rings would be refracted at different angles, and ſo mixed together, that nothing but whiteneſs could be the reſult. *

SECTION VI.

OBSERVATIONS AND DISCOVERIES CONCERNING THE INFLECTION OF LIGHT.

THE ſame power that diſpoſes the light which actually falls upon bodies to be reflected from them, or refracted into them, affects the light which only paſſes very near to them; ſome of the rays being thereby made to bend from them, which is ſimilar to *reflexion*, and others to bend towards them, which is a kind of partial *refraction*. The experiments of Sir Iſaac Newton relate to both theſe modes of inflection; and had he purſued his inquiries into this ſubject, in the manner in which he ſeems to have intended to have done, he would probably have attended to them both in conjunction, and have compared their effects. Though the inveſtigations which he began cannot be ſaid to be completed, yet we are much obliged to ſeveral ingenious foreigners who have made experiments ſimilar to his; and by comparing them together, and diverſifying them a little more, it ſeems not to be difficult to arrive at a pretty ſatisfactory illuſtration of this ſubject.

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* Ac. Par. 1755, M. p. 201, &c.

The perſon whoſe name we find firſt upon the liſt of thoſe who purſued any experiments ſimilar to thoſe of Newton on inflected light is M. Maraldi, whoſe obſervations chiefly reſpect the inflection of light towards other bodies, whereby their ſhadows are partially illuminated; and many of the circumſtances which he noticed relating to it are well worthy of our attention, as the reader will be convinced from the following account of them.

He expoſed in the light of the ſun a cylinder of wood 3 feet long, and 6½ lines in diameter; when its ſhadow, being received upon a paper held cloſe to it, was every where equally black and well defined, and continued to be ſo to the diſtance of 23 inches from it. At a greater diſtance the ſhadow appeared to be of two different denſities; for the two extremities of the ſhadow, in the direction of the length of the cylinder, were terminated by two dark ſtrokes, a little more than a line in breadth. Within theſe dark lines there was a faint light, equally diſperſed through the ſhadow, which formed an uniform penumbra, much lighter than the dark ſtrokes at the extremity, or than the ſhadow received near the cylinder. This appearance is repreſented in fig. 112.

As the cylinder was removed to a greater diſtance from the paper, the two black lines continued to be nearly of the ſame breadth, and the ſame degree of obſcurity; but the penumbra in the middle grew lighter, and its breadth diminiſhed; ſo that the two dark lines at the extremity of the ſhadow approached one another, till, at the diſtance of 60 inches, they coincided, and the penumbra in the middle intirely vaniſhed. At a ſtill greater diſtance a faint penumbra was viſible, but it was ill defined, and grew broader as the cylinder was removed farther off, but was ſenſible at a very great diſtance.

Beſides the black and dark ſhadow, which the cylinder formed near the opaque body, a narrow and faint penumbra was ſeen on the outſide of the dark ſhadow. And on the outſide of this there was a tract more ſtrongly illuminated than the reſt of the paper.

The breadth of the external penumbra increaſed with the diſtance of the ſhadow from the cylinder, and the breadth of the tract of light on the outſide of it was alſo enlarged; but its ſplendor diminiſhed with the diſtance.*

He repeated theſe experiments with three other cylinders, of different dimenſions, and from them all he inferred, that every opaque cylindrical body, expoſed to the light of the ſun, makes a ſhadow which is black and dark to the diſtance of 38 to 45 diameters of the cylinder which forms it, and that, at a greater diſtance, the middle part begins to be illuminated in the manner deſcribed above. †

In explaining theſe appearances, our author ſuppoſes that the light which diluted the middle part of the ſhadow was occaſioned by the inflection of the rays, which, bending inwards, on their near approach to the body, did at a certain diſtance enlighten all the ſhadow, except the edges, which was left undiſturbed. At the ſame time other rays were deflected from the body, and formed a ſtrong light on the outſide of the ſhadow, and which might, at the ſame time, contribute to dilute the outer ſhadow, though he ſuppoſed that penumbra to be occaſioned principally by that part of the paper not being enlightened, except by a part of the ſun's diſk only, according to the known princples of opticks.*

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* Ac. Par. 1723, M. p. 159. † Ib. 161.

The ſame experiments he made with *globes*, of ſeveral diameters, but he found that, whereas the ſhadows of the cylinders did not diſappear but at the diſtance of 41 of their diameters, thoſe of the globes were not viſible beyond 15 of their diameters; which he thought was owing to the light being inflected on every ſide of a globe, and conſequently in ſuch a quantity, as to diſperſe the ſhadows ſooner than in the caſe of the cylinders.†

In all theſe caſes the penumbra, occaſioned by the inflected light, began to be viſible at a leſs diſtánce from the body in the ſtronger light of the ſun than in a weaker, on account of the greater quantity of rays inflected on thoſe circumſtances.

Conſidering the analogy between theſe experiments and the phenomena of an eclipſe of the moon, immerſed in the ſhadow of the earth, he imagined that part of the light by which ſhe is then viſible is inflected light, and not that which is refracted by the atmoſphere; though this may be ſo copious as to efface ſeveral of the above mentioned appearances, occaſioned by inflected light only.§ But this gentleman ſhould have conſidered ed, that, as no light is inflected but what paſſes exceedingly near to any body, perhaps ſo near as the diſtance of one fortieth part of an inch, this cauſe muſt be altogether inadequate to the effect.

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* Ac. Par. 1723, M. p. 164. † Ib. p. 169. § Ib. 173.

Being ſenſible that the above mentioned phenomena of the ſhadows were cauſed by inflected light, he was induced to give more particular attention to this remarkable property; and in order to it to repeat the experiments of Grimaldi and Sir Iſaac Newton in a darkened room. In doing this, he preſently obſerved that, beſides the enlarged ſhadow of a hair, a fine needle, &c. the bright gleam of light that bordered it, and the three coloured rings next to this enlightened part, when the ſhadow was at a conſiderable diſtance from the hair, the dark central ſhadow was divided in the middle by a mixture of light, and that it was not of the ſame denſity, except when it was very near the hair.*

This new appearance will be ſeen to be exactly ſimilar to what our philoſopher had obſerved with reſpect to the ſhadows in the open day light above mentioned; but the following obſervations, which he made with ſome variation of his apparatus, are much more curious and ſtriking, though they ariſe from the ſame cauſe.

Having placed a briſtle, which is thicker than a common hair, in the rays of the ſun, admitted into a dark chamber by a ſmall hole, at the diſtance of nine feet from the hole, it made a ſhadow, which being received at five or ſix feet from the object, he obſerved to conſiſt of ſeveral ſtreaks of light and ſhade. The middle part was a faint ſhadow, or rather a kind of penumbra, bordered by a darker ſhadow, and after that by a narrower penumbra, next to which was a light ſtreak broader than the dark part, and next to the ſtreak of light the red, violet, and blue colours were ſeen as in the ſhadow of the hair.

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* Ac. Par. 1723, M. p. 175.

In the ſame manner he placed, in the ſame rays of the ſun, ſeveral *needles* of different ſizes; but the appearances were ſo exceedingly various, though ſufficiently ſingular, that he does not recite them particularly, but chuſes rather to give, at ſome length, the obſervations he made on the ſhadows of two *plates*, as by that means he could better explain the phenomena of the round bodies.

He expoſed in the rays of the ſun, admitted by a ſmall hole into a dark chamber, a plate that was two inches long, and a little more than half a line broad. This plate being fixed perpendicularly to the rays, at the diſtance of nine feet from the hole, a faint light was ſeen uniformly diſperſed over the ſhadow, when it was received perpendicularly to it, and very near. The ſhadow of the ſame plate, being received at the diſtance of two feet and a half, was divided into four very narrow black ſtreaks, ſeparated by ſmall lighter intervals equal to them. The boundaries of this ſhadow on each ſide had a penumbra, which was terminated by a very ſtrong light, next to which were the coloured ſtreaks of red, violet, and blue as before. This is repreſented in fig. 113.

The ſhadow of the ſame plate, at 4½ feet diſtance from it, was divided into two black ſtreaks only, the two outermoſt having diſappeared, as in fig. 114; but theſe two black ſtreaks which remained were broader than before, and ſeparated by a lighter ſhade, twice as broad as one of the former black ſtreaks, when the ſhadow was taken at 2⅓ feet. This penumbra in the middle had a tinge of red. After the two black ſtreaks there appeared a pretty ſtrong penumbra, terminated by the two ſtreaks of light, which were now broad and ſplendid, after which followed the coloured ſtreaks.

A ſecond plate, two inches long, and a line broad, being placed like the former, fourteen feet from the hole, by which the rays of the ſun were admitted, its ſhadow being received perpendicularly, very near the plate, was illuminated by a faint light, equally diſperſed, as in the caſe of the preceding plate. But being received at the diſtance of thirteen feet from the plate, ſix ſmall black ſtreaks began to be viſible, as in fig. 115. At ſeventeen feet from the plate, the black ſtreaks were broader, more diſtinct, and more ſeparated from the ſtreaks that were leſs dark. At forty-two feet from the plate, only two black ſtreaks were ſeen in the middle of the penumbra, as in fig. 116. This middle penumbra, between the two black ſtreaks, was tinged with red. Next to the black ſtreaks there always appeared the ſtreaks of light, which were broad and the coloured ſtreaks next to them.

Receiving the ſhadow of the ſame plate at the diſtance of ſeventy-two feet, the appearances were the ſame as in the former ſituation, except that the two black ſtreaks were broader, and the interval between them, occupied by the penumbra, was broader alſo, and tinged with a deeper red.

In the ſame rays of the ſun he placed different plates, and larger than the former, one of them a line and a half, another two lines, another three lines broad, &c. but receiving their ſhadows upon paper, he could not perceive in them thoſe ſtreaks of faint light which he had obſerved in the ſhadows of the ſmall plates, though he received theſe ſhadows at the diſtance of fifty-ſix feet. Nothing was ſeen but a weak light, equally diffuſed, as in the ſhadows of the two ſmalleſt plates, received very near them. But had his dark chamber been large enough, he did not doubt but that, at a proper diſtance, there would have been the ſame appearances in the ſhadows of the larger plates, as in thoſe of the ſmalleſt. For the ſame reaſon he ſuppoſed that, if the ſhadows of the ſmall needles could have been diſtinctly viewed very near thoſe bodies, the different ſtreaks of light and ſhade would have been as viſible in them as in thoſe of the ſmall plates; and indeed he did obſerve the ſame appearances in the ſhadows of needles, of a middling ſize.*

The ſtreaks of light in theſe ſhadows our author aſcribed to the rays of light which are inflected at different diſtances from the bodies; and he imagined that their croſſing one another was ſufficient to account for the variations obſervable in them at different diſtances.

The extraordinary ſize of the ſhadows of theſe ſmall ſubſtances M. Maraldi thought to be occaſioned by the ſhadow from the enlightened part of the ſky, added to that which was made by the light of the ſun, and alſo to a *vortex*, occaſioned by the circulation of the inflected light behind the object; but my reader will probably not think it neceſſary for me either to produce all his reaſons for this hypotheſis, or to enter into a refutation of them.

Our author having made the preceding experiments upon ſingle long ſubſtances, had the curioſity to place two of them, ſo as to croſs one another in a beam of the ſun's light. The ſhadows of two hairs placed in this manner, and received at ſome diſtance from them, appeared to be painted reciprocally one upon the other, ſo that the obſcure part of one of them was viſible upon the obſcure part of the other. The ſtreaks of light alſo croſſed one another, and the coloured ſtreaks did the ſame.

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* Ac. Par. 1723, M. p. 176, &c.

Having placed a needle and a hair croſſing one another, their ſhadows, at the ſame diſtance, exhibited the ſame appearances as the ſhadows of the two hairs, though the ſhadow of the needle was the ſtronger.

He alſo placed in the rays of the ſun a briſtle and a plate of iron a line thick, ſo that they croſſed one another obliquely; and when their ſhadows were received at the ſame diſtance, the light and dark ſtreaks of the ſhadow of the briſtle were viſible ſo far as the middle of the ſhadow of the plate on the ſide of the acute angle, but not on the ſide of the obtuſe angle, whether the briſtle or the plate were placed next to the rays. The plate made a ſhadow ſufficiently dark, divided into ſix black ſtreaks; and theſe were again divided by as many light ones equal to them, and yet all the ſtreaks belonging to the ſhadow of the briſtles were viſible upon it, as in fig. 117. To explain this appearance, he ſuppoſed that the rays of the ſun glide a little along the briſtle, ſo as to enlighten part of that which is behind the plate. But this ſeems to be an arbitrary, and improbable ſuppoſition.

Our philoſopher did not fail to expoſe ſeveral ſmall globes in the light of the ſun in his dark chamber, and to compare their ſhadows with thoſe of the long ſubſtances, as he had done in the day light; and the appearances were ſtill ſimilar. It was particularly evident, that there was much more light in the ſhadows of the globes than in thoſe of the cylinders, not only when they were both of an equal diameter, but when that of the globe was larger than that of the cylinder, and the ſhadows of both the bodies were received at the ſame diſtance. He alſo obſerved, that he could perceive no difference of light in the ſhadows of the plates which were a little more than one line broad, though they were received at the diſtance of 72 feet; but he could eaſily ſee a difference of ſhades in thoſe of the globes, taken at the ſame diſtance, though they were $2\frac{1}{4}$ lines in diameter. *

In order to explain the colours at the edges of theſe ſhadows, he contrived to throw ſome of the ſhadows upon others; and the following obſervations, though they did not enable him to accompliſh what he intended, are curious and worth reciting.

Having thrown ſeveral of the ſimilar colours upon one another, and thereby produced a tinge more lively than before, he threw the gleam of light, which always intervened between the colours and the darker part of the ſhadow, upon different parts of other ſhadows; and obſerved that, when it fell upon the exterior penumbra made by another needle, it produced a beautiful ſky blue colour, almoſt like that which was produced by two blue colours thrown together. When the ſame gleam of light fell upon the deeper ſhadow in the middle, it produced a red colour, which ſeemed to prove that the reddiſh colour in the middle of ſeveral of the ſhadows might come from the little light inflected into that place. But here our author ſeems to have been miſled by ſome falſe hypotheſis concerning colours.

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* Ac. Par. 1723, M. p. 197.

He placed two plates of iron, each three or four lines broad, very near one another, but with a very ſmall interval between them; and having placed them in the rays of the ſun, and received their ſhadows at the diſtance of fifteen or twenty feet from them, he ſaw no light between them, but a continued ſhadow, in the middle of which were ſome ſtreaks of a lively purple, parallel to one another, and ſeparated by other black ſtreaks; but between them there were other ſtreaks, both of a very faint green, and alſo of a pale yellow. He alſo informs us that M. Deliſle had obſerved colours in the ſtreaks of light and ſhade, which are obſervable in ſhadows taken near the bodies.*

Our author concludes his curious paper with an account of the following experiment, which he repeated from Grimaldi. He introduced a beam of the ſun's light into a darkened chamber, by an aperture of about half an inch in diameter. At the diſtance of ſeven or eight feet from the hole, he placed in the light of the ſun a cylinder of wood or copper, an inch in diameter, in ſuch a manner, as that only part of the beam fell upon the cylinder. In this ſituation there was a reflexion of the rays of the ſun by the cylindrical body, and this reflexion made a ſemicircular train of light, the centre of which was in that part of the cylinder on which the image of the ſun fell. Having received part of this reflected light upon a piece of white paper, in any part of the ſemicircular ſpace, a great variety of lively colours were ſeen in it. Theſe colours were red, violet, yellow, blue, and green; ſo that the paper which received them, had the appearance of being marbled with thoſe different colours. In order to ſee them diſtinctly, it was neceſſary, however, to receive them at ſome diſtance from the image of the ſun.†

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* Ac. Par. 1723, M. p. 199. † Ib. p. 200.

Among thoſe who followed Sir Iſaac Newton in his obſervations on the inflection of light, we alſo find the ingenious M. MAIRAN; but, without attempting the diſcovery of new facts, he only endeavoured to explain the old ones, by the hypotheſis of an atmoſphere ſurrounding all bodies; and conſequently making two reflexions and refractions of the light that impinges upon them, one at the ſurface of the atmoſphere, and the other at that of the body itſelf. This atmoſphere he ſuppoſed to be of a variable denſity, and refractive power, like the air.

M. Mairan was ſucceeded by M. DU TOUR, who thought the variable atmoſphere ſuperfluous, and imagined that he could account for all the phenomena by the help of an atmoſphere of an uniform denſity, and of a leſs refractive power than the air ſurrounding all bodies. But what we are moſt obliged to this gentleman for is, not his ingenious *hypotheſis*, but the beautiful variety with which he has exhibited the *experiments*, which will render it much eaſier for any perſon to inveſtigate the true cauſes of them.

Before M. Du Tour gave his attention to this ſubject, only three fringes had been obſerved in the colours produced by the inflection of light; but he was accidentally led to obſerve a much greater number of them, and hit upon the following ingenious method of making them all appear very diſtinct.

He took a circular board, ABED, fig. 118, 13 inches in diameter, the ſurface of which was black, except at the edge, where there was a ring of white paper, about three lines broad, in order to trace the circumference of a circle, divided into 360 degrees, beginning at the point A, and reckoning 180 degrees on each hand to the point E; B and D being each of them placed at 90 degrees. A ſlip of parchment three inches broad, and diſpoſed in the form of a hoop, was faſtened round the board, and pierced at the point E with a ſquare hole, each ſide being four or five lines, in order to introduce a ray of the ſun's light. Laſtly, in the center of the board C, and perpendicular to it, he fixed a pin, about $\frac{1}{3}$ of a line in diameter.

This hoop being ſo diſpoſed, that a ray of light entering the dark chamber, through a vertical cleft of two lines and a half in length, and about as wide as the diameter of the pin, went through the hole at E, and paſſing parallel to the plane of the board, projected the image of the ſun and the ſhadow of the pin at A.

In theſe circumſtances he obſerved, 1ſt. that quite round the concave ſurface of this hoop, there were a multitude of coloured ſtreaks; but that the ſpace *m* A *n*, of about eighteen degrees, the middle of which was occupied by the image of the ſun, was covered with a faint light only.

2. The order of the colours in theſe ſtreaks was generally ſuch that the moſt refrangible rays were the neareſt to the incident ray ECA; ſo that, beginning from the point A, the violet was the firſt, and the red the laſt colour in each of the ſtreaks. In ſome of them, however, the colours were diſpoſed in a contrary order.

3. The image of the ſun projected on each ſide of the point A, was divided by the ſhadow of the pin, which was bordered by two luminous ſtreaks.

4. The coloured ſtreaks were narrower in ſome parts of the hoop than others, and generally decreaſed in breadth in receding from the point A.

5. Among theſe coloured ſtreaks, there were ſometimes others which were white, a line, or a line and an half in breadth, which

were always bordered on both ſides by a ſtreak of orange colour, at leaſt when the light of the ſun was intenſe, and the chamber ſufficiently dark.

From this experiment he thought it was evident, that the rays which paſſed beyond the pin were not the only ones that were decompoſed; for that thoſe which are reflected back from the pin were decompoſed alſo, from which he concluded, that they muſt have undergone ſome refraction. He alſo thought that thoſe which went beyond the pin ſuffered a reflexion, ſo that they were all affected in a ſimilar manner.*

In order to account for theſe facts, our author deſcribes the progreſs of a ray of light through an uniform atmoſphere, which he ſuppoſes to ſurround the pin, and ſhews that the differently refrangible rays will be ſeparated at their emergence from it; but he refers to ſome experiments and obſervations in a future memoir, to demonſtrate that all the coloured ſtreaks are produced by rays that are both reflected and refracted. †

To give ſome idea of his hypotheſis, he ſhews that the ray *a b*, fig. 119, after being refracted at *b*, reflected at *r* and *u*, and again refracted at *s* and *t*, will be divided into its proper colours; the leaſt refrangible, or the red rays, iſſuing at *x*, and the moſt refrangible, or violet at *y*, which agrees with his obſervations. Thoſe ſtreaks in which the colours appear in a contrary order he thinks are to be aſcribed to inequalities in the ſurface of the pin. This might eaſily have been aſcertained by turning the pin round, in which caſe theſe differently coloured ſtreaks would have changed their places.

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* Memoires Preſentés Vol. 5, p. 636.

† Ib. p. 641.

If any perſon ſhould chuſe to repeat theſe experiments of the hoop, he obſerves that it requires that the ſky be very clear and free from vapours, in order to exhibit the colours with the greateſt diſtinctneſs; ſince even the vapours that are imperceptible, will diminiſh the luſtre of the colours on every part of the hoop, and even efface ſome of them, eſpecially thoſe which are on that part in which the beam of light enters, as at E, where the colours are always fainter than in any other place, and indeed can never be diſtinguiſhed except when the hole E is confined by black ſubſtances, ſo as to intercept a part of the light that might reach the pin; and unleſs, alſo, thoſe rays which go beyond the pin, to form the image of the ſun at A, be ſtopped, ſo that no rays are viſible except thoſe that are reflected towards the hole, and which make the faint ſtreaks.

The coloured ſtreaks that are next the ſhadow of the pin, he ſhews, are formed by thoſe rays which, entering the atmoſphere, do not fall upon the pin, and, without any reflexion, are only refracted at their entering and leaving the atmoſphere, as at *b* and *r u*, fig. 120. In this caſe the red or leaſt refrangible rays will iſſue at *r*, and the violet at *u*.

To diſtinguiſh the rays which fell upon the hoop in any particular direction, from thoſe that came in any other, he made an opening in the hoop, as at P, by which means he could with advantage, and at any diſtance from the center, obſerve thoſe rays unmixed with any other.*

To account for the coloured ſtreaks being larger next the ſhadow of the pin, and growing narrower to the place where the light was admitted, he ſhews, by fig. 121, that the rays *a b* are farther ſeparated by both the refractions than the rays *c d*.

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* Memoires de Mathem. et de Phyſ. Vol. 5, p. 650.

Sometimes our author obſerved, that the broader ſtreaks were not diſpoſed in this regular order; but then he found that, by turning the pin, they changed their places, ſo that this circumſtance muſt have been an irregularity, depending upon the accidental ſurface of the pin.*

The white ſtreaks intermixed with the coloured ones he aſcribes to ſmall cavities in the ſurface of the pin, or ſome other foreign circumſtance; for they alſo changed their places when the pin was made to turn upon its axis.†

Other obſervations of our author ſeem to prove that the refracting atmoſpheres ſurrounding all kinds of bodies are of the ſame ſize; for when he placed a great variety of ſubſtances, and of different ſizes alſo, he always found the coloured ſtreaks of the ſame dimenſions.§

M. Du Tour obſerves that his hypotheſis contradicts an obſervation of Sir Iſaac Newton, that thoſe rays which paſs the neareſt to any body, are the moſt inflected; but he thinks that Newton's obſervations were not ſufficiently accurate. Beſides, he obſerves that Newton only ſaid that *he thought it to be ſo*, without aſſerting it poſitively.‡

Since the rays which formed theſe coloured ſtreaks are but little diverted out of their way, our author infers that this atmoſphere is of ſmall extent, and that its refractive power is not much leſs than that of air.||

Expoſing two pieces of paper in the beam of light, ſo that part of it paſſed between two planes formed by them, M. Du Tour obſerved that the edges of this light, received upon paper, were bordered with two orange coloured ſtreaks, which Newton had not taken notice of in any of his experiments.* To account for them, he ſuppoſes that, in fig. 122, the more refrangible of the rays, which enter at *b*, are ſo refracted, that they do not reach the ſurface of the body itſelf, at R; ſo that the red and orange coloured light may be reflected from thence in the direction *d* M, where the orange coloured ſtreaks will be formed; and, for the ſame reaſon, another ſtreak of orange will be formed at *m*, by the rays which enter the atmoſphere on the other ſide of the chink. In a ſimilar manner he accounts for the orange coloured fringes at the borders of the white ſtreaks, in the experiment of the hoop. †

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* Memoires Preſentés, Vol. 5, p. 653. † Ib. p. 654. § Ib. p. 655. ‡ Ib. p. 656. || Ib. p. 657.

The blue rays, which are not reflected at R, he ſuppoſes paſs on to I, and that, of theſe rays, the blue tinge obſervable in the ſhadows of ſome bodies are formed.

To explain an experiment of Newton's, ſimilar to another of his own, in which two ſtreams of light were darted from the edges of bodies placed in a beam of the ſun, he ſuppoſes that one of them is made by light reflected by the edge of the knife itſelf, and the other by the inſide of the atmoſphere that ſurrounds it, ſo that the rays which formed them croſſed one another, as in fig. 123.

He found, by placing bodies on the outſide of his darkened room, that the rays P *d* and P *e* came from the part *a* P of the incident ray of light, and that the rays *r f*, *r g* came from the part *b r* of the ray.§

Drawing a piece of paper acroſs the hole in his window ſhutter, he obſerved that when the chink by which the light was admitted was exceedingly narrow, the image of the ſun made by the light that was admitted through it became conſiderably extended, and that it was bordered firſt by the orange coloured ſtreaks, then thoſe coloured in the uſual manner, and then by a faint gleam of light, which grew weaker and weaker as it receded from the image.

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* Memoires Preſentés, Vol. 5, p. 666. † Ib. p. 672. § Ib. 675.

This dilitation of the image, to places where it had not reached when the hole was wider, he aſcribed to the compreſſion of the atmoſpheres by the paper on one ſide, and the wood on the other; by which means they were made flatter than before, ſo that the rays, falling upon them more obliquely, were thrown farther out of their natural courſe than they would otherwiſe have been.* This he particularly explains by fig. 124, where the ray *a b*, paſſing through the ſpherical atmoſphere, would iſſue at D; but paſſing through the elliptical one, would iſſue at *d*. Upon this I would obſerve, that the ſuppoſition of the atmoſpheres compreſſing one another is unneceſſary for this purpoſe. If atmoſpheres be ſpheres in which certain powers take place, they will, when they intermix, increaſe or leſſen each others effects, according as they are of the ſame, or a contrary nature.

This author promiſes a fuller explication of theſe experiments, and alſo an account of others ſimilar to them, in a future memoir.

M. Le Cat has well explained a phenomenon of viſion depending upon the inflection of light, which ſhews that, in ſome caſes, objects appear magnified by this means. Looking at a diſtant ſteeple, when a wire, of a leſs diameter than the pupil of his eye, was held pretty near to it, and drawing it ſeveral times betwixt his eye and that object, he was ſurprized to find that, every time the wire paſſed before his pupil, the ſteeple ſeemed to change its place, and ſome hills beyond the ſteeple ſeemed to have the ſame motion, juſt as if a lens had been drawn betwixt his eye and them.

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* Memoires Preſentés, Vol. 5, p. 676.

Examining this appearance more attentively, he found that there was a poſition of the wire, but very difficult to keep, in which the ſteeple ſeemed not to have any motion, when the wire was paſſed before his eye; and in this caſe the ſteeple appeared leſs diſtinctly, and ſeemed to be magnified. Theſe effects being ſimilar to thoſe of a lens, he attended to them more particularly, and placed his eye in ſuch a manner, with reſpect to the ſteeple, that the rays of light by which he ſaw it muſt come very cloſe to the edge of a window, where he had placed himſelf to make his obſervations. Then paſſing the wire once more before his eye, he obſerved, that when it was in the viſual axis the ſteeple appeared nearer to the window, on which ever ſide the wire was made to approach. He repeated this experiment, and conſtantly with the ſame reſult, the object being always magnified, and nearly doubled by this means.*

This phenomenon is eaſily explained by fig. 125, in which B repreſents the eye, A the ſteeple, and C the diameter of the wire. The black lines expreſs the cone of light by which the natural image of the ſteeple A is formed, and which is much narrower than the diameter of the wire C; but the dotted lines include not only that cone of light, ſtopped and turned out of its courſe by the wire, but alſo more diſtant rays, inflected by the wire, and thereby thrown more converging into the pupil; juſt as would have been the effect of the interpoſition of a lens between the eye and the object. The reſult of this experiment was the ſame, whatever ſubſtances he made uſe of in the place of the wire, provided they were of the ſame diameter.

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* Traité des Sens, p. 299.

This diſcovery, depending upon the inflection of the rays of light, led him to ſeveral others depending upon the ſame principle. Thus he magnified ſmall objects, as the head of a pin, by looking at them through a ſmall hole in a card; ſo that the rays which formed the image muſt neceſſarily paſs ſo near the circumference of the hole, as to be attracted by it. He alſo obſerved that, by bringing his finger near the cone of light, which tranſmitted to him the image of any object, well inſulated, as a red coal in the midſt of cinders, or a black coal in the midſt of the fire, the object ſeemed to ſtretch itſelf towards his finger, as it approached, and to follow it to a certain diſtance when it was withdrawn. He thought it was owing to the ſame cauſe, that the clouds which paſs over the face of the ſun, occaſion various motions in the ſhadows of bodies; and when theſe clouds are interrupted in different places, thoſe ſhadows ſeem to dance, which is particularly viſible in the ſhadows made by the leads in glaſs windows. It was to the ſame inflection of light that he aſcribed, in part, the priſmatic colours which he ſaw by means of a very fine pin, placed near to his eye, and on which he made the light of a candle to fall obliquely.*

I ſhall conclude this ſection with an obſervation of M. Muſchenbroeck, that if, in the night, a braſs wire, or a ſmall needle, be held between the eye and a candle, and the wire be moved ſometimes nearer to, and ſometimes farther from the candle, there is a place in which the wire will appear tranſparent, and of a reddiſh colour. This, he thinks, may be owing to the inflection of the light, by the ſides of the wire.* But in fact this may only be a partial ſhadow made by the pin upon the eye.

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* Traité des Sens, p. 304.

SECTION VII.

OF THE MEASURES OF LIGHT.

THE ingenious obſervations that have been made by philoſophers concerning the *meaſures of light*, in a variety of caſes, are ſo numerous, ſince the time of Sir Iſaac Newton, that they deſerve to be diſpoſed in a ſeparate ſection. That ſome luminous bodies give a ſtronger, and others a weaker light; and that ſome reflect more light than others, was always obvious to all mankind; but no perſon hit upon any method of aſcertaining the proportion that two or more lights bear to each other, with any tolerable accuracy, before M. Bouguer, who applied ſome exquiſite contrivances to this purpoſe, and who, in his application of them, was led to a variety of important philoſophical diſcoveries, which have been already recited. As his obſervations on this ſubject deſerve the chief place in this ſection, I ſhall introduce them by giving an idea of the methods that he uſed to meaſure the proportion of different lights. Thoſe which he moſt commonly uſed were the following.

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* Introductio, Vol. 2, p. 779.

He took two pieces of wood or paſteboard EC and CD, fig. 126, in which he made two equal holes, P and Q, over which he drew pieces of oiled, or white paper. Upon theſe holes he contrived that the light of the different bodies he was comparing ſhould fall, while he placed a third piece of paſteboard, FC to prevent the two lights from mixing with one another. Then placing himſelf ſometimes on one ſide and ſometimes on the other, but generally on the oppoſite ſide of this inſtrument, with reſpect to the light, he altered their poſition, till the papers in the two holes appeared to be equally enlightened. This being done, he computed the proportion of their light by the ſquares of the diſtances at which the luminous bodies were placed from the objects. If, for inſtance, the diſtances were as three and nine, he concluded that the light they gave were as nine and eighty-one. Where any light was very faint, he ſometimes made uſe of lenſes, in order to condenſe it, and he incloſed them in tubes or not, as his particular application of them required.

To meaſure the intenſity of light proceeding from the heavenly bodies, or reflected from any part of the ſky, he contrived an inſtrument which reſembles a kind of portable Camera Obſcura. He had two tubes, of which the inner was black, faſtened at their lower extremities by a hinge, C. fig. 127. At the bottom of theſe tubes were two holes, R and S, three or four lines in diameter, covered with two pieces of fine white paper. The two other extremities had each of them a circular aperture, an inch in diameter; and one of the tubes conſiſted of two, one of them ſliding into the other, which produced the ſame effect as varying the aperture at the end. When this inſtrument is uſed, the obſerver has his head, and the end of the inſtrument, C, ſo covered, that no light can fall upon his eye, beſides that which comes through the two holes S and R, while an aſſiſtant manages the inſtrument, and draws out or ſhortens the tube D E, as the obſerver directs. When the two holes appear equally illuminated, the intenſity of the lights is judged to be inverſely as the ſquares of the lengths of the tubes.

In uſing this inſtrument, it is neceſſary that the object ſhould ſubtend an angle larger than the aperture A or D, ſeen from the other end of the tube; for, otherwiſe, the lengthening of the tube has no effect. To avoid, in this caſe, making the inſtrument of an inconvenient length, or making the aperture D too narrow, he has recourſe to another expedient. He conſtructs an inſtrument, repreſented fig. 128, conſiſting of two object glaſſes, AE and DF, exactly equal, fixed in the ends of two tubes ſix or ſeven feet, or, in ſome caſes, ten or twelve feet long, and having their foci at the other ends. At the bottom of theſe tubes, B are two holes, three or four lines in diameter, covered with a piece of white paper; and this inſtrument is uſed exactly like the former.

If the two objects to be obſerved by this inſtrument be not equally luminous, the light that iſſues from them muſt be reduced to an equality, by diminiſhing the aperture of one of the object glaſſes; and then the remaining ſurface of the two glaſſes will give the proportion of their lights. But, for this purpoſe, the central parts of the glaſs muſt be covered, in the ſame proportion with the parts near the circumference, leaving the aperture ſuch as is repreſented fig. 129; becauſe the middle part of the glaſs is thicker, and leſs tranſparent than the reſt.

If all the objects to be obſerved lie nearly in the ſame direction, our author obſerves that theſe two long tubes may be reduced into one, the two object glaſſes being placed cloſe together, and one eye glaſs ſufficing for them both. The inſtrument will then be the ſame with that of which he publiſhed an account in 1748, and which he called a heliometer, or aſtrometer.*

Our author obſerves that it is not the abſolute *quantity*, but only the *intenſity* of the light that is meaſured by theſe two inſtruments, or the number of rays, in proportion to the ſurface of the luminous body; and it is of great importance that theſe two things be diſtinguiſhed. The intenſity of light may be very great, when the quantity, and its power of illuminating other bodies, may be very ſmall, on account of the ſmallneſs of its ſurface; or the contrary may be the caſe, when the ſurface is large. †

Having explained theſe methods which M. Bouguer took to meaſure the different proportions of light, I ſhall ſubjoin, in this place, a few miſcellaneous examples of his application of them; the reſult of his moſt important experiments having been already recited under the heads to which they reſpectively belong.

It is obſervable, that when a perſon ſtands in a place where there is a ſtrong light, he cannot diſtinguiſh objects that are placed in the ſhade; nor can he ſee any thing upon going immediately into a place where there is very little light. It is plain, therefore, that the action of a ſtrong light upon the eye, and alſo the impreſſion which it leaves upon it, makes it inſenſible to the effect of a weaker light. M. Bouguer had the curioſity to endeavour to aſcertain the proportion between the intenſities of the two lights in this caſe; and by throwing the light of two equal candles upon a board, he found that the ſhadow made by intercepting the light of one of them, could not be perceived by his eye, upon the place enlightened by the other, at little more than eight times the diſtance; from whence he concluded that when one light is eight times eight, or 64 times leſs than another, its preſence or abſence will not be perceived. He allows, however, that the effect may be different on different eyes, and ſuppoſes that the boundaries in this caſe, with reſpect to different perſons, may lie between 60 and 80.*

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* Traité de Optique, p. 37. † Ib. p. 40.

Applying the two tubes of his inſtrument, mentioned above, to meaſure the intenſity of the light reflected from different parts of the ſky; he found that when the ſun was 25 degrees high, the light was four times ſtronger at the diſtance of eight or nine degrees from his body, than it was at 31 or 32 degrees. But what ſtruck him the moſt was to find that, when the ſun is 15 or 20 degrees high, the light decreaſes on the ſame parallel to the horizon to 110 or 120 degrees, and then increaſes again to the place exactly oppoſite to the ſun.§

The light of the ſun, our author obſerves, is too ſtrong, and that of the ſtars too weak, to determine the variation of their light at different altitudes; but as, in both caſes, it muſt be in the ſame proportion with the diminution of the light of the moon, in the ſame circumſtances, he made his obſervations on that luminary, and found that its light at 19° 16′, is to its light at 66° 11′, as 1681 to 2500; that is, the one is nearly two thirds of the other. He choſe thoſe particular altitudes, becauſe they are thoſe of the ſun at the two ſolſtices at Croiſic, where he then reſided. When one limb of the moon touched the horizon of the ſea, its light was 2000 times leſs than at the altitude of 66° 11′. But this proportion he acknowledges muſt be ſubject to many variations, the atmoſphere near the earth varying ſo much in its denſity.* From this obſervation he concludes that, at a medium, light is diminiſhed in the proportion of about 2500 to 1681, in traverſing 7469 toiſes of denſe air.†

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* Traité de Optique, p. 52.

§ Ib. p. 73.

Laſtly, our accurate philoſopher applied his inſtrument to the different parts of the ſun's diſk, and found that the center is conſiderably more luminous than the extremities of it. As near as he could make the obſervation, it was more luminous than a a part of the diſk $\frac{3}{4}$ths of the ſemi-diameter from it, in the proportion of 28 to 35; which, as he obſerves, is more than in the proportion of the ſines of the angles of obliquity.§ On the other hand, he obſerves that, both the primary and ſecondary planets are more luminous at their edges than near their centers.‡

The compariſon of the light of the ſun and moon is a ſubject that has frequently exerciſed the thoughts of philoſophers; but we find nothing but random conjectures, before our author applied his accurate meaſures in this caſe. In general, the light of the moon is imagined to bear a much greater proportion to that of the ſun than it really does; and not only are the imaginations of the vulgar, but thoſe of philoſophers alſo impoſed upon with reſpect to it. It was a great ſurpriſe to M. De la Hire to find that be could not, by the help of any burning mirror, collect

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[*] Traité de Optique, p. 81. [†] Ib. p. 85. [§] Ib. p. 93. [‡] Ib. p. 121.

the beams of the moon in a ſufficient quantity to produce the leaſt ſenſible heat.* Other philoſophers have ſince made the like attempts with mirrors of greater power, though without any greater ſucceſs; but this will not ſurprize us, when we ſee the reſult of M. Bouguer's obſervations on this ſubject.

In order to ſolve this curious problem, concerning the comparison of the light of the ſun and moon, he compared each of them to that of a candle in a dark room, one in the day time, and the other in the night following, when the moon was at her mean diſtance from the earth; and, after many trials, he concluded that the light of the ſun is about 300000 times greater than that of the moon; which is ſuch a diſproportion, that, as he obſerves, it can be no wonder that philoſophers have had ſo little ſucceſs in their attempts to collect the light of the moon with burning glaſſes. For the largeſt of them will not increaſe the light 1000 times, which will ſtill leave the light of the moon, in the focus of the mirror, 300 times leſs than the intenſity of the common light of the ſun.

To this account of the proportion of the light which we actually receive from the moon, it cannot be diſpleaſing to my reader if I compare it with the quantity which would have been tranſmitted to us from that opaque body, if it reflected all the light it receives. Dr. Smith thought that he had proved, from two different conſiderations, that the light of the full moon would be to our day light as one to about 90900, if no rays were loſt at the moon.

In the firſt place he ſuppoſes that the moon, enlightened by the ſun, is as luminous as the clouds are at a medium. He therefore ſuppoſed the light of the ſun to be equal to that of a whole hemiſphere of clouds, or as many moons as would cover the ſurface of the heavens.* But upon this I would obſerve that, it is true that the light of the ſun, ſhining perpendicularly upon any ſurface, would be equal to the light reflected from the whole hemiſphere, if every part of it reflected all the light that fell upon it; but the light that would, in fact, be received from the whole hemiſphere (part of it being received obliquely) would be only one half as much as would be received from the whole hemiſphere, if every part of it ſhone directly upon the ſurface to be illuminated.

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* Ac. Par. 1705, M. 455.

In his *Remarks*, p. 17, he draws the ſame concluſion from a different method of induction; but in this caſe alſo he made a miſtake of one half, ſuppoſing all the enlightened hemiſphere of the moon to receive the direct rays of the ſun; whereas, in fact, no more can be received than would fall perpendicularly on the ſuperficial ſection of one great circle, which is juſt one half of the ſurface of the hemiſphere.

Mr. Michell made this computation in a much more ſimple and eaſy manner, and in which there is much leſs danger of falling into any miſtake. Conſidering the diſtance of the moon from the ſun, and that the denſity of the light muſt decreaſe in the proportion of the ſquare of that diſtance, he calculated the denſity of the ſun's light, at that diſtance, in proportion to its denſity at the ſurface of the ſun; and in this manner he found that, if the moon reflected all the light it receives from the ſun, it would only be the 45000th part of the light we receive from that greater luminary. Admitting, therefore, with M. Bouguer, that the moon reflects only a 300000th part of it, Mr. Michell concludes, that it reflects no more than between the 6th and 7th part of the light that falls upon it.*

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* Opticks, Vol. 1, p. 29.

SECTION VIII.

Of the properties of ISLAND CRYSTAL.

BESIDES thoſe properties of the rays of light, by which they are diſpoſed to be reflected, refracted, and inflected, they may be poſſeſſed of many other properties, reſpecting bodies, independent of theſe; by which means the mutual action between light and bodies may be exceedingly various. Some new and remarkable properties of light have been diſcovered by means of *Iſland cryſtal*; for the plain inference from the experiments which have been made on the refractive power of this extraordinary foſſil is, that different ſides of the rays are poſſeſſed of different properties. The facts which led the way to this diſcovery were firſt noticed by Eraſmus Bartholin, and the greateſt number of obſervations were made by M. Huygens; but ſince the probable cauſe of them was not ſuggeſted before Sir Iſaac Newton, and ſome obſervations on the ſubject have been made ſince his time, I have choſen to give the whole hiſtory in one view, and to place it in the preſent period.

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* Phil. Tranſ. Vol. 53, p. 236.

Bartholin obſerves that the figure of the Iſland cryſtal is rhomboidal, and that its parts, when it is broken, are of the ſame figure, excepting one caſe, in which it exhibits a triangular pyramid.* Meaſuring the angles of this ſubſtance, which is repreſented fig. 130 he found the obtuſe angle to be 101 degrees, and the acute 79°.

Placing the baſe of a piece of this cryſtal upon the object B or A, fig. 130; and looking at it through the whole thickneſs of the piece, with his eye above it, he was ſurprized, he ſays, to obſerve two diſtinct images of them; B appearing both at G and H, and A at CD and EF. The diſtance between theſe two images of the ſame object was greater in proportion to the thickneſs of the piece of cryſtal, through which it was viewed, ſo that, in very thin pieces, the images nearly coincided.† Looking intently upon two images of the ſame object, he conceived that that which was farther from his eye was more elevated than the other.

In one poſition of the eye, he found that the object appeared ſingle, as through any other pellucid medium, and in ſome other poſitions he perceived no leſs than ſix images; but this he conjectured was when the images were reflected from the ſides of the cryſtal, ſo as to reach the eye at the ſame time with thoſe which were ſeen by the direct rays; and when one image only could be ſeen; he concluded that the other was ſo circumſtanced, as not to reach the eye. Perhaps this might be when the rays paſſed through a place in which the ſeveral cryſtallizations interfered with one another.*

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* Perhaps Bartholin had ſeen the foſſil under this form in the complete cryſtals, before they were broken; for this is the form which they affect when they are complete.

† Experimenta Cryſtalli, p. 13.

The diſtance between the two images of any object was the greateſt when it was placed in a line that biſected the two acute angles of the figure.

If the cryſtal was turned round upon any object, he perceived that one of the images was fixed, while the other performed a revolution about it. He found it poſſible, however, to turn it in ſuch a manner, as that either of the images would be fixed while the other was moveable; or he could make them both move. He obſerved that it was with reſpect to one of theſe images only, that a ray of light falling perpendicularly upon a tranſparent medium is ſubject to no refraction. He alſo obſerved that the unuſual refraction of this ſubſtance was regulated by a line parallel to the ſides of it, whereas the uſual refraction was regulated by a line perpendicular to its ſurface.§

The ſine of the angle of incidence to that of the uſual refraction of this cryſtal, he found to be as five to three.

When this philoſopher comes to conſider the *cauſe* of this remarkable property of Iſland cryſtal, he has nothing better to propoſe than a conjecture, that it may be occaſioned by the diſpoſition of the pores through which the light is tranſmitted.

The ſpecimens of this ſtone which our author examined, were brought from Iceland, and the beſt of them came from a high mountain near the bay called Roerford, from whence pieces were ſometimes dug of a foot thick.

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* Experimenta Cryſtalli, p. 17.

§ Ib. p. 21, 31, 32.

M. Huygens made conſiderable additions to the obſervations of Bartholin on the optical properties of Iſland cryſtal. He was alſo much more accurate in his deſcription of them, which are as follows. Let ABFE, fig. 131, be a fragment of this cryſtal, and let the obtuſe angle ACB be equally divided by the right line CG. If, then, we conceive the cryſtal to be cut by a plane paſſing through this line and CF, the ſection will be GCFH. This he calls the *principal ſection* of the cryſtal, as being moſt neceſſary in explaining the properties of it with reſpect to light.

If the ſurface AB be covered, and only a ſmall aperture be left, at the point K, in the right line CG, and the cryſtal be ſo placed, as that the light of the ſun ſhall fall perpendicularly upon that ſurface, the ray IK will be divided into two, part of it going ſtraight forwards in KL, and another part going in the direction KM, in the plane CGHF; being drawn towards the obtuſe angle C, and making with KL an angle of about 6° 40′. When the ray KM has been tranſmitted through the cryſtal, it proceeds in the line MZ, parallel to IK. As by means of the unuſual refraction, the point M is ſeen by the refracted ray MKI, ſo the point L, by a ſimilar refraction, muſt neceſſarily be ſeen by the refracted ray LRI, ſo that LR may be nearly parallel to MK, provided the diſtance of the eye be very great. But ſince the point L is alſo ſeen by the uſual refraction, in the line IK, it muſt neceſſarily appear double. For the ſame reaſon, if L be a ſmall hole in the paper, or any other ſubſtance that is applied cloſe to the cryſtal, and the cryſtal be held to the light, two holes will appear, which will be farther from one another in proportion to the thickneſs of the cryſtal.

If the cryſtal was placed ſo that the incident ray NO, in the plane GCFH, make an angle of 73° 20′ with the line CG, ſo as to be nearly parallel to the ſide CF, which makes an angle of 70° 57′ with the ſide FH, he found by calculation, that the ray would be divided at O, into two parts, one of which will proceed in a ſtraight line OP, without ſuffering any refraction, either at its entrance or exit; whereas the other part will be refracted into the line OQ. And he found it to be peculiar to the plane GCFH, and its parallels, that all incident rays keep in the ſame plane after they divide within the cryſtal; but that this is not the caſe with rays in the plane of any other ſection.*

Having carefully examined the *uſual refraction* of this cryſtal, and having found ſufficient reaſon, to acquieſce in the concluſion of Bartholin, who determined it to be as five to three, with reſpect to the ſine of the angle of incidence; with the ſame care he endeavoured to aſcertain the proportion of the *unuſual* refraction; and having found the point H perpendicularly over another point E, in the baſe of the cryſtal, fig. 132, he obſerved the appearance of the line CD, which is formed by the unuſual refraction of the ray ED, and the eye being ſo placed in Q, that that apparent line made a right line with KL, ſeen without refraction, he found the triangles REH, RES and alſo the angles RSH and RES, which the incident and refracted rays make with the perpendicular; but he found that in this refraction the ratio of ER to RS was not conſtant, as in the uſual refraction, but varied with the inclination of the incident ray.

He alſo obſerved that when QRE made a right line, that is when the incident ray entered the cryſtal, without ſuffering any refraction (which he knew by the point E, ſeen by the common refraction, being in the line CD, ſeen without refraction) the angle QRG contained 73° 20′, ſo that it is not a ray parallel to the ſide of the cryſtal, which is tranſmitted through it without refraction, as Bartholin concluded, ſince it has an inclination of 70° 57′. This he thought it proper to take notice of, leaſt it ſhould be imagined that the ſingular property of this ray depends upon its paralleliſm to the ſide of the cryſtal.

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* Hugenii Opera, Vol. 3, p. 41, &c.

Purſuing his obſervations on this remarkable property of Iſland cryſtal, he found that its unuſual refraction was according to the following rule. If GCFH, fig. 133, be the ſame ſection of the cryſtal that was before expreſſed by the ſame letters when the incidences of two rays, coming from oppoſite ſides, are equal, as VKSK, the lines into which they will be refracted, KX and KT, will meet the line HF; ſo that the point X and T will be equally diſtant from the point M, on which the refracted perpendicular ray IK will fall.* But the refractions of any other ſection of this cryſtal have this remarkable property, that the refracted rays always go out of the plane of the incident ray perpendicular to the ſuperficies, turning towards that ſide to which the cryſtal inclines.§

Laſtly, he found that if two pieces of this cryſtal were placed ſo that ſome ſpace was betwixt them, and their ſides were parallel to one another, if any ray of light, as AB, fig. 134, was divided in the firſt piece, as into BD and BC, one of theſe lines being according to the uſual refraction, and the other the unuſual one, that neither of theſe parts of the ray would be divided in paſſing through the next piece; but that which had ſuffered the uſual refraction, as DG, would be refracted in the uſual manner only, in GH, and the other part CE, would ſuffer the irregular refraction only, in FF. The ſame thing happens, he ſays, not only in this diſpoſition of the cryſtals, but alſo in every other poſition, in which the principal ſection of each piece is in the ſame plane; nor is it neceſſary that the two ſurfaces, which are oppoſite to one another, be parallel.

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* Huygenii Opera, Vol. 3, p. 44. § Ib. p. 56.

If the cryſtals be ſo diſpoſed, that the planes of the principal ſections be at right angles to one another, whether the ſurfaces that are placed oppoſite to one another be parallel or not, the ray which iſſues from the firſt piece, according to the regular refraction, as DG, fig. 135, ſuffers the unuſual refraction only in the next piece; and, on the contrary, the ray which iſſued from the firſt piece by the irregular refraction, as CE, will ſuffer the regular refraction only in the next piece.

In the innumerable other poſitions, beſides thoſe above mentioned, the rays DG and CE are again divided into two parts, by the refraction of the ſecond piece of cryſtal; ſo that from the original ray AB four will be formed, ſometimes all of them equally luminous, and ſometimes ſome of them will be brighter than others, according to the different poſitions of either of the cryſtals.*

This remarkable property of a *double refraction*, our author found not to be peculiar to the Iſland cryſtal. He obſerved it in the common rock cryſtal, though leſs evidently; for when he had got ſeveral fruſtums of it, cut into ſmooth priſms, in different ſections, he obſerved in each of them, through which he looked at the flame of a candle, or the leads of his window, that all objects appeared double, though the images were but a little diſtant from one another. By this experiment he clearly ſaw the reaſon why a ſubſtance ſo tranſparent as cryſtal is not fit to be uſed in long teleſcopes.

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* Huygenii Opera, Vol. 3, p. 68.

Sir Iſaac Newton doth not ſeem to have made any original obſervations on the properties of Iſland cryſtal, but he firſt ſuggeſted the moſt probable method of accounting for the facts above mentioned, viz. on the principle, that different ſides of the rays of light are poſſeſſed of different properties.

Having recited the principal of the obſervations that were before quoted from Bartholin and M. Huygens on this ſubject, he concludes from them, that there is an original difference in the rays of light; by means of which ſome rays are conſtantly refracted after the uſual, and others after the unuſual manner. For, ſays he, if the difference be not original, but ariſes from new modifications, impreſſed on the rays at their firſt refraction, it would be altered by new modifications in the three following refractions; whereas it ſuffers no alteration, but is conſtant, and has the ſame effect upon the rays in all the refractions. The unuſual refraction he, therefore, concludes is performed by an original property of the rays, and it remains to be inquired whether the rays have not more original properties than are yet diſcovered.

Have not the rays of light, ſays he, ſeveral ſides, endued with ſeveral original properties. For if the planes of perpendicular refraction of the ſecond cryſtal be at right angles with the planes of perpendicular refraction of the firſt cryſtal, the rays which are refracted after the uſual manner, in paſſing through the firſt cryſtal, will be all of them refracted after the unuſual manner in paſſing through the ſecond cryſtal; and the rays which are refracted after the unuſual manner in paſſing through the firſt cryſtal, will be all of them refracted after the uſual manner in paſſing through the ſecond cryſtal. There are not, therefore, two ſorts of rays differing in their nature from one another, one of which is conſtantly, and in all poſitions, refracted after the uſual manner, and the other conſtantly and in all poſitions after the unuſual manner; for the difference between them was only in the poſitions of the ſides of the rays to the planes of perpendicular refraction; and one and the ſame ray is refracted ſometimes after the uſual, and ſometimes after the unuſual manner, according to the poſition in which the ſides are to the cryſtals.

Every ray, therefore, may be conſidered as having four ſides, two of which, oppoſite to one another, incline the ray to be refracted after the unuſual manner, and the other two after the uſual manner. And, ſince theſe diſpoſitions were in the rays before their incidence on the ſecond, third, and fourth ſurfaces of the two cryſtals, and ſuffered no alteration by their refraction; and ſince they were refracted by the ſame laws at all the four ſurfaces, it appears that thoſe diſpoſitions were originally in them.*

Father Beccaria corrects the obſervations of M. Huygens and Sir Iſaac Newton concerning the refraction of rock cryſtal, which they had not examined with ſo much attention as they had given to the Iſland cryſtal. The double refraction of the Iſland cryſtal, he ſays, is when the ray is tranſmitted through two parallel ſides, and therefore iſſues colourleſs; whereas the double refraction of the rock cryſtal is through two ſides that are inclined to one another, which makes the ray iſſue coloured. He concludes his obſervations with a query, whether there be not other ſubſtances, beſides theſe two ſpecies of cryſtal, in which there is a manifold refraction. Graveſende, he ſays, had a priſm made of a Braſil pebble, which had a double refraction at each angle, but of a different kind from one another. He alſo adds ſeveral other queries upon the ſubject.*

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* Newton's Opticks, p. 335.

We are indebted to that induſtrious philoſopher Mr. B. Martin for ſeveral additional obſervations on the refractive power of Iſland cryſtal, ſince thoſe of M. Huygens and Sir Iſaac Newton. He obſerves that, though Iſland cryſtal is of a talcky nature, and much ſofter than glaſs, yet it will take a poliſh little inferior to that of glaſs, at leaſt that it anſwers for all purpoſes and experimants of priſms, ſeveral of which he made and ſhewed in his public lectures.

By theſe priſms it appears, he ſays, that there is not only a *double*, but a *multuple* refraction in Iſland cryſtal. For ſome of theſe priſms, held in a beam of the ſun's light, will ſeparate it into two only, each of which produces a coloured image above that of the ſun, much larger, and the colours more vivid than in thoſe of glaſs priſms of the ſame refracting angle. And the refraction is ſo nearly equal in the two beams, that ſcarce any difference can be ſeen in the coloured images they make of the ſun, a candle, or other object.

Again, ſome priſms ſeparate the ſolar ray into *four* parts, or particular beams of coloured light, which make four images of the ſun, all nearly equally coloured, and ſtrong. Theſe priſms make every object ſeen through them appear four, and each one coloured, as in a glaſs priſm.

Some other pieces of Iſland cryſtal afford priſms with a *ſextuple* power of refraction, for they will divide a beam of the ſun's light into ſix ſeparate coloured beams, and, conſequently, ſhew as many coloured images of the ſun, and of luminous bodies viewed through them. But theſe pieces of cryſtal are not very common. He had ſeen but one that was clear enough to make a good priſm. What he thought particularly remarkable was, that the ſame piece of Iſland cryſtal formed into two priſms, with angles nearly equal, would ſhew through one angle two images only, but through the other it would exhibit ſix.

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* Phil. Tranſ. Vol. 52, p. 489.

He had not been able to procure priſms of Iſland cryſtal of more than a ſextuple, or ſixfold refraction, ſingly uſed; but if two priſms were combined, or placed together, ſo as either to increaſe the refracting angle, or diminiſh it, they became a multiplier, and a multiplicand, and produced a number of images, according to the number in each ſingly. Thus a priſm of two images applied to one of ſix, produced a priſm of twelve images. Again two priſms of four images, each made a compound priſm of ſixteen images. A priſm of four, applied to a priſm of ſix, produced a priſm of twenty four images; and two priſms of ſix images each, compoſed one that exhibited thirty ſix images.

But what Mr. Martin thought to be the moſt extraordinary of all the properties of Iſland cryſtal was, that though the ſides of its plane of perpendicular refraction be parallel to one another, a beam of light tranſmitted through them will not be colourleſs, as is the caſe with all other known ſubſtances. For, ſays he, if PO, fig. 136, be a beam of the ſun's rays, incident upon the firſt ſurface CG of the cryſtal, in a dark room, then will it be divided into two, viz. OL, OM. At the oppoſite and parallel ſurface FH there will be another double, but very different ſort of refraction, of each of the two rays at L and M. For part of the ray OL will be equally refracted into LQ, parallel to the incident ray PO, after the uſual manner in glaſs, &c. but the other part of the ſame ray, OL, will be unequally refracted from L towards T, as they are in a priſm, according to their different degrees of refrangibility; and therefore all the beam LT will appear of various coloured light in the darkened room. After the ſame manner, on the other part, the beam OM will be equally and unequally refracted into the two parts MR, parallel to PO, and MS, conſiſting of coloured light diverging from the point M.

By theſe three beams, refracted from the ſecond ſurface of the cryſtal, there would be three different images of the hole at P, through which the firſt beam entered the room. For by the rays refracted in the uſual manner in LQ, and MR, the hole would have a double image at Q and R, on the ſcreen, but intirely colourleſs, at the diſtance equal to LM, in the correſponding parts from each other.

But the other two rays, LT, MS, painted each of them, a coloured image of the hole in the ſhutter, at ſuch a diſtance on either ſide, as made the angles TLQ, RMS of about five or ſix degrees; and this different refrangibility of rays was ſtrictly agreeable to that in glaſs, but in a greater degree. By variouſly inclining the ſurface to the incident ray, the angle of refraction might on each ſide be varied, from two or three degrees to ſixty or ſeventy.

As theſe pieces of cryſtal made three images, in a right line, in the plane of perpendicular refraction FCGH, and that in the middle was double; ſo he found other pieces by which this line of three images was refracted on each ſide into another line of three images, and the middle line of images was doubled; ſo that, upon the whole, there were no leſs than twelve diſtinct images of the hole of the window-ſhutter, formed upon the ſcreen, by twelve ſeparate rays of light, regularly refracted through the cryſtal, terminated by parallel ſides.

But that this curious optical phenomenon might be more clearly apprehended, he repreſented it to the eye in fig. 137, where the images at 1, 2, 3, are thoſe in the line juſt mentioned, and which, in this piece of cryſtal, became doubled in the images 4, 5, and 6. This line was alſo refracted ſideways, ſo as to form three other images, on each ſide, as at 7, 8, and 9, above and at 10, 11, and 12 below, and all in ſo regular a manner, as to form a mathematical figure, or rhombus, every way ſimilar to that of the plane of perpendicular refraction CFHG.

Theſe images were all of them tinged with a variety of colours, except the two central ones, at 2 and 5, which appeared nearly as white as before. They were not equally coloured, however; for ſome were almoſt wholly red, others yellow, others green, blue, or violet, according as the inclination of the ſurface to the incident ray was varied. The two images at 9 and 10, in the acute angles, were very faint, and unleſs the object was very bright, as the ſun, a candle, &c. could not be ſeen.

If the cryſtal was turned round on its axis, the whole ſyſtem of images moved round with it, as in other caſes, conſtantly, with the red part of each image towards the central hole or beam, and the violet the moſt remote from it.

If priſms of two, four, or ſix images were ſucceſſively applied to the parallelopiped of cryſtal, they would multiply the number

ber of images accordingly in the rhombus, and produce twenty-four, forty-eight, and ſeventy-two images of the ſun, or a candle, the greateſt part of which would be very diſtinct, and completely tinged with regular priſmatic colours, ſo as to compoſe a kind of natural girandole of painted luminaries, infinitely exceeding any production or imitation, by the art of glaſs.*

Theſe are the facts which our author recites. To account for them, he owns himſelf at a loſs; but thinks that ſome peculiar ſtructure of this ſubſtance, hitherto unobſerved, or ſome peculiar modification of the particles of light, which Newton himſelf was not apprized of, muſt be, in part, the cauſe of them, and ſuſpects that, poſſibly, they may, in ſome meaſure, be owing to certain fine fiſſures, which are viſible in ſome poliſhed pieces of this cryſtal, the planes of which are at right angles to the plane of perpendicular refraction, and have the ſame inclination with the right line CF, fig. 13[illegible]. Theſe fiſſures are diſtinguiſhed by rings of coloured light, and each of them ſevers from the parallelopiped an equiangular priſm. They had not been mention by M. Huygens or Sir Iſaac Newton, and Mr. Martin never met with more than one piece that ſhewed them very diſtinctly. They are repreſented in fig. 139, by the ſeveral dotted parallel lines, ſuch as their interſections appear to make with the ſurface of the cryſtal, and at right angles to the plane of refraction DE.

It is to be regretted that Mr. Martin has not been more particular in noting the circumſtances of theſe very remarkable appearances. At preſent we do not even know how to look for them again, as he has neither mentioned the ſizes of the refracting angles of his priſms, nor the ſituations of their planes reſpectively to the native planes of the cryſtal. It is a conſiderable advantage, however, that philoſophers are now appriſed of theſe appearances; and it is to be hoped that, if they hit upon them again, they will attend more accurately to the law of them, in order to trace their cauſes. It is a great hindrance of theſe inquiries that, beſides the difficulty there is in poliſhing this cryſtal, it is not eaſy to procure pieces of it ſufficiently tranſparent for the purpoſe.

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* Martin's Eſſay on Iſland cryſtal, p. 13.

SECTION IX.

Of light proceeding from PUTRESCENT SUBSTANCES, &c. and PHOSPHORUS.

In general, a very conſiderable degree of heat is requiſite to the emiſſion of light from any body; but there are ſeveral exceptions to this, eſpecially in light proceeding from putreſcent ſubſtances and phoſphorus, together with that of glow-worms, and other ſimilar appearances. Light proceeding from putreſcent animal and vegetable ſubſtances, as well as from glow-worms, is mentioned by Ariſtotle. Thomas Bartholin mentions four kinds of luminous inſects, two with wings, and two without; but in hot climates travellers ſay they are found in much greater numbers, and of different ſpecies. Columna, an induſtrious naturaliſt, obſerves that their light is not extinguiſhed immediately upon the death of the animal.*

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* Bartholin De Luce Animalium, p. 206.

The firſt diſtinct account that we meet with of light proceeding from putreſcent animal fleſh is that which is given by Fabricius Ab Aquapendente, who ſays, that when three Roman youths, reſiding at Padua, had bought a lamb, and had eaten part of it on Eaſterday 1592, ſeveral pieces of the remainder, which they kept till the day following, ſhone like ſo many candles, when they were caſually viewed in the dark. Part of this luminous fleſh was immediately ſent to Aquapendente, who was profeſſor of anatomy in that city. He obſerved that both the lean and the fat of this meat ſhone with a whitiſh kind of light, and alſo took notice that ſome pieces of kid's fleſh, which had happened to have lain in contact with it, was luminous, as well as the fingers, and other parts of the bodies of thoſe perſons who touched it.* Thoſe parts, he obſerved, ſhone the moſt which were ſoft to the touch, and ſeemed to be tranſparent in candle light; but where the fleſh was thick and ſolid, or where a bone was near the outſide, it did not ſhine. †

After this appearance, I find no account of any other ſimilar to it, before that which was obſerved by Bartholin, and of which he gives a very pompous deſcription in his ingenious treatiſe *De Luce Animalium*. This happened at Montpelier in 1641, when a poor old woman had bought a piece of fleſh in the market, intending to make uſe of it the day following. But happening not to be able to ſleep well that night, and her bed and pantry being in the ſame room, ſhe obſerved ſo much light to come from the fleſh, as to illuminate all the place where it was hung. A part of this luminous fleſh was carried as a curioſity to Henry Bourbon, Duke of Condé, the governor of the place, who viewed it for ſeveral hours, with the greateſt aſtoniſhment.

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* Bartholin De Luce Animalium, p. 183.

† Aquapendente De Viſione, &c. p. 45.

This light was obſerved to be whitiſh, and not to cover the whole ſurface of the fleſh, but certain parts only, as if gems of unequal ſplendor had been ſcattered over it. This fleſh was kept till it began to putrify, when the light vaniſhed, which, as ſome religious people fancied, it did in the form of a croſs.*

It is natural to expect that the almoſt univerſal experimental philoſopher Mr. Boyle ſhould try the effect of his air pump upon theſe luminous ſubſtances. Accordingly we find that he did not fail to do it; when he preſently found that the light of rotten wood was extinguiſhed in *vacuo,* and revived again on the admiſſion of the air, even after a long continuance in vacuo; but the extinction of this light was not ſo complete immediately upon exhauſting the receiver, as ſome little time afterwards.† He could not perceive, however, that the light of rotten wood was increaſed in condenſed air; but this he imagined might ariſe from his not being able to judge very well of the degree of light, through ſo thick and cloudy a glaſs veſſel as he then made uſe of; but I find that the light of a ſhining fiſh, which was put into a condenſing engine before the Royal Society, in 1668, was rendered more vivid by that means.§ The principal of Mr. Boyle's experiments were made in October 1667.

This philoſopher attended to a great variety of circumſtances relating to this curious phenomenon. Among other things, he obſerved that change of air was not neceſſary to the maintenance of this light; for it continued a long time when a piece of the wood was put into a very ſmall glaſs hermetically ſealed;‡ and it made no difference when this tube which contained the wood was put into an exhauſted receiver. This he alſo obſerved with reſpect to a luminous fiſh, which he put into water, and placed in the ſame circumſtances.* He alſo found that the light of ſhining fiſhes had other properties in common with that of ſhining wood; but the latter, he ſays, was preſently quenched with water, ſpirit of wine, a great variety of ſaline mixtures, and other fluids.† Water, however, did not quench all the light of ſome ſhining veal, on which he tried it, though ſpirit of wine deſtroyed its virtue preſently.§

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* Bartholin De Luce Animalium, p. 184.

† Boyle's Works, Vol. 3, p. 156.

§ Birch's Hiſtory, Vol. 2, p. 254.

‡ Boyle's Works, Vol. 3, p. 158.

Mr. Boyle was often diſappointed in his experiments on ſhining fiſhes, finding that they did not always ſhine in the very ſame circumſtances, as far as he could judge, with others which had ſhined before. At one time that they failed to ſhine, according to his expectations, he obſerved that the weather was variable, and not without ſome days of froſt and ſnow.‡ In general, he made uſe of whitings, finding them the fitteſt for his purpoſe.‖ I find, however, that, in a diſcourſe upon this ſubject at the Royal Society, in 1681, it was aſſerted that, of all fiſhy ſubſtances, the eggs of lobſters, after they had been boiled, ſhone the brighteſt.** Olig. Jacobœus obſerves, that, upon opening a ſea polypus, it was ſo luminous, as to ſtartle ſeveral perſons who ſaw it; and he ſays that the more putrid the fiſh was, the more luminous it grew. The nails alſo, and the fingers of the perſons who touched it became luminous; and the black liquor which iſſued from the animal, and which is its bile ſhone alſo; but with a very faint light.††

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* Boyle's Works, Vol. 3, p. 163. † Ib. p. 166. § Ib. p. 170.
‡ Ib. p. 162. ‖ Ib. p. 163. ** Birch's Hiſtory, Vol. 2, p. 70.
†† Acta Hafnienſia, Vol. 5, p. 282.

Mr. Boyle's obſervation of light proceeding from fleſh meat was quite caſual. On the 15th of February 1672, one of his ſervants was greatly alarmed with the ſhining of ſome *veal*, which had been kept a few days, but had no bad ſmell, and was in a ſtate very proper for uſe. The ſervant immediately made his maſter acquainted with this extraordinary appearance; and though he was then in bed, he ordered it to be immediately brought to him, and he examined it with the greateſt attention. Suſpecting that the ſtate of the atmoſphere had ſome ſhare in the production of this phenomenon, he takes notice, after deſcribing the appearance, that the wind was South Weſt, and bluſtering, the air hot for the ſeaſon, the moon was paſt its laſt quarter, and the mercury in the barometer was at 29 $\frac{1}{10}$ inches.*

Mr. Boyle draws a minute compariſon between the light of burning coals and that of ſhining wood or fiſh, ſhewing in what particulars they agree, and in what they differ. Among other things, he obſerves that extreme cold extinguiſhes the light of ſhining wood, as appeared when a piece of it was put into a glaſs tube, and held in a frigorifick mixture.§ It is poſſible that this extreme cold might check the putrefaction which was the cauſe of that light. Mr. Boyle alſo found that rotten wood did not waſte itſelf by ſhining, and that the application of a thermometer to it did not diſcover the leaſt degree of heat.

There is a remarkable ſhell fiſh called *pholas*, which forms for itſelf holes in various kinds of ſtone, &c. The figure of it, and of its cell, may be ſeen Ac. Par. 1722, plate 7, fig. 1. 2. That this fiſh is luminous was noticed by Pliny, who obſerves that it ſhines in the mouth of the perſon who eats it, and, if it touch his hands or cloaths makes them luminous. He alſo ſays that the light depends upon its *moiſture*. The light of this fiſh has furniſhed matter for various obſervations and experiments to M. Reaumur, and the Bolognian Academicians, eſpecially Beccarius, who took ſo much pains with the ſubject of phoſphoreal light.

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* Birch's Hiſtory, Vol. 2, p. 169. § Ib. p. 166.

M. Reaumur obſerves that, whereas other fiſhes give light when they tend to putreſcence, this is more luminous in proportion to its being freſh; that when they are dried their light will revive if they be moiſtened, either with freſh or ſalt water; but that brandy immediately extinguiſhes it. He endeavoured to make this light permanent, but none of his ſchemes ſucceeded.*

The attention of the Bolognian Academicians was engaged to this ſubject by M. F. Marſilius, in 1724, who brought a number of theſe fiſhes, and the ſtones in which they were incloſed, to Bologna, on purpoſe for their examination.

Beccarius obſerved that, though this fiſh ceaſed to ſhine when it became putrid, yet that, in its moſt putrid ſtate, it would ſhine, and make the water in which it was immerſed luminous, when they were agitated.† Galeatius and Montius found that wine or vinegar extinguiſhed this light, that in common oil it continued ſome days, but in rectified ſpirit of wine, or urine, hardly a minute.§

In order to obſerve in what manner this light was affected by different degrees of *heat*, they made uſe of a Reaumur's thermometer, and found that water rendered luminous by theſe fiſhes increaſed in light till the heat arrived to 45 degrees, but that it then became ſuddenly extinct, and could not be revived again.‖

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* Ac. Par. 17, 23, H. 10, M. 290. &c.

† Com. Bonon. Vol. 2, p. 232.

§ Ib. 254.

‖ Ib. p. 256.

In the experiments of Beccarius, a ſolution of ſea ſalt increaſed the light of the luminous water, a ſolution of nitre did not increaſe it quite ſo much. Sal Ammoniac diminiſhed it a little, oil of tartar per deliquium nearly extinguiſhed it, and the acids intirely.* This water poured upon freſh calcined gypſum, rock cryſtal, ceruſs, or ſugar, became more luminous. He alſo tried the effects of it when poured upon various other ſubſtances, but there was nothing very remarkable in them. Afterwards, uſing luminous milk, he found that oil of vitriol extinguiſhed the light; but that oil of tartar increaſed it.†

This gentleman had the curioſity to try how differently *coloured ſubſtances* were affected by this kind of light; and having, for this purpoſe, dipped ſeveral ribbons in it, the white came out the brighteſt, next to this was the yellow, and then the green; the other colours could hardly be perceived. It was not, however, any *particular colour*, but only *light* that was perceived in this caſe.§ He then dipped boards painted with the different colours, and alſo glaſs tubes, filled with ſubſtances of different colours, in water rendered luminous by the fiſhes. In both theſe caſes the red was hardly viſible, the yellow was the brighteſt, and the violet the dulleſt. But on the boards the blue was nearly equal to the yellow, and the green more languid; whereas in the glaſſes, the blue was inferior to the green.‡

Of all the liquors to which he put the pholades, *milk* was rendered the moſt luminous. A ſingle pholas made ſeven ounces of milk ſo luminous, that the faces of perſons might be diſtinguiſhed by it, and it looked as if it was tranſparent.‖

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* Com. Bonon, Vol. 2, p. 257. † Ib. p. 262. § Ib. p. 260. ‡ Ib. p. 260. ‖ Ib. p. 261.

Air appeared to be neceſſary to this light; for when Beccarius put the luminous milk into glaſs tubes, no agitation would make it ſhine, unleſs bubbles of air were mixed with it.* Alſo Montius and Galeatius found that, in an exhauſted receiver, the pholas loſt its light, but the water was ſometimes made more luminous; which they aſcribed to the riſing of bubbles of air through it.†

Beccarius, as well as Reaumur, had many ſchemes to render the light of theſe pholades permanent. For this purpoſe he kneaded the juice into a kind of paſte, with flour, and found that it would give light when it was immerſed in warm water; but it anſwered beſt to preſerve the fiſh in honey. In any other method of preſervation the property of becoming luminous would not continue longer than ſix months, but in honey it had laſted above a year; and then it would, when plunged in warm water, give as much light as ever it had done.§

Similar, in ſome reſpects, to thoſe obſervations on the light of the pholas, was that which was obſerved to proceed from wood which was moiſt, but not in a putrid ſtate, which was very conſpicuous in the dark.‡

That the *ſea* is ſometimes luminous, eſpecially when it is put in motion, by the daſhing of oars, or the beating of it againſt a ſhip, has been obſerved with admiration by a great number of perſons. Mr. Boyle, after reciting all the circumſtances of this appearance, as far as he could collect them from the accounts of navigators, as its being extended as far as the eye could reach, and, at other times being viſible only when the water was daſhed againſt ſome other body; that, in ſome ſeas, this phenomenon is accompanied by ſome particular winds, but not in others; and that ſometimes one part of the ſea will be luminous when another part, not far from it, will not be ſo, concludes with ſaying, that he could not help ſuſpecting that theſe odd phenomena, belonging to great maſſes of water, were in ſome meaſure owing to ſome coſmical law, or cuſtom of the terreſtrial globe, or at leaſt of the planetary vortex.* But we ſhall find that this philoſopher looked much too high for the cauſe of this appearance, great and magnificent as it is.

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* Com. Bonon. Vol. 2, p. 264. † Ib. 268. § Ib. 273. ‡ Acta Cæſarienſia, Vol. 5, p. 485.

Some curious obſervations on the ſhining of ſome fiſhes, and the pickle in which they were immerſed, were made by Dr. Beal, in May 1665; and had they been properly attended to and purſued, might have led to the diſcovery of the cauſe of this appearance. Having put ſome boiled mackerel into water, together with ſalt and ſweet herbs, when the cook was, ſome time after, ſtirring it, in order to take out ſome of the fiſhes, ſhe obſerved that, at the firſt motion, the water was very luminous; and that the fiſh ſhining through the water, added much to the light which the water yielded. The water was of itſelf thick and blackiſh, rather than of any other colour; and yet it ſhined on being ſtirred; and at the ſame time the fiſhes appeared more luminous than the water. Wherever the drops of this water, after it had been ſtirred, fell to the ground, they ſhined, and the children in the family diverted themſelves with taking the drops, which were as broad as a penny, and running with them about the houſe. The cook obſerved that when ſhe turned up that ſide of the fiſh that was loweſt, no light came from it, and that when the water had ſettled for ſome time it did not ſhine at all. The day following, the water gave but little light, and only after a briſk agitation, though the fiſhes continued to ſhine as well from the inſide as the outſide, and eſpecially about the throat, and ſuch places as ſeemed to have been a little broken in the boiling.

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* Phil. Tranſ. Vol. 59, p. 450.

When, in the light of the ſun, he examined, with a microſcope, a ſmall piece of a fiſh which had ſhined very much the night before, he found nothing remarkable on its ſurface, except that he thought he perceived what he calls a *ſteam*, rather dark than luminous, ariſing like a very ſmall duſt from the fiſh, and here and there a very ſmall, and almoſt imperceptible ſparkle. Of the ſparkles he had no doubt, but he thought it poſſible that the ſteam might be a deception of the ſight, or ſome duſt in the air.

Finding the fiſh to be quite dry, he moiſtened it with his ſpittle, and then obſerved that it gave a little light, though but for a ſhort time. The fiſh, at that time, was not fetid, nor yet inſipid to the beſt diſcerning palate. Two of the fiſhes he kept two or three days longer for farther trial; but the weather being very hot they became fetid; and, contrary to his expectations, there was no more light produced either by the agitation of the water, or in the fiſh.*

Father Bourzes, in his voyage to the Indies, in 1704, took particular notice of the luminous appearance of the ſea; but though many of the circumſtances which he mentions ſeemed to point to the true cauſe of it, viz. the putreſcent matter in the ſea, he does not ſeem to have been aware of it. The light was ſometimes ſo great, that he could eaſily read the title of a book by it, though he was nine or ten feet from the ſurface of the water.

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* Phil. Tranſ. ab. Vol. 3, p. 639.

water. Sometimes he could eaſily diſtinguiſh, in the wake of the ſhip, the particles that were luminous from thoſe that were not; and they appeared not to be all of the ſame figure. Some of them were like points of light, and others ſuch as ſtars appear to the naked eye. Some of them were like globes, of a line or two in diameter, and others as big as ones head. Sometimes they formed themſelves into ſquares of three or four inches long, and one or two broad. Sometimes all theſe different figures were viſible at the ſame time, and ſometimes there were what he calls *vortices* of light, which at one particular time appeared and diſappeared immediately, like flaſhes of lightning.

Nor only did the wake of the ſhip produce this light, but fiſhes alſo, in ſwimming, left ſo luminous a track behind them, that both their ſize and ſpecies might be diſtinguiſhed by it. When he took ſome of the water out of the ſea, and ſtirred it ever ſo little with his hand, in the dark, he always ſaw in it an infinite number of bright particles; and he had the ſame appearance whenever he dipped a piece of linen in the ſea, and wrung it in a dark place, even though it was half dry; and he obſerved that when the ſparkles fell upon any thing that was ſolid, it would continue ſhining for ſome hours together.

After mentioning ſeveral circumſtances which did not contribute to this appearance, this Father obſerves that it depends very much upon the *quality of the water*; and he was pretty ſure that this light is the greateſt when the water is fatteſt and fulleſt of foam. For in the main ſea, he ſays, the water is not every where equally pure, and that ſometimes if linen be dipped in the ſea, it is clammy when it is drawn up again; and he often obſerved that when the wake of the ſhip was the brighteſt, the water was the moſt fat and glutinous, and that linen moiſtened with it produced a great deal of light, if it was ſtirred or moved briſkly. Beſides, in ſome parts of the ſea, he ſaw a ſubſtance like ſaw duſt, ſometimes red, and ſometimes yellow; and when he drew up the water in thoſe places, it was always viſcous and glutinous. The ſailors told him that it was the ſpawn of whales, that there are great quantities of it in the North, and that ſometimes, in the night, they appeared all over of a bright light, without being put in motion by any veſſel or fiſh paſſing by them.

As a confirmation of his conjecture, that the more glutinous the ſea water is, the more it is diſpoſed to become luminous, he obſerves that one day they took a fiſh which was called a *boneta*, the inſide of the mouth of which was ſo luminous, that, without any other light, he could read the ſame characters which he had before read by the light in the wake of the ſhip; and the mouth of this fiſh was full of a viſcous matter, which, when it was rubbed upon a piece of wood, made it immediately all over luminous; though, when the moiſture was dried up, the light was extinguiſhed.*

The Abbé Nollet was much ſtruck with the luminouſneſs of the ſea when he was at Venice in 1749, and, after taking a great deal of pains to aſcertain the circumſtances of it, concluded that it was occaſioned by a ſhining inſect; and having examined the water very often, he at length did find a ſmall inſect, which he particularly deſcribes, and to which he attributes the light.§ The ſame hypotheſis had alſo occured to M. Vianelli, profeſſor of medicine in Chioggia near Venice; and both he and M. Grizellini, a phyſician in Venice, have given drawings of the inſects from which they imagined this light to proceed.

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* Phil. Tranſ. ab Vol. 5, p. 213.

§ Ac. Par. 1750, M. p. 88.

The Abbé was the more confirmed in his hypotheſis by obſerving, ſome time after, the motion of ſome luminous particles in the ſea. For, going into the water, and keeping his head juſt above the ſurface, he ſaw them dart from the bottom, which was covered with weeds, to the top, in a manner which he thought very much reſembled the motions of inſects; though, when he endeavoured to catch them, he only found ſome luminous ſpots upon his handkerchief, which were enlarged when he preſſed them with his finger.*

M. Le Roi, making a voyage on the Mediterranean, preſently after the Abbé Nollet made his obſervations at Venice, took notice that, in the day time, the prow of the ſhip in motion threw up many ſmall particles, which, falling upon the water, rolled upon the ſurface of the ſea, for a few ſeconds, before they mixed with it; and in the night the ſame particles, as he concluded, had the appearance of fire.† Taking a quantity of the water, the ſame ſmall ſparks appeared whenever it was agitated; but, as was obſerved with reſpect to Dr. Beal's experiments, every ſucceſſive agitation produced a leſs effect than the preceding, except after being ſuffered to reſt a while; for then a freſh agitation would make it almoſt as luminous as the firſt.§ This water, he obſerved, would retain its property of ſhining by agitation a day or two; but it diſappeared immediately on being ſet on the fire, though it was not made to boil.‡

This gentleman, after giving much attention to this phenomenon, concludes that it is not occaſioned by any ſhining inſects,

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* Ac. Par. 1750, M. p. 91. † Memoires Preſentés. Vol. 3, p. 144. § Ib. p. 145. ‡ Ib. p. 146.

ſects, as the Abbé Nollet imagined, eſpecially as, after carefully examining ſome of the luminous points, which he caught upon an handkerchief, he found them to be round like large pins heads, but with nothing of the appearance of any animal, though he viewed them with a microſcope.* He alſo found that the mixture of a little ſpirit of wine with water juſt drawn from the ſea, would give the appearance of a great number of little ſparks, which would continue viſible longer than thoſe in the ocean. All the acids, and various other liquors, produced the ſame effect; though not quite ſo conſpicuouſly; but no freſh agitation would make them luminous again. M. Le Roi is far from aſſerting that there are no luminous inſects in the ſea. He even ſuppoſes that the Abbé Nollet and M. Vianelli had found them. But he was ſatisfied that the ſea is luminous chiefly on ſome other account, though he does not ſo much as advance a conjecture about what it is.§

M. Ant. Martin made many experiments on the light of fiſhes, with a view to diſcover the cauſe of the light of the ſea, before he had ſeen any thing that had been written upon the ſubject; and though many of his obſervations had been anticipated, others are original, and of ſome importance. He thought that he had reaſon to conclude, from a great variety of experiments, that all ſea fiſhes have this property; but that it is not to be found in any that are produced in freſh water. Nothing depended upon the *colour* of the fiſhes, except that he thought that the white ones, and eſpecially thoſe that had white ſcales, were a little more luminous than others. This light, he found, was increaſed by a ſmall quantity of ſalt, and alſo by a ſmall degree of warmth, though a greater degree extinguiſhed it. This agrees with another obſervation of his, that it depends intirely upon a kind of moiſture, which they had about them, and which a ſmall degree of heat would expell, when an oilineſs remained which did not give this light, but would burn in the fire. Light from the fleſh of birds or beaſts is not ſo bright, he ſays, as that which proceeds from fiſh. Human bodies, he ſays, have ſometimes emitted light about the time that they began to putrify, and the walls and roof of a place in which dead bodies had often been expoſed, had a kind of dew or clammineſs upon it, which was ſometimes luminous; and he imagined that the lights which are ſaid to be ſeen in burying grounds may be owing to this cauſe.*

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* Memoires Preſentés, Vol. 3, p. 152. § Ib.

The obſervations of Father Bourzes, which were recited near the beginning of this ſection, make it extremely probable that the luminouſneſs of the ſea ariſes from ſlimy and other putreſcent matter with which it abounds; but ſome late experiments of Mr. Canton ſeem to be deciſive in favour of this hypotheſis, which, indeed, may be ſaid to be his own; for though Father Bourzes concluded that the light of the ſea was owing to certain ſubſtances which float on its ſurface, he does not mention their tendency to putrifaction, as a circumſtance of any conſequence to the appearance. The experiments of Mr. Canton are the more valuable, as they are very eaſily made, ſo that any perſon may repeat them, and thereby fully ſatisfy himſelf with reſpect to this appearance, and the cauſe of it.

On the evening of the 14th of June 1768, he put a ſmall freſh whiting into a gallon of ſea water, in a pan which was about fourteen inches in diameter, and took notice that neither the whiting nor the water, when agitated, give any light: A Fahrenheit's thermometer, in the cellar where the pan was placed, ſtood at fifty four degrees. The 15th, at night, that part of the fiſh which was even with the ſurface of the water was luminous, but the water itſelf was dark. He drew the end of a ſtick through it, from one ſide of the pan to the other, and the water appeared luminous behind the ſtick all the way, but gave light only where it was diſturbed. When all the water was ſtirred, the whole became luminous, and appeared like milk, giving a conſiderable degree of light to the ſides of the pan; and it continued to do ſo for ſome time after it was at reſt. The water was moſt luminous when the fiſh had been in it about twenty eight hours, but would not give any light by being ſtirred, after it had been in it three days.

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* Swed. Abhand, Vol. 23, p. 225.

He then put a gallon of freſh water into one pan, and an equal quantity of ſea water into another, and into each pan he put a freſh herring, of about three ounces. The next night the whole ſurface of the ſea-water was luminous, without being ſtirred; but it was much more ſo when it was put in motion; and the upper part of the herring, which was conſiderably below the ſurface of the water, was alſo very bright; while at the ſame time, the freſh water, and the fiſh that was in it, were quite dark. There were ſeveral very bright luminous ſpots on different parts of the ſurface of the ſea-water, and the whole, when viewed by the light of a candle, ſeemed covered with a greaſy ſcum. The third night the light of the ſea water, while at reſt, was very little, if at all leſs than before; but when ſtirred its light was ſo great as to diſcover the time by a watch, and the fiſh in it appeared as a dark ſubſtance. After this its light was evidently decreaſing, but was not quite gone before the ſeventh night. The freſh water and the fiſh in it were perfectly dark during the whole time. The thermometer was generally above 60.

The preceding experiments were made with ſea water, but he now made uſe of other water, into which he put common or ſea ſalt, till he found, by an hydrometer, that it was of the ſame ſpecific gravity with the ſea water; and, at the ſame time, in another gallon of water, he diſſolved two pounds of ſalt; and into each of theſe waters he put a ſmall freſh herring. The next evening the whole ſurface of the artificial ſea water was luminous without being ſtirred; but gave much more light when it was diſturbed. It appeared exactly like the real ſea water in the preceding experiment, its light laſted about the ſame time, and went off in the ſame manner; while the other water, which was almoſt as ſalt as it could be made, never gave any light. The herring which was taken out of it the ſeventh night, and waſhed from its ſalt, was found firm and ſweet; but the other herring was very ſoft and putrid, much more ſo than that which had been kept as long in freſh water. If a herring, in warm weather, be put into ten gallons of artificial ſea water, inſtead of one, the water, he ſays, will ſtill become luminous, but its light will not be ſo ſtrong.

From theſe experiments it is evident that the quantity of ſalt contained in ſea water haſtens putrefaction, which agrees with the remarkable obſervation of Sir John Pringle; but ſince that preciſe quantity of ſalt which promotes putrefaction the moſt, is leſs than that which is found in ſea water, it is probable, Mr. Canton obſerves, that if the ſea was leſs ſalt, it would be more luminous.

It appeared by ſome of the firſt obſervations on this ſubject, that *heat* extinguiſhes the light of putreſcent ſubſtances. Mr. Canton alſo attended to this circumſtance, and obſerves, that though the greateſt ſummer heat is well known to promote putrefaction, yet twenty degrees more than that of the human blood ſeems to hinder it. For putting a ſmall piece of a luminous fiſh into a thin glaſs ball, he found that water of the heat of 118 degrees would extinguiſh its light in leſs than half a minute; but that, on taking it out of the water, it would begin to recover its light in about ten ſeconds; but it was never afterwards ſo bright as before.

Mr. Canton made the ſame obſervation that Mr. Ant. Martin had done, viz. that ſeveral kinds of river fiſh could not be made to give light, in the ſame circumſtances in which any ſea fiſh became luminous. He ſays, however, that a piece of carp made the water very luminous, though the outſide, or ſcaly part of it, did not ſhine at all.

For the ſake of thoſe perſons who may chuſe to repeat his experiments, he obſerves that *artificial ſea water* may be made without the uſe of an hydrometer, by the proportion of four ounces avoirdupoize of ſalt, to ſeven pints of water, wine meaſure.*

It ſeems probable that ſome kinds of putreſcent matter, capable of giving a conſiderable degree of light, are *volatile*, and that this is the cauſe of that luminous appearance which goes by the name of *ignis fatuus*, or in common Engliſh *Will with a wiſp*, to which the credulous vulgar aſcribe very extraordinary and eſpecially miſchievous powers. This phenomenon is chiefly viſible in damp places, and is alſo ſaid to be very often ſeen in burying grounds, and near dunghills. Travellers ſay that it is very frequent near Bologna in Italy, and in ſeveral parts of Spain and Ethiopia. The form and ſize of it are very various, and often variable.*

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* Phil. Tranſ. Vol. 59, p. 446, &c.

It was the opinion of many philoſophers, and eſpecially Willoughby and Ray, that the ignis fatuus is made by ſhining inſects; but this opinion was never well ſupported. Sir Iſaac Newton calls it *a vapour ſhining without heat*, and ſuppoſes that there is the ſame difference between this vapour and flame, that there is between wood ſhining without heat, and burning coals of fire.† That this opinion is juſt, and, moreover, that the light of this vapour ſhining without heat is of the ſame nature with light from putreſcent ſubſtances, may, I think, be concluded from the following circumſtances relating to them, as deſcribed by Dr. Derham, and G. B. Beccari.

The former of theſe gentlemen, having obſerved an ignis fatuus in ſome boggy ground, between two rocky hills, in a dark and calm night, got by degrees within two or three yards of it, and thereby had an opportunity of viewing it to the greateſt advantage. It kept ſkipping about a dead thiſtle, till a ſlight motion of the air, occaſioned as he ſuppoſed, by his near approach to it, made it jump to another place; and as he advanced, it kept flying before him. He was ſo near to it, that, had it been the ſhining of glow-worms, he was ſatisfied that he could not but have diſtinguiſhed the ſeparate lights of which it muſt have conſiſted; whereas it was one uniform body of light. He therefore thought that it muſt be an ignited vapour.§

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* Muſchenbroeck's Introductio, Vol. 2, p. 1062. † Opticks, p. 316.

§ Similar in ſome reſpects to this light, was one that ſurrounded the body and the bed of a woman at Milan, which fled from the hand that approached it, but was at length diſperſed by the agitation of the air. Acta Cæſarienſia, Vol. 3, p. 11.

M. Beccari made it his buſineſs to inquire concerning this phenomenon of all his acquaintance, who had had opportunities of obſerving it, either on the mountains, or on the plain. He found that two which appeared on the plains, one to the North, and the other to the Eaſt of Bologna, were to be ſeen almoſt every dark night, eſpecially the latter; and the light they gave was equal to that of an ordinary faggot. That to the eaſt of Bologna once appeared to a gentleman of his acquaintance, as he was travelling, and kept him company above a mile, conſtantly moving before him, and caſting a ſtronger light upon the road than the torch which was carried along with him. All theſe luminous appearances, he ſays, gave light enough to make all neighbouring objects viſible, and they were always obſerved to be in motion, but this motion was various and uncertain. Sometimes they would riſe up, and at other times ſink; but they commonly kept hovering about ſix feet from the ground. They would alſo diſappear of a ſudden, and inſtantly appear again, in ſome other place. They differed both in ſize and figure, ſometimes ſpreading pretty wide, and then again contracting themſelves; ſometimes breaking into two, and then joining again; ſometimes floating like waves, and dropping, as it were, ſparks of fire. He was aſſured that there was not a dark night all the year round in which they did not appear, and that they were obſerved more frequently when the ground was covered with ſnow, than in the hotteſt ſummer; nor did rain or ſnow in the leaſt hinder their appearance; but, on the contrary, they were obſerved more frequently, and caſt a ſtronger light in rainy and wet weather; nor were they much affected by the wind.*

The ground to the Eaſt of Bologna, where the largeſt of theſe appearances was ſeen, is, he ſays, a hard chalky and clayey ſoil, which will retain the water a long time, and afterwards, in hot weather, would break into large cracks; but on the mountains, where the *ignes fatui* were ſmaller, the ſoil was of a looſe ſandy texture, which would not keep the water very long. According to the beſt information he could procure, theſe lights very much frequent brooks and rivers, being often obſerved on the banks of them; perhaps, he ſays, becauſe the current of air carries them thither more readily than to any other place.*

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* Phil. Tranſ. ab Vol. 7, p. 57.

This gentleman concludes his account of theſe appearances with the following curious narrative. An intelligent gentleman travelling in March, between eight and nine in the evening, in a mountainous road, about ten miles South of Bologna, perceived a light, which ſhone very ſtrongly upon ſome ſtones which lay on the banks of the river *Rioverde*. It ſeemed to be about two feet above the ſtones, and not far from the water. In ſize and figure it had the appearance of a parallelopiped, ſomewhat more than a foot in length, and half a foot high, the longeſt ſide being parallel to the horizon. Its light was ſo ſtrong, that he could plainly diſtinguiſh by it, part of a neighbouring hedge, and the water of the river; only in the Eaſt corner of it the light was rather faint, and the ſquare figure leſs perfect, as if it was cut-off, or darkened by the ſegment of a circle.

His curioſity tempting him to examine this appearance a little nearer, he advanced gently towards the place; but was ſurpriſed to find that it changed gradually from a bright red, firſt to a yellowiſh, and then to a pale colour, in proportion as he drew nearer; and when he came to the place itſelf it quite vaniſhed. Upon this he ſtepped back, and not only ſaw it again, but found that the farther he went from it, the ſtronger and brighter it grew. When he examined the place of this luminous appearance, he could not perceive the leaſt ſmell, or any other mark of fire.

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* Phil. Tranſ. ab. Vol. 7, p. 59.

This extraordinary account was confirmed to M. Beccari by another gentleman, who frequently travelled the ſame road, and who aſſured him that he had ſeen the very ſame light five or ſix different times, in Spring and Autumn, and that he had always obſerved it to be of the very ſame ſhape, and in the ſame place; and he once took particular notice of its coming out of a neighbouring place, and ſettling itſelf in the figure above deſcribed.

M. Beccari owns himſelf to be greatly at a loſs to account not only for this very remarkable appearance, but alſo for the ignes fatui in general. He only ſays, that all perſons who ever ſaw any of theſe appearances, agree, that they caſt a light quite different from that of ſhining flies.*

Dr. Shaw deſcribes an ignis fatuus, which he ſaw in the Holy Land, the circumſtances of which are ſo remarkable, that they well deſerve to be recited, eſpecially as ſome of them ſeem to point to the cauſe of the phenomenon. As he and his company were travelling by night, through the valleys of Mount Ephraim, they were attended, more than an hour, by an ignis fatuus, which was ſometimes globular, or in the form of the flame of a candle; and which would, immediately afterwards, ſpread itſelf ſo much as to involve the whole company in a pale inoffenſive light, and then contract itſelf again, and ſuddenly diſappear. But in leſs than a minute it would become viſible as before; or, running along from one place to another, with a ſwift progreſſive motion, would expand itſelf, at certain intervals, over more than two or three acres of the adjacent mountains. The atmoſphere, from the beginning of the evening, had been remarkably thick and hazy, and the dew, as they felt it upon their bridles, was unuſually clammy and unctuous. In the ſame kind of weather, he ſays, he has obſerved thoſe luminous appearances, which, at ſea, ſkip about the maſts and yards of ſhips, and which the ſailors call *Corpuſanſe*, which is a corruption of the Spaniſh *Cuerpo ſanto*.*

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* Phil. Tranſ. ab Vol. 7, p. 60.

A light in ſome reſpects ſimilar to that of putreſcent matter has been found to proceed from that celebrated chymical production called *phoſphorus*, which is, in fact, an *imperfect ſulphur*, tending to decompoſe itſelf, and ſo as to take fire by the acceſs of *air* only. Phoſphorus, therefore, when it emits light is properly a *body ignited*; though when a very ſmall quantity of it is uſed, as what is left after drawing it over paper, or what may be diſſolved in eſſential oil, the heat is not ſenſible. But perhaps the matter which emits the light in what we call *putreſcent ſubſtances* may be ſimilar to it, though it be generated by a different proceſs, and burn with a leſs degree of heat. Putreſcence does not ſeem to be neceſſary to the light of glow-worms, or of the pholades; and yet their light is ſufficiently ſimilar to that of ſhining wood or fleſh. Electric light is unqueſtionably ſimilar to that of phoſphorus, though the ſource of it is apparently very different.

The phoſphorus, the light of which I am now conſidering, was firſt made by one Brandt, a citizen of Hamburgh, in 1677, as he was purſuing ſome hints, which had been thrown out by the famous Kunckel, concerning the value of urine for the purpoſe of making the philoſopher's ſtone. Brandt, it is ſaid, communicated the ſecret to Kraft of Dreſden; but neither of them would acquaint Kunckel himſelf with it. This able chymiſt, however, going immediately to work, preſently diſcovered the very ſame thing that Brandt had done. A ſimilar phoſphorus was alſo made by Mr. Boyle, after ſeeing a piece of it in the hands of Kraft, who brought it to London in 1679, in order to ſhew it to the King and Queen of England, being only told that it was made of ſomething from the human body.*

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* Shaw's Travels, Fol. p. 363.

Kunckel formed his phoſphorus into a kind of pills, about the ſize of peas, which, being moiſtened a little, and ſcraped in the dark, yielded a very conſiderable light, but not without ſmoke. The light was much more pleaſing when eight or ten of theſe pills were put into a glaſs of water; for being ſhaken in the dark, the whole glaſs ſeemed to be filled with light. Kunckel alſo reduced his phoſphorus into the form of larger ſtones, which being warmed by a perſon's hand, and rubbed upon paper, would deſcribe letters that were very legible in the dark.§

The greateſt variety of experiments with the light of phoſphorus were made by Dr. Slare, who ſays that the liquid phoſphorus (which is nothing more than the ſolid phoſphorus diſſolved in any of the eſſential oils) would not hurt even a lady's hand, or that, when the hands or face were waſhed with it, it would not only make them viſible to other perſons in the dark; but that the light was ſo conſiderable, as to make other neighbouring objects viſible.

When the ſolid phoſphorus is quite immerſed in water, he obſerves that it ceaſes to ſhine; but that if any part of it chance to emerge, or get into the air, it will ſhine though the glaſs be hermetically ſealed. In a large glaſs he kept it without water for ſeveral days, and yet it continued ſhining, with very little diminution of its light or weight. If the letters that were written with this phoſphorus were warmed by the fire, they preſently became dark lines, which continued upon the paper, like ink. To try how much light was given by a ſmall quantity of this phoſphorus, he obſerved that it continued to flame in the open air for ſeven or eight days; the light being viſible whenever he ſhut his window.*

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* Ac. Par. 1737, M. p. 475. § Phil. Tranſ. ab. Vol. 3, p. 347.

As air was generally thought to contain the *pabulum* of flame, Dr. Slare was determined to try this with reſpect to phoſphorus; and for this purpoſe he placed a large piece of it in a receiver; but upon exhauſting it, he perceived that it became more luminous, and that, upon admitting the air, it returned to its former ſtate.† This property of the light of phoſphorus, which is the very reverſe of that of ſhining wood and fiſhes, was alſo aſcertained by ſeveral very accurate experiments of Mr. Haukſbee's.§

Endeavouring to blow the phoſphorus into a flame with a pair of bellows, Dr. Slare found that it was preſently blown out, and that it was a conſiderable time before the light revived again. All liquors would extinguiſh this light when the phoſphorus was put into them, nor would it ſhine, or burn, though it was even boiled in the moſt inflammable liquors, as oil of olives, ſpirit of turpentine, or even ſpirit of wine.

In order to keep his phoſphorus from conſuming, he uſed to put it in a glaſs of water, and ſometimes he has ſeen it, when thus immerſed in water, make ſuch bright and vigorous coruſcations in the air, as, he ſays, would ſurprize and frighten thoſe who are not uſed to the phenomenon. This fiery meteor, he ſays, is contracted in its paſſage through the water, but expands as ſoon as it gets above it. If any perſon would make this experiment to advantage, he informs them that the glaſs muſt be deep and cylindrical, and not above three quarters filled with water. This effect he perceived in warm weather only, and never in cold.*

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* Phil. Tranſ. ab. Vol. 3, p. 348. † Ib. p. 350. § Phyſico Mechanical Experiments, p. 122.

The phoſphorus of which I have been treating is prepared from urine, but, in ſome caſes, the *ſweat*, which is ſimilar to urine, has been obſerved to be phoſphoraceous, without any preparation. This once happened to a perſon who uſed to eat great quantities of ſalt, and who was a little ſubject to the gout, after ſweating with violent exerciſe. Stripping himſelf in the dark, his ſhirt ſeemed to be all on fire, which ſurpriſed him very much. Upon examination, red ſpots were found upon his ſhirt, and the phyſician who was preſent perceived an urinous ſmell, though it had nothing in it of volatile alkali, but of the muriatic acid; the ſame, he ſays, that iſſues from cabbage much ſalted, and ſtrongly fermenting.§

This phoſphoraceous light, and that which was ſeen round the body and bed of the woman at Milan, ſeem to ſhow ſome connection between the light of phoſphorus, and that of putreſcent ſubſtances, or the ignis fatuus.

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* Phyſico Mechanical Experiments. p. 351.

§ Acta Cæſarienſia, Vol. 5, p. 334.

SECTION X.

OBSERVATIONS ON THE RAINBOW.

HAVING, in the preceding ſections, given a view of all the diſcoveries that have been made concerning the *fundamental properties* of light, and of bodies reſpecting light, the remaining ſections of this period will relate, chiefly to what philoſophers have done towards the explanation of *appearances in nature,* and the phenomena of viſion, according to the laws already diſcovered.

Of all the optical appearances in nature, the *rainbow* is, perhaps, the moſt ſtriking. Accordingly, we have found that it has always engaged the attention of philoſophers; and we have ſeen new light thrown upon it in every ſucceſſive period of this hiſtory. And though all the capital circumſtances of this phenomenon were fully explained by Sir Iſaac Newton, yet ſome particulars were left to be obſerved in this period; eſpecially with reſpect to the varieties in which it has been exhibited, ſome of the more ſtriking, of which I ſhall give an account of in this ſection, together with the conjectures that have been made concerning their cauſes, as they cannot fail to give a pleaſure to my reader.

Of thoſe who adopted the principles of Sir Iſaac Newton, Dr. Halley was the firſt who took much pains with this ſubject of the rainbow. We have ſeen that the inner bow is formed by thoſe rays which ſuffer two refractions with one reflection between them, and that the outer bow is formed by two refractions, with two reflections between them. Here nature ſtops, or rather our organs are not ſenſible enough to perceive more rainbows; but the mind of man is able to purſue the ſubject much farther, and to aſcertain the dimenſions of thoſe rainbows which would be formed by 3, 4, or more reflexions, and Dr. Halley has diſcuſſed this ſubject in the Philoſophical Tranſactions for 1700, in which he alſo lays down a direct method for finding the diameter of the bow when the refraction is given.

The reſult of his inveſtigation was, that the firſt bow is produced by thoſe rays whoſe angle of incidence is ſuch, that the double of the refracted angle correſponding to it is as great as poſſible. In the ſecond bow the exceſs of the tripple of that angle is as great as poſſible. In the third bow, the quadruple, &c. ſo that the dimenſions of any poſſible bow may be determined, by finding an angle of incidence ſuch, as that a certain given multiple of the correſponding angle of refraction ſhall exceed it by a quantity the greateſt poſſible.

From theſe *data* he finds that the firſt bow has a radius of 42° 30′, the ſecond 51° 55′, both oppoſite to the ſun; but that the third would be 40° 20′, and the fourth 45° 33′, from that luminary, which is ſo near the ſun, that, on this account, probably, they are not viſible to us. At leaſt this muſt happen very rarely, and only in particular circumſtances. The ſame problem has, likewiſe, been conſidered by later mathematicians, particularly M. Herman Bernoulli, and the Marquis De Courtivron.* Another calculation of the angles into which light is refracted in the formation of the rainbow, was made by Dr. Pemberton, and may be ſeen Phil. Tranſ. ab. p. 141, &c.

I ſhall begin my account of remarkable rainbows with that which Dr. B. Languith deſcribes, who obſerved ſeveral ſeries of colours, which increaſed the rainbow to a breadth far exceeding what had been determined by calculation. The moſt perfect of theſe appearances was that which he ſaw Auguſt 21, 1722. About half an hour paſt five o'clock in the evening of that day, the weather being temperate, and the wind at North Eaſt, the colours of the primary rainbow were as uſual; only the purple was very much inclining to red, and well defined. Under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more duſky green. Under this were alternately, two arches of reddiſh purple, and two of green; and under all was a faint appearance of another arch of purple, which vaniſhed and returned ſeveral times ſo quick, that he could not fix his eyes ſteadily upon it. Thus, the order of the colours was

1. Red, orange, yellow, green, light blue, deep blue, purple.
2. Light green, dark green, purple.
3. Green, purple.
4. Green, faint vaniſhing purple.

So that here were four orders of colours, and perhaps the beginning of a fifth, for he made no queſtion, but that what he calls the purple was a mixture of the purple of each of the upper ſeries, with the red of the next below it; and that the green was a mixture of the intermediate colours. He wrote not this barely upon the credit of his own eyes, for there was a clergyman and four other gentlemen in company, whom he deſired to view the colours attentively, and all agreed that they appeared in the manner now deſcribed.

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* Montucla, Vol. 2, p. 652.

There are two things, he ſays, which will deſerve to be taken notice of, as they may perhaps direct us in ſome meaſure to the ſolution of this curious phenomenon.

1. That the breadth of the firſt ſeries ſo far exceeded that of any of the reſt. As near as he could judge, it was equal to them all taken together.

2. That he had never obſerved thoſe inner orders of colours in the lower parts of the rainbow, though they were often incomparably more vivid than the upper parts, under which the colours appeared. He took notice of this ſo very often, that he thought it could hardly be conſidered as accidental, and that, if it ſhould prove true in general, it would bring the diſquiſition into a narrow compaſs, as it would ſhew that this effect depends upon ſome property which the drops retain whilſt they are in the upper part of the air, but which they loſe as they come lower, and are more mixed with one another.*

M. Bouguer frequently ſaw this phenomenon when he was upon the mountains of Peru, where the ſky is often extremely ſerene. It appeared to him that this circumſtance of ſerenity was neceſſary on the ſide of the ſun; but that it was more neceſſary that the ſky ſhould be quite dark on the oppoſite ſide. The colours of the ſecond bow were in the ſame order as thoſe of the firſt, and the red of the ſecond was well ſeparated from the violet of the other; ſo that the two bows were better diſtinguiſhed than they were when Dr. Langwith ſaw them. M. Bouguer thought that he could ſometimes perceive faint traces of a third bow, immediately within the ſecond.*

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* Phil. Tranſ. ab. Vol. 7, p. 105.

The extraordinary rainbow which was ſeen by M. L. Gentil, the 18th of November 1756, ſeems to have been of the ſame nature of this which was obſerved by Dr. Langwith. Under the two common bows, he ſaw two other large bows, one below the other. The upper of theſe immediately touched the violet of the common bow. Theſe two bows were ſomething farther from one another than their breadth, which might be a little more than the third of the breadth of the inner bow. They were blue, and of the ſame luſtre with the blue of the inner bow. He alſo thought that the ſpace which they occupied was nearly of the ſame extent with the inner bow.†

Upon another occaſion the ſame gentleman and M. De Fouchy ſaw, under the violet of the common bow, a ſpace without colour, as broad as the green and blue taken together, and after that a lively green, as lively as that of the bow.§

Dr. Pemberton has attempted the explanation of this remarkable appearance on the Newtonian doctrine, of the fits of eaſy reflection and tranſmiſſion; but he goes upon the ſuppoſition that the differently coloured rays have their ſeparate fits, on their arrival at the ſurface of any medium, without any regard to the thickneſs of it; whereas it is plain that, in ſuch a caſe, all kinds of rays are reflected or tranſmitted promiſcuouſly.

It is moſt probable that theſe colours are formed in very minute drops of water, or vapour, intermixed with the larger drops, that their formation depends upon the ſame principle with the colours of *thin plates*, and that they are ſimilar to thoſe of ſeveral kinds of halo's. Dr. Pemberton himſelf obſerves, that it is moſt likely, that theſe additional rings of colours are formed in the vapour of the cloud, which the air, being put in motion by the fall of the rain, may carry down along with the larger drops. This, he ſays, may be the reaſon why theſe colours appear under the upper part of the bow only, this vapour not deſcending very low. As a farther confirmation of this, theſe colours, he obſerves, are ſeen ſtrongeſt when the rain falls from very black clouds, which cauſe the fierceſt rains; by the fall of which the air will be moſt agitated.*

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* Ac. Par, 1757. M. 62. † Ib. p. 59. § Ib. p. 61.

Dr. Langwith once ſaw a rainbow lying on the ground, the colours of which were almoſt as lively as thoſe of the common rainbow. It was extended ſeveral hundred yards, and the colours were ſo ſtrong, that it might have been ſeen much farther, if it had not been terminated by a bank, and the hedge of a field. The more remarkable particulars in this bow were, 1. that the figure of it was not round, but oblong; being, as he conceived, the portion of an hyperbola. 2. that the convex part of it was turned towards the eye, and the vertex at a ſmall diſtance before him; and 3dly that the colours took up leſs ſpace, and were much more lively in thoſe parts of the bow that were near him, than in thoſe which were at a diſtance.

Theſe phenomena he endeavoured to explain by comparing this bow DEC, fig. 140, with the common rainbow, *kei*E, formed by drops falling in the air, at a ſmall diſtance from the eye of the ſpectator at H, and touching the ground with the lower part of its arch in E, the vertical point of the rainbow DEC. Produce, ſays he, the cone H *iek* E, and its interſection with the plane of the horizon will give the figure of the bow DEC. Hence it follows that, as the angle *e* HG happens to be greater than, equal to, or leſs than 90 degrees, the figure will be an hyperbola, parabola, or ellipſis. 2. That as the ſun was about 30 degrees high when the phenomenon was ſeen, the bow was an hyperbola. 3. That the arches of the ſame bow, conſiſting of colours of different degrees of refrangibility, may alſo, in ſome caſes, be different ſections of the cone. 4. Since the angle *e* HF is always given, from the height of the point of view, HG and the ſun's altitude SLA, the dimenſions of theſe bows are eaſily determined.*

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* Phil. Tranſ. ab. Vol. 6 p. 140.

The inverted arches which generally accompany parhelia, M. Weidler ſays, have been ſometimes ſeen without them; and he takes notice that M. Tachard, in his account of his voyage to Siam, always obſerved that when two waves daſhed againſt one another, ſo as to throw up ſmall drops of water, if his eye was elevated a little above them, he always ſaw an inverted bow; whereas when the drops fell back again, or when rain deſcended in the ſame place, there was a rainbow in its uſual poſition. §

Appearances like theſe Deſcartes explained by the reflexion of the rays of the ſun from the ſurface of the water, but M. Weidler thinks this account inſufficient, and ſays that the appearance will be produced whenever the rays of the ſun (which fall upon drops of water, ſo that they would have come to the axis of the rainbow after two refractions and one reflexion) fall upon the eye, placed beyond this point of the concourſe of the rays with the axis, under half that angle. This account is very general and obſcure, and indeed the author ſeems not to have examined it thoroughly, as he gives no figures, to ſhew how, by the progreſs of the rays, theſe appearances may be produced in the manner that he deſcribes.

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* Phil. Tranſ. ab Vol. 7, p. 103.

§ Commentarius, p. 30.

M. Celſius deſcribes a third rainbow, larger than the two others, and interſecting them, which he ſaw in Dalecarlia in 1743; and which appears, from the circumſtances, to have been produced by the reflexion of the ſun from a river. Similar to this was that which Dr. Halley ſaw at Cheſter in 1698, and which he accounts for in the ſame manner. The perſon who firſt ſuggeſted this method of explaining theſe excentric bows was M. Eſtienne, Canon of Chartres, who, after having deſcribed a rainbow cut by a kind of broken and circular chevron, of the ſame nature as the primary rainbow, but weaker with reſpect to its colours, obſerves that, when he made the obſervation, the river Chartres was betwixt him and the bow, and upon the ſame level, about 150 paces from him. This obſervation was made the tenth of Auguſt 1665 at half paſt ſix in the evening.*

M. G. Edwards deſcribes a rainbow which muſt have been formed by the exhalations from the city of London, when the ſun had been ſet twenty minutes, and conſequently the center of the bow above the horizon. The colours were the ſame as in the common rainbow, but fainter.§

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* Ac. Par. 1743, H. p. 54.

§ Phil. Tranſ. Vol. 50, p. 294.

SECTION XI.

OBSERVATIONS CONCERNING HALO'S AND PARHELIA.

AT the time that philoſophers firſt attended to the phenomena of the rainbow, other appearances in the atmoſphere, as *halo*'s and *parhelia*, did not paſs unobſerved by them; but as they are rare, and conſiderably various, it could not be expected that philoſophers, in the early periods of my hiſtory, who had few opportunities of making obſervations upon them, ſhould have the ſame ſucceſs in their attempts to account for them. I have, therefore, generally omitted their remarks on this ſubject, and have reſerved them for an introduction to the beſt account I have been able to collect of theſe remarkable appearances, unſatisfactory as, after all, I am ſenſible it will be. I ſhall acquit myſelf in the beſt manner that I am capable of, upon this intricate ſubject, by relating, in the firſt place, ſome of the more remarkable facts, and then reciting the opinions of the moſt eminent philoſophers upon the ſubject.

Corona's, or *halo*'s, are luminous circles, ſurrounding the ſun, the moon, the planets, or fixed ſtars. Sometimes they are white, and ſometimes coloured, like the rainbow. Sometimes one only is viſible, and ſometimes ſeveral concentric corona's make their appearance at the ſame time. Thoſe which have been ſeen about Sirius and Jupiter were never more than three, four, or five degrees in diameter; thoſe which ſurround the moon are, alſo, ſometimes no more than three or five degrees; but theſe, as well as thoſe which ſurround the ſun, are of very different magnitudes, viz. of 12°. 0′, 22°. 35′, 30°. 0′, 38°. 0′, 41°. 2′, 45°. 0′, 46°. 24′, 47°. 0′, and 90°. or even larger than this. Their diameters alſo ſome times vary during the time of obſervation, and the breadths both of the coloured and white circles are very different, viz. of 2, 4, or 7 degrees.

The *colours* of theſe corona's are more dilute than thoſe of the rainbow; and they are in a different order, according to their ſize. In thoſe which Newton obſerved in 1692, and of which a more particular account will be given hereafter, they were in the following order, reckoning from the inſide. In the innermoſt were blue, white, and red; in the middle were purple, blue, green, yellow, and pale red; in the outermoſt pale blue, and pale red. M. Huygens obſerved red next the ſun, and a pale blue outwards. Sometimes they are red on the inſide, and white on the outſide. M. Weidler obſerved one that was yellow on the inſide, and white on the outſide. In France one was obſerved in 1683, the middle of which was white; after which followed a border of red, next to it was blue, then green, and the outermoſt circle was a bright red. In 1728 one was ſeen of a pale red outwardly, then followed yellow, and then green, terminated by white.

Theſe corona's are very frequent. In Holland M. Muſchenbroeck ſays, fifty may be ſeen in the day time, almoſt every year; but they are not much noticed, becauſe people ſeldom look towards the ſun; and, beſides, they can hardly be ſeen except the eye be ſo ſituated, that not the body of the ſun, but only the neighbouring parts of the heavens can be ſeen. Mr. Middleton ſays, that this phenomenon is very frequent in North America; for that there is generally one or two about the ſun every week, and as many about the moon every month.* Halo's round the ſun are very frequent in Ruſſia. M. Æpinus ſays that, from the twenty-third of April 1758, to the twentieth of September, he himſelf had obſerved no leſs than twenty-ſix, and that he has ſometimes ſeen twice as many in the ſame ſpace of time.†

Corona's may be produced by placing a lighted candle in the midſt of ſteam in cold weather. Alſo, if glaſs windows be breathed upon, and the flame of a candle be placed ſome feet from it, while the ſpectator is alſo at the diſtance of ſome feet from another part of a window, the flame will be ſurrounded with a coloured halo. And if a candle be placed behind a glaſs receiver, when air is admitted into the vacuum within it, at a certain degree of denſity, the vapour with which it is loaded will make a coloured halo round the flame. This was obſerved by Otto Guericke.§ In December 1756, M. Muſchenbroeck obſerved that, when the glaſs windows of his room were covered with a thin plate of ice, on the inſide, the moon appearing through it was ſurrounded with a large and variouſly coloured halo; and, opening the window, he found that it aroſe intirely from that thin plate of ice, for none was ſeen, except through it.||

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* Muſchenbroeck's Introductio, Vol. 2, p. 1036. † Nov. Com. Petrop. Vol. 8, p. 392. § Experimenta Magdeburgica, p. 89. || Introductio, p. 1046.

Similar, in ſome reſpects, to the halo, was the remarkable appearance which M. Bouguer deſcribes, as obſerved by himſelf and his companions, on the top of Mount Pichinca, in the Cordeliers. I ſhall content myſelf with a bare recital of the facts, ſince neither the ingenious obſerver, nor any of the perſons who have repeated his account of it have attempted any explanation.

When the ſun was juſt riſing behind them, and a cloud was about thirty paces before them, ſo as to appear white, each of them ſaw his own ſhadow projected upon it, and no other. The diſtance was ſuch, that all the parts of the ſhadow were eaſily diſtinguiſhable, as the arms, the leg, and the head; but what ſurpriſed them moſt, was, that the head was adorned with a kind of glory, conſiſting of three or four ſmall concentric crowns, of a very lively colour, each exhibiting all the varieties of the primary rainbow, and having the circle of red on the outſide.

The intervals between theſe circles continued equal, though the diameters of them all were conſtantly changing. The laſt of them was very faint, and, at a conſiderable diſtance was another great white circle, which ſurrounded the whole.

As near as M. Bouguer could compute, the diameter of the firſt of theſe circles was about 5 $\frac{2}{3}$ degrees, that of the ſecond 11; that of the third 17, and ſo on; but the diameter of the white circle was about 67 degrees. This phenomenon never appeared but in a cloud, conſiſting of frozen particles, and never in drops of rain, like the rainbow. When the ſun was not in the horizon, only part of the white circle was viſible, as M. Bouguer frequently obſerved afterwards.*

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* Ac. Par. 1744, M. p. 366.

Similar alſo, to this curious appearance, was one that was obſerved by Dr. M' Fait in Scotland. This gentleman obſerved a rainbow round his ſhadow in the miſt, when he was upon an eminence above it. In this ſituation the whole country round ſeemed, as it were, buried under a vaſt deluge, and nothing but the tops of diſtant hills appeared here and there, above the flood; ſo that a man would think of diving down into it with a kind of horror. In thoſe upper regions the air, he ſays, is, at that time, very pure and agreeable to breath in. At another time he obſerved a double range of colours round his ſhadow in theſe circumſtances. The colours of the outermoſt range were broad, and very diſtinct, and every where about two feet diſtant from the ſhadow. Then there was a darkiſh interval, and after that another narrower range of colours, cloſely ſurrounding the ſhadow, which was very much contracted. This perſon ſeems to think that theſe ranges of colours are cauſed by the inflection of the rays of light, the ſame that occaſioned the ring of light which ſurrounds the ſhadows of all bodies, obſerved by M. Maraldi, and this author.*

Such are the *facts* which I have been able to collect on the ſubject of halo's, and they are, certainly, extremely curious, and pleaſing. I wiſh it were in my power to give my reader as much pleaſure with reſpect to the *cauſe* of theſe appearances. I would obſerve, in general, that the prodigious variety with which they are exhibited ſeems to ſhew that many of them do not reſult from the general laws of reflexion, refraction, or inflection, belonging to tranſparent ſubſtances of a large maſs, but upon the alternate reflexion and tranſmiſſion of the different kinds of rays, peculiar to ſubſtances reduced to the form of thin plates, or conſiſting of ſeparate and very minute parts. But where the dimenſions of the corona's are pretty conſtant, as in the uſual and larger halo, which is about half the diameter of the rainbow, they may, perhaps, be explained on the general principles of refraction only. If, however, I be not able to give my reader intire ſatisfaction in this reſpect, I ſhall endeavour to amuſe him, and at the ſame time put him upon thinking for himſelf, by preſenting him with a ſeries of opinions on this ſubject.

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* Edinb. Eſſays, Vol. 1, p. 198.

Deſcartes obſerves, that the halo never appears when it rains, from which he concludes that this phenomenon is occaſioned by the refraction of light in the round particles of ice, which are then floating in the atmoſphere; and though theſe particles are flat when they fall to the ground, he thought they muſt be purtuberant in the middle, before their deſcent; and according to this pertuberancy he imagined that the diameter of the halo would vary.*

In treating of meteors, Gaſſendi ſuppoſed that a halo is the ſame thing with the rainbow, the rays of light being, in both caſes, twice refracted and once reflected, within each drop of rain or vapour, and that all the difference there is between them ariſes from their different ſituation with reſpect to the obſerver. For, whereas, when the ſun is behind the ſpectator, and conſequently the rainbow before him, his eye is in the *center* of the circle; when he views the halo, with his face towards the ſun, his eye is in the *circumference* of the circle; ſo that, according to the known principles of geometry, the angle under which the object appears in this caſe, muſt be juſt half of what it is in the other.[*] Though this writer ſays a great deal upon the ſubject, and endeavours to give reaſons why the colours of the halo are in a different order to thoſe of the rainbow, he does not deſcribe the progreſs of the rays of light from the ſun, to the eye of the ſpectator, when a halo is formed by them, and he gives no figures to explain his ideas.

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* Dioptrics, p. 230.

Dechales, alſo, endeavours to ſhow that the generation of the halo is ſimilar to that of the rainbow. If, ſays he, a ſphere of glaſs or cryſtal, AB. fig. 141, full of water, be placed in the beams of the ſun ſhining from C, there will not only be two circles of coloured light, on the ſide next the ſun, and which conſtitute the two rainbows; but there will alſo be another on the part oppoſite to the ſun, the rays belonging to which meeting at E, afterwards diverge, and form the coloured circle G, as will be viſible, if the light that is tranſmitted through the globe be received on a piece of white paper. The colours alſo will appear to an eye placed in any part of the ſurface of the cone FEG. Meaſuring the angle FEH, he found it to be 23 degrees. They were only the extreme rays of this cone that were coloured like thoſe of the rainbow.

This experiment he thought ſufficiently illuſtrated the generation of the halo, ſo that whenever the texture of the clouds is ſuch, as not entirely to intercept the rays of the ſun or moon, and yet have ſome degree of denſity, there will, always be an halo round them, the colours of the rainbow appearing in thoſe drops which are 23 degrees diſtant from the ſun or moon. If the ſun be at A, fig. 142, and, the ſpectator in B, the halo will be the circle DFE, DBE being 46 degrees, or twice 23.

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[*] Gaſſendi Opera, Vol. 2, p. 103.

The reaſon why the colours of the halo are more dilute then thoſe of the rainbow, he ſays, is owing principally to their being formed not in large drops of rain, but in very ſmall vapour; for if the drops of water were large, the cloud would be ſo thick, that the rays of the ſun could not be regularly tranſmitted through them; and, on the other hand, he had obſerved, that when the rainbow is formed by very thin vapours, the colours hardly appear.*

As for thoſe circles of colours which are ſometimes ſeen round candles, it was his opinion that they are owing to nothing but moiſture on the eye of the obſerver; for that he could never produce this appearance by means of vapour only, if he wiped his eyes carefully; and he had obſerved that ſuch circles are viſible to ſome perſons and not to others, and to the ſame perſons at one time and not at another.

The moſt conſiderable of all the theories reſpecting halo's, and that which has met with the moſt favourable and the longeſt reception, is that of M. Huygens. Sir Iſaac Newton mentions it with reſpect, and Dr. Smith, in his *Complete ſyſtem of optics*, does not ſo much as hint at any other. On this account, my reader will expect a fuller account of it than I myſelf ſhould, otherwiſe, think it deſerved.

The occaſion of M. Huygens publiſhing his thoughts on this ſubject was the appearance of a halo at Paris, on the 12th of May 1667, of which he gave an account in a paper read at the Royal Academy in that city, which was afterwards tranſlated, and publiſhed in the Engliſh Philoſophical tranſactions; and which may be ſeen in Lowthorp's Abridgment, Vol. 2, p. 189.

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* Curſus Mathematicus, Vol. 3, p. 758.

But this article contains nothing more than the heads of a diſcourſe, which he afterwards compoſed, but never quite finiſhed on this ſubject; and which has been tranſlated, with ſome additions, by Dr. Smith, from whom the following account is, chiefly, extracted.

Our philoſopher had been firſt engaged to think particularly upon this ſubject by the appearance of five ſuns at Warſaw, in 1658, preſently after which, he ſays, he hit upon the true cauſe of halo's, and not long after of that of mock ſuns alſo.

To prepare the way for the following obſervations, it muſt be remarked, that if we can conceive any kind of bodies in the atmoſphere, which, according to the known laws of opticks, will, either by means of reflexion or refraction, produce the appearance in queſtion, when nothing elſe can be found that will do it, we muſt acquieſce in the hypotheſis, and ſuppoſe ſuch bodies to exiſt, even though we cannot give a ſatisfactory account of their generation. Now two ſuch bodies are aſſumed by M. Huygens, one of them a round ball, opaque in the center, but covered with a tranſparent ſhell, and the other is a cylinder, of a ſimilar compoſition. By the help of the former he endeavours to account for halo's, and by the latter for thoſe appearances which are called mock ſuns. Thoſe bodies which M. Huygens requires, in order to explain theſe phenomena are not, however, a mere aſſumption; for ſome ſuch, though of a larger ſize than his purpoſe requires, have been actually found, conſiſting of ſnow within, and ice without. They are particularly mentioned by Deſcartes. I ſhall begin with our philoſopher's account of the halo.

The balls with the opaque kernel, which he ſuppoſed to have been the cauſe of them, he imagines not to exceed the ſize of a turnip ſeed; but, in order to illuſtrate this hypotheſis, he gives a figure of one, of a larger from ABCD, fig. 143, EF repreſenting the kernel of ſnow in the middle of it. If the rays of light, coming from GH, fall upon the ſide AD, it is manifeſt they will be ſo refracted at A and D, as to bend inwards; and many of them will ſtrike upon the kernel EF. Others, however, as GA, and HD, will only touch the ſides of the kernel; and being again refracted at B and C, will emerge in the lines BK, CK, croſſing each other in the point K, whoſe neareſt diſtance from the globule is ſomewhat leſs than its apparent diameter. If, therefore, BK and CK be produced towards M and L, fig. 144, it is evident that no light can reach the eye placed within the angle LKM, but may fall upon it when placed out of that angle, or rather the cone repreſented by it.

For the ſame reaſon, every other of theſe globules will have a ſhadow behind it, in which the light of the ſun will not be perceived. If the eye be at N, and that be conceived to be the vertex of a cone, the ſides of which NR, NQ, are parallel to the ſides of the former cone KL, KM, it is evident that none of the globules within the cone QNR can ſend any rays of the ſun to the eye at N. But any other globule out of this cone, as X, may ſend thoſe rays, which are more refracted than XZ to the eye; ſo that this will appear enlightened, while thoſe within the cone will appear obſcure. It is evident from this, that a certain area, or ſpace, quite round the ſun, muſt appear dark; and that the ſpace next to this area will appear luminous, and more ſo in thoſe parts that are neareſt to the obſcure area; becauſe, he ſays, it may eaſily be demonſtrated, that thoſe globules which are neareſt to the cone QNR exhibit the largeſt image of the ſun.

It is plain, alſo, that a corona ought to be produced in the ſame manner, whatever be the ſun's altitude, becauſe of the ſpherical figure of the globules.

To verify this hypotheſis, our philoſopher adviſes us to expoſe to the ſun a thin glaſs bubble, filled with water, and having ſome opaque ſubſtance in the center of it; and he ſays we ſhall find, that we ſhall not be able to ſee the ſun through it, unleſs at a certain diſtance from a place oppoſite to the center of it; but as ſoon as we do perceive the light, the image of the ſun will immediately appear the brighteſt, and coloured red, for the ſame reaſon as in the rainbow.

Theſe corona's, he ſays, often appear about the moon; but the colours are ſo weak as to appear only white. Such white corona's he had alſo ſeen about the ſun, when the ſpace within them appeared ſcarce darker than that without. This he ſuppoſes to happen when there are but few of thoſe globules in the atmoſphere; for the more plentiful they are, the more lively the colours of the halo appear; at the ſame time alſo the area within the corona will be the darker. The apparent diameter of the corona, which is generally about 45 degrees, depends upon the ſize of the dark kernel; for the larger it is with reſpect to the whole globule, the larger will be the dark cone behind it.

The globules that form theſe halo's, Mr. Huygens ſuppoſes to have conſiſted of ſoft ſnow, and to have been rounded by continual agitation in the air, and thawed on their outſides by the heat of the ſun.

To make the diameter of the halo 45 degrees, he demonſtrates that the ſemi-diameter of the globule muſt be to the ſemi-diameter of the kernel of ſnow very nearly as 1000 to 480; and that to make a corona of 100 degrees, it muſt be as a 1000 to 680.

Mr. Weidler, in his *Commentary on Parhelia*'s, publiſhed at Wittemburgh in 1738, obſerves that it is very improbable that ſuch globes as Mr. Huygens' hypotheſis requires, with nuclei of ſuch a preciſe proportion, ſhould exiſt; and if there were ſuch bodies, he thinks they would be too ſmall to produce the effects aſcribed to them.

Beſides, he obſerves that appearances exactly ſimilar to halo's are not uncommon, where fluid vapours alone are concerned, as when a candle is placed behind the ſteam of boiling water in froſty weather, or in the midſt of the vapour iſſuing copiouſly from a bath, or behind a receiver, when the air is ſo much rarefied as to be incapable of ſupporting the water it contains.*

The rays of the ſun twice reflected and twice refracted within ſmall drops of water are ſufficient, he ſays, without any opaque kernel, to produce all the appearances of the halo's that have the red light towards the ſun, as may be proved by experiment. That the diameter of the halo is generally half of that of the rainbow, he accounts for as Gaſſendi did before him.

M. Mariotte accounts for the formation of the *ſmall corona*'s by the tranſmiſſion of light through aqueous vapours, where it ſuffers two refractions, without any intermediate reflection. He ſhews that light which comes to the eye, after being refracted in this manner, will be chiefly that which falls upon the drop nearly perpendicular; becauſe more rays fall upon any given quantity of ſurface in that ſituation, fewer of them are reflected with ſmall degrees of obliquity, and they are not ſo much ſcattered after refraction.* The red will always be outermoſt in theſe corona's, as conſiſting of rays which ſuffer the leaſt refraction. And whereas he had ſeen, when the clouds were driven briſkly by the wind, halo's round the moon, varying frequently in their diameter, being ſometimes of two, ſometimes of three, and ſometimes of four degrees; ſometimes alſo being coloured, ſometimes only white, and ſometimes diſappearing intirely; he concluded that all theſe variations aroſe from the different thickneſs of the clouds, through which ſometimes more and ſometimes leſs light was tranſmitted. He ſuppoſed, alſo, that the light which formed them might ſometimes be reflected, and at other times refracted.†

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* Commentarius, p. 24.

As to thoſe corona's which conſiſt of two orders of colours, he imagined that they were produced by ſmall pieces of ſnow, which when they begin to diſſolve, form figures which are a little convex towards their extremities. Sometimes, alſo, the ſnow will be melted in different ſhapes; and in this caſe, the colours of ſeveral halo's will be intermixed, and confuſed; and ſuch, he ſays, he had ſometimes obſerved round the ſun.§

M. Mariotte then proceeds to explain the *larger corona*'s, namely thoſe that are about 45 degrees in diameter, and for this purpoſe he has recourſe to equiangular priſms of ice, in a certain poſition with reſpect to the ſun; and he takes pains to trace the progreſs of the rays of light for this purpoſe; but this hypotheſis is ſo improbable, that I ſhall eaſily be excuſed for not following him in his inveſtigation. In ſome caſes he thought that theſe large corona's were cauſed by hail-ſtones, of a pyramidal figure; becauſe, after two or three of them had been ſeen about the ſun, there fell the ſame day ſeveral ſuch pyramidal hail-ſtones.* M. Mariotte explains *parhelia* by the help of the ſame ſuppoſitions.

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* Mariotte's Works, p. 268. † Ib. p. 270. § Ib. 270.

Sir Iſaac Newton does not appear to have given any particular attention to the ſubject of halo's, but he has hinted at his ſentiments concerning them occaſionally; by which we perceive that he conſidered the larger and leſs variable appearances of this kind as produced according to the common laws of refraction, but that the leſs and more variable appearances depend upon the ſame cauſe with the colours of thin plates.

He concludes his explication of the rainbow with the following obſervation on halo's and parhelias. The light which comes through drops of rain by two refractions, without any reflexion, ought to appear the ſtrongeſt at the diſtance of about 26 degrees from the ſun, and to decay gradually both ways, as the diſtance from him increaſes; and the ſame is to be underſtood of light tranſmitted through ſpherical hail-ſtones; and if the hail be a little flatted, as it often is, the tranſmitted light may be ſo ſtrong, at a little leſs diſtance than that of 26 degrees, as to form a halo about the ſun or moon; which halo, as often as the hail-ſtones are duly figured, may be coloured, and then it muſt be red within by the leaſt refrangible rays, and blue without by the moſt refrangible ones; eſpecially if the hail-ſtones have opaque globules of ſnow in their centers to intercept the light within the halo, as M. Huygens has obſerved, and make the inſide of it more diſtinctly defined than it would otherways be. For ſuch hail-ſtones, though ſpherical, by terminating the light by the ſnow, may make a halo red within and colourleſs without, and darker within the red than without, as halo's uſe to be. For of thoſe rays which paſs cloſe by the ſnow, the red making ones will be the leaſt refracted, and ſo come to the eye in the ſtraighteſt lines.*

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* Marriotte's Works, p. 275.

Some farther thoughts of Sir Iſaac Newton's, on the ſubject of halo's we find ſubjoined to the account of his experiments on the colours of *thick plates of glaſs*, which, as we have ſeen, he conceived to be ſimilar to thoſe which are exhibited by *thin ones*. As light reflected by a lens quick ſilvered on the back ſide makes the rings of colours above deſcribed, ſo he ſays, it ought to make the like rings in paſſing through a drop of water. At the firſt reflexion of the rays within the drop, ſome colours ought to be tranſmitted, as in the caſe of a lens, and others to be reflected back to the eye. For inſtance, if the diameter of a ſmall drop or globule of water be about the 500th part of an inch, ſo that a red making ray, in paſſing through the middle of this globule, has 250 fits of eaſy tranſmiſſion within the globule, and all the red making rays which are at a certain diſtance from this middle ray round about it have 249 fits within the globule, and all the like rays at a certain farther diſtance round about it have 248 fits, and all thoſe at a certain farther diſtance 247 fits, and ſo on, theſe concentric circles of rays, after their tranſmiſſion, falling on a white paper, will make concentric rings of red upon the paper; ſuppoſing the light which paſſes through one ſingle globule ſtrong enough to be ſenſible, and in like manner, the rays of other colours will make rings of other colours. Suppoſe now that in a fair day the ſun ſhould ſhine through a thin cloud of ſuch globules of water or hail, and that the globules are all of the ſame ſize, the ſun ſeen through this cloud ought to appear ſurrounded with the like concentrick rings of colours, and the diameter of the firſt ring of red ſhould be $7\frac{1}{4}$ degrees, that of the ſecond $10\frac{1}{4}$, that of the third 12°, 33′, and according as the globules of water are bigger or leſs, the ring ſhould be leſs or bigger.

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* Newton's Opticks, p. 155.

This curious theory our author informs us was confirmed by an obſervation which he made in June 1692. He ſaw by reflexion, in a veſſel of ſtagnating water, three halo's, crowns, or rings of colours about the ſun, like three little rainbows concentric to his body. The colours of the firſt, or innermoſt crown, were blue next the ſun, red without, and white in the middle, between the blue and red. Thoſe of the ſecond crown were purple and blue within, and pale red without, and green in the middle. And thoſe of the third were pale blue within, and pale red without. Theſe crowns incloſed one another immediately, ſo that their colours proceeded in this continual order from the ſun outward; blue, white, red; purple, blue, green, pale yellow, and red; pale blue, pale red. The diameter of the ſecond crown, meaſured from the middle of the yellow and red on one ſide of the ſun, to the middle of the ſame colour on the other ſide, was $9\frac{1}{3}$ degrees, or thereabouts. The diameters of the firſt and third he had not time to meaſure, but that of the firſt ſeemed to be about five or ſix degrees, and that of the third about twelve. The like crowns appear ſometimes about the moon. For in the beginning of the year 1664, on February 19th at night, he ſaw two ſuch crowns about her. The diameter of the firſt, or innermoſt, was about three degrees, and that of the ſecond about five degrees and a half. Next about the moon was a circle of white, and next about that the inner crown, which was of a bluiſh green within, next the white, and of a yellow and red without; and next about theſe colours were blue and green on the inſide of the outer crown, and red on the outſide of it.

At the ſame time there appeared a halo at the diſtance of about 22°. 35′ from the center of the moon. It was elliptical, and its long diameter was perpendicular to the horizon, verging below fartheſt from the moon. He was told that the moon has ſometimes three or more concentric crowns of colours encompaſſing one another next about her body. The more equal the globules of water or ice are to one another, the more crowns of colours will appear, and the colours will be the more lively. The halo, at the diſtance of 22 ½ degrees from the moon, is of another ſort. By its being oval, and more remote from the moon below than above, he concludes that it was made by refraction in ſome kind of hail or ſnow floating in the air in an horizontal poſture, the refracting angle being about 50 or 60 degrees.* Dr. Smith, however, makes it ſufficiently evident, that the reaſon why this halo appeared oval, and more remote from the moon towards the horizon, is a deception of ſight, and the ſame with that which makes the moon, appear larger in the horizon.

Dr. Kotelnihow, having, like Dr. Halley, made very accurate obſervations to determine the number of poſſible rainbows, conſidered the coloured halo which appears about a candle as the ſame thing with one of theſe bows which is formed near the body of the ſun, but which is not viſible on account of his exceſſive ſplendor.§

Laſtly, M. Muſchenbroeck concludes his account of corona's with obſerving, that ſome denſity of vapour, or ſome thickneſs of the plates of ice divides the light in its tranſmiſſion either through the ſmall globules of water, or their interſtices, into its ſeparate colours; but what that denſity was, or what was the ſize of the particles which compoſed the vapour, he could not pretend to determine.*

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* Newton's Opticks, p. 288. § Nov. Com. Petrop. Vol. 7, p. 276.

Theſe are all the obſervations worth much notice that I have been able to collect on the ſubject of corona's or halo's. I muſt now proceed to the conſideration of a more ſplendid phenomenon, or rather a train of phenomena; with the ſight of which we are but ſeldom favoured. I mean *parhelia*, and the circles which accompany them.

Mention is made of PARHELIA both by the ancients and the moderns. Ariſtotle relates that two were ſeen in Boſphorus, from morning to evening, though, in general, he obſerves, they are not ſeen except when the ſun is near the horizon. Pliny has related under what conſuls this phenomenon was ever ſeen at Rome. Gaſſendi ſays that, in 1635 and 1636, he often ſaw one mock-ſun. In 1689 M. De la Hire ſaw two, as did Caſſini in 1693, Mr. Grey in 1700, and Dr. Halley in 1702; but the moſt celebrated appearance of this kind is that which was ſeen at Rome by Scheiner, in which there were four mock ſuns. The ſame number, however, was ſeen by M. Muſchenbroeck at Utrecht, and ſeven ſuns were ſeen by Hevelius at Sedan, in 1661.

The apparent ſize of parhelia is the ſame as that of the true ſun; but they are not always round, and alſo they are not always, though they are ſometimes ſaid to be, as bright as the true ſun. When there are a number of them, ſome are not ſo bright as others. Externally they are tinged with colours, like the rainbow, and many have a long fiery tail oppoſite to the ſun, but paler towards the extremity. Dr. Halley obſerved one which had tails extending both ways, and ſuch a one alſo M. Muſchenbroeck obſerved in 1753, the tails being in a right line drawn through both the ſuns. Both of them, alſo, were in coloured circles. M. Weidler ſaw a parhelion with one tail pointing upwards and another downwards, a little crooked; the external limb, with reſpect to the ſun, being of a purple colour, and on the other ſide it was tinged with the colours of the rainbow. The tails of theſe parhelia, for the moſt part, appear in a white horizontal circle.*

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* Introductio, p. 1037.

Corona's generally accompany parhelia, ſome tinged with the colours of the rainbow, and others white. They differ in number and ſize, but they are all of the ſame breadth, which is that of the apparent diameter of the ſun.

A very large *white circle*, parallel to the horizon, generally paſſes through all the parhelia; and, if it were intire, it would go through the center of the ſun. Sometimes there are arcs of leſſer circles concentric to this, touching thoſe coloured circles which ſurround the ſun. They are alſo tinged with colours, and contain other parhelia. There are alſo ſaid to have been other circles obliquely ſituated with reſpect to all thoſe we have mentioned; but of this I have met with no authentic account. The order of the colours, in theſe circles, is the ſame as in the rainbow; but on the inſide, with reſpect to the ſun, they are red, as is alſo obſerved in many other corona's.

M. Feuillée ſaw a circle parallel to the horizon tinged with brighter colours than thoſe of the other circle which had the ſun in its center, and the diameter of this parallel circle was only one half of the other.

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* Introductio, p. 1039

A remarkable appearance of this kind was ſeen by M. Muſchenbroeck at Leyden in 1753. There was only one parhelion, but it had three tails, two of them parallel to the horizon, and the third pointing towards the zenith, and which he ſuppoſed might poſſibly be a portion of a circle. It was twelve degrees in length, tinged with all the colours of the rainbow, very vivid, and eaſily diſtinguiſhed from one another. The red was next the ſun, and the violet outwards. Towards the zenith there was a coloured arc, which was one fourth part of a circle, parallel to the horizon, 50 degrees in diameter. The breadth of this arc was the ſame as that of the primary rainbow, but the gibbous part was next to the ſun, 47 degrees diſtant from it. The colours were ſeven, as in the rainbow, but ſo bright, that they were more eaſily diſtinguiſhed from one another than by the help of the beſt priſm. The red of this circle was outwards, that is next the ſun, and the violet inwards towards the center. The heavens were covered at the time with a ſlight fog, with ſmall white clouds interſperſed, and there was no wind.

In 1753, a remarkable appearance, ſimilar, as M. Muſchenbroeck thought, to that which he himſelf had obſerved, was ſeen by M. Griſhove at Berlin, though there was no parhelion viſible, but only two coloured arcs, gibbous toward the ſun, the nearer of them being 47°. 10′ from the ſun, which was 18°. 30′ high, and the more diſtant of them was 7°. from the firſt. In the exterior arc the colours, reckoning from the outer part toward the center, were in the following order, a deep violet, indigo, and ſo on to red, and in the interior arc the colours were in a contrary order, the red being outermoſt. At the beginning, and ſome hours before this appearance, no rain had fallen;

only the clouds looked black where theſe arcs were viſible, but afterward it rained copiouſly.

M. Æpinus, on the 23d of April 1758, obſerved a parhelion, in which the corona next the ſun was accompanied by a white elliptical area, the leſs axis of which was perpendicular to the horizon, and the ſame with that of the corona, but the larger was parallel to the horizon.* He ſaw the ſame appearance more diſtinctly on the 13th of May, and the figure of it is repreſented 145. This philoſopher thinks that ſome former appearances, in which exterior circles are mentioned, were of the ſame kind with this, but imperfectly ſeen, or improperly deſcribed.†

On the 15th of October, 1758, M. Æpinus ſaw a parhelion in which was a limb of a circle IHK, fig. 146, the center of which was in the zenith, coloured like the corona about the ſun CFED, and the vertex, H, was 45 degrees from the center of the ſun. He ſuſpects that parhelia with elliptical corona's are more frequent in the northern regions, and thoſe with circular ones in the ſouthern.§

Parhelia are generally ſituated in the interſections of circles, but Caſſini ſays, that thoſe which he ſaw in 1683, were on the outſide of the coloured circle, though their tails were in the circle that was parallel to the horizon. Parhelia have been viſible for 1, 2, 3, and 4 hours together; and in North America they are ſaid to continue ſome days, and to be viſible from ſun riſe to ſun ſet.‡

When the parhelia diſappear, it ſometimes rains, or there falls ſnow in the form of oblong *ſpiculæ*, as Maraldi, Weidler, Krafft, and others have obſerved; and becauſe the air in North America abounds with ſuch frozen ſpiculæ, which are even viſible to the eye, according to Ellis and Middleton, ſuch particles have been thought to be the cauſe of all corona's and parhelia.*

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* Nov. Com. Petrop. Vol. 8, p. 393. † Ib. p. 394. § Ib. p. 400, &c. ‡ Ib. p. 1041.

Mr. Wales ſays that, at Churchill, in Hudſon's bay, the riſing of the ſun is always preceded by two long ſtreams of red light, one on each ſide of him, and about 20° diſtant from him. Theſe riſe as the ſun riſes, and as they grow longer, begin to bend towards each other, till they meet directly over the ſun, juſt as he riſes, forming there a kind of parhelion, or mock ſun. Theſe two ſtreams of light, he ſays, ſeem to have their ſource in two other parhelia, which riſe with the true ſun; and in the winter ſeaſon, when the ſun never riſes above the haze or fog, which, he ſays, is conſtantly found near the horizon, all theſe accompany him the whole day, and ſet with him in the ſame manner as they riſe. Once, or twice, he ſaw a fourth parhelion directly under the true ſun; but this, he ſays, is not common.† Theſe facts being conſtant, are very valuable, and may throw great light on the theory of theſe remarkable phenomena.

Sometimes parhelia appear in a different manner, as when three ſuns have been ſeen in the ſame vertical circle, well defined and touching one another. The true ſun was in the middle, and the loweſt touched the horizon, and they ſet one after the other. This appearance was ſeen by M. Maleziew in 1722. Other appearances ſimilar to this are recited by M. Muſchenbroeck.§

Sometimes the ſun has riſen or ſet with a luminous tail projecting from him, of the ſame breadth with his diameter, and perpendicular to the horizon. Such an appearance was ſeen by

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* Nov. Com. Petrop. Vol. 8, p. 1042.

† Phil. Tranſ. Vol. 60, p. 129.

§ Introductio, p. 1046.

Caſſini in 1672 and 1692, by De la Hire, in 1702, and by Mr. Ellis in Hudſon's bay.

As M. Feuillée was walking on the banks of the river La Plata, he ſaw the ſun riſing over the river with a luminous tail projecting downwards, which continued till he was ſix degrees high.*

Paraſelenæ have alſo been ſeen, accompanied with tails and coloured circles, like thoſe which accompany the parhelia. An account of ſeveral, and a particular deſcription of a fine appearance of this kind may be ſeen in Muſchenbroeck.‡

The *Roman phenomenon*, obſerved by Scheiner, is ſo famous, on account of its having been the firſt appearance of the kind that engaged the attention of philoſophers, that my reader will be diſappointed if he do not find a more particular deſcription of it in this place. I ſhall, therefore, here introduce it.

This celebrated phenomenon is repreſented in fig. 147, in which A is the place of the obſerver, B his zenith, C the true ſun, AB a plane paſſing through the obſerver's eye, the true ſun, and the zenith. About the ſun C, there appeared two concentric rings, not compleat, but diverſified with colours. The leſſer of them, DEF, was fuller, and more perfect; and though it was open from D to F, yet, thoſe ends were perpetually endeavouring to unite, and ſometimes they did ſo. The outer of theſe rings was much fainter, ſo as ſcarcely to be diſcernible. It had, however, a variety of colours, but was very inconſtant. The third circle, KLMN, was very large, and all over white, paſſing through the middle of the ſun, and every where parallel to the horizon. At firſt this circle was intire; but towards the end of the appearance it was weak and ragged, ſo as hardly to be perceived from M towards N.

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* Muſchenbroeck's Introductio, p. 1046. ‡ Ib. p. 1047.

In the interſection of this circle, and the outward iris GKI, there broke out two parhelia, or mock ſuns, N and K, not quite perfect; K being rather weak, but N ſhone brighter and ſtronger. The brightneſs of the middle of them was ſomething like that of the ſun, but towards the edges they were tinged with colours, like thoſe of the rainbow, and they were uneven and ragged. The parhelion N was a little wavering, and ſent out a ſpiked tail NP, of a colour ſomewhat fiery, the length of which was continually changing.

The parhelia at L and M, in the horizontal ring, were not ſo bright as the former, but were rounder, and white, like the circle in which they were placed. The parhelion N diſappeared before K; and while M grew fainter, K grew brighter, and vaniſhed the laſt of all.

It is to be obſerved, farther, that the order of the colours in the circles DEF, GKN was the ſame as in the common halo's, namely red next the ſun, and the diameter of the inner circle was alſo about 45 degrees; which is the uſual ſize of a halo.

The hypotheſis of Deſcartes, to account for that horizontal circle, in which mock ſuns appear, has leſs probability than any of his conjectures. He ſuppoſes that a great quantity of frozen vapours, crouded together by contrary winds, form a prodigious cylinder, which reflects the light that fall from it on all ſides, ſo as to exhibit that lucid appearance in all the ſurrounding clouds. This thought does not deſerve any kind of animadverſion, nor is there any thing in his attempt to explain any other part of the Roman phenomenon, which he particularly deſcribes, worth repeating.*

Dechales expreſſes himſelf diſſatisfied with what had been written upon this ſubject of parhelia, or mock ſuns. He thought it poſſible, however, that they might be the reflection of the image of the ſun from clouds properly figured and diſpoſed. For though, in general, clouds are not ſmooth enough, nor of a proper form to reflect the perfect images of any object, yet he thought it poſſible that they might ſome times be capable of doing it. For his maſter in philoſophy informed him, that all the inhabitants of Veſul in Burgundy, were once exceedingly terrified by the appearance of an armed ſoldier in the air, which, upon examination, he concluded to have been the image of St. Michael, which was erected at the top of the church, reflected from the clouds. But if this had been the caſe, would not the church itſelf, or at leaſt ſome part of it, have been viſible alſo? If my reader chuſe to look into Dechales, he will find this ſtory related with great gravity.

M. Huygens took no leſs pains with the ſubject of *parhelia*, and the phenomena connected with them, than with that of the *halo*, and my reader will expect ſome account of his hypotheſis, eſpecially as ſome of the particulars of it are more probable, and are at this day more generally approved than his account of halo's.

This philoſopher, on giving his attention to theſe appearances, was preſently ſenſible that they could not ariſe from ſuch globules as formed the halo's, yet ſince parhelia are always attended with halo's, he was ſatisfied that their cauſes muſt be much alike. Conſidering, then, what other figures hail ſtones might poſſibly have, beſides a ſpherical one, he could find no other ſo ſimple as that of a cylinder; and, indeed, he had often obſerved, that ſnow did conſiſt of ſeveral ſlender oblong particles, mixed with thoſe of other ſhapes; and ſeeing that ſmall globules were ſufficient for the production of halo's, he imagined that a great number of ſmall cylinders, floating in the air, might produce ſimilar appearances. He alſo remembered that Deſcartes had taken notice of certain ſmall columns, or cylinders, which he had ſeen lying on the ground, the extremities of which were bounded with flat ſtars, conſiſting of ſix rays or points.

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* Dioptrics, p. 234.

The large and white horizontal circle, in the Roman phenomenon, M. Huygens ſuppoſed to be produced by the reflexion of the ſun's rays from the outſides of the upright cylinders; ſince, when the ſun ſhines upon a great number of ſuch cylinders, ſuſpended in the air, a white circle muſt neceſſarily appear to paſs through the ſun, parallel to the horizon, and of the ſame breadth with that of the ſun. This he ſhews very diſtinctly by a large figure of a cylinder, and by pointing out the progreſs of the ſun's rays reflected from it. For every point of the ſun's vertical diameter, as well as his center, will illuminate a circle of cylinders, of the ſame apparent height as the illuminating point. It is obſervable that no thick clouds are ſeen in the air, when theſe circles appear, but only ſuch as are very thin, and ſcarce viſible. For in moſt of theſe obſervations the ſky is ſaid to have been very clear and ſerene, which very well agrees with this hypotheſis; ſince theſe minute cylinders muſt conſtitute a very thin cloud, uniformly extended; through which the ſun, and even the blue colour of the ſky may be ſeen.

M. Huygens ſuppoſes that the parhelia at K and N are produced by the ſame upright cylinders, which produce the great white circle; but not in the ſame manner as that was cauſed, viz. by ſimple reflexion, but by means of two refractions, exactly in the ſame manner as halo's are produced; the center of the cylinder being ſnow, and the outerpart ice. The interval between theſe parhelia, he ſays, will be greater, as the cylinder of ſnow is thicker in proportion to the whole cylinder; and becauſe the ſun appears brighteſt through thoſe cylinders which are on the outſides of the arch KN; and this brightneſs decays gradually to a certain diſtance, it exhibits the appearance of a parhelion with a tail, always tending towards the white circle, and making it brighter as far as it extends. The tail NP, of the parhelion at N, is drawn out of this circle; but this was a miſtake, as Hevelius obſerves; and though it is not ſaid that the parhelion at K had any tail, yet it is plain that a certain part of the white circle was its tail, though but a dull one; becauſe this parhelion is ſaid to have been fainter than the other; and it appears by the obſervations of Hevelius, and his own, that theſe parhelia, or mock ſuns, as well as *paraſelenæ*, or mock moons, always have tails. Laſtly the extraordinary brightneſs of theſe parhelia, which was ſaid to rival that of the ſun, he ſuppoſed might eaſily be accounted for, by conſidering that every cylinder ſhines through its whole length; whereas the round grains in the halo, or the rainbow, emit but little light; ſo that the cylinder may probably afford more light than ten of thoſe round grains; and if there be great plenty of cylinders in the air, it is no wonder that thoſe images of the ſun ſhould appear ſo bright.

He then proceeds to demonſtrate his hypotheſis, by an accurate conſideration of the refraction of the rays of light through the ſides of ſuch cylinders as have been deſcribed; and he inveſtigates the diſtance at which the parhelia muſt appear from the ſun; ſhewing that it is ſo much the greater, as the internal cylinder of ſnow is thicker in proportion to the whole cylinder; and that this diſtance muſt become greater as the ſun riſes higher: and he ſubjoins a table of theſe diſtances, in proportion to any given altitude of the ſun; from which it appears, that if a ſufficient number of cylinders be placed one above another, (ſome of which are melted more and ſome leſs) beſides the collateral parhelia next the ſun, two or more may appear farther from him, but ſtill in the ſame white circle; which was alſo confirmed by the obſervations of Hevelius in 1661, and of Scheiner in 1630. It appears, alſo, from the ſame tables, that while the cylinders remain in the ſame ſituation, as the ſun riſes higher, the intervals between him and the two parhelia will increaſe, which was the very thing which he himſelf had obſerved. But a greater alteration of theſe intervals may happen if the cylinders be more melted; and from hence might ariſe that very remarkable phenomenon, which is ſaid to have appeared in the time of Auguſtus, when three ſuns were ſeen at the ſame time, all of which were ſoon contracted into one; which, he obſerves, would be effected by the warmth of the air melting the ſnow in the cylinders; by which means both the parhelia would draw nearer to the ſun, till they coincided with it; at which time all the cylinders would be melted, and changed into round drops.

In the next place our author proceeds to explain the cauſe of that corona in the Roman phenomenon, of which the ſun is the center. And here his former hypotheſis concerning halo's, he acknowledges, will not anſwer the purpoſe. For though, when ſmall cylinders are produced in the air, it is probable that half thawed globules are lodged there at the ſame time, for the production of corona's; yet it is difficult to conceive how they can be thawed in ſo exact a proportion, as to produce a corona that ſhall paſs preciſely through thoſe very parhelia. Neither, as he obſerves, can this appearance be produced by cylinders obliquely ſituated; but only by the round ends of upright cylinders. And to ſuppoſe the above mentioned cylinders to be always terminated by plain baſes, he ſays, is by no means agreeable to nature; for ſince their tops and bottoms will be thawed, as well as their ſides, and to the ſame degree quite round, the water at their extremities will neceſſarily aſſume a round figure; ſo that not only thoſe upright cylinders which lie on each ſide of the ſun, and produce the collateral parhelia, will tranſmit his rays to the eye; but alſo thoſe that lie quite round beyond him, to a certain diſtance, which will, therefore, produce a corona.

To verify this hypotheſis by experiment, he ordered a glaſs cylinder to be made with round ends; and, incloſing in it an opaque body of the ſame ſhape, he found that it did, in reality, refract the light in ſuch a manner as would produce this appearance. Conſidering, therefore, the neceſſity there is for the cylinders being of this particular ſhape, in order that the rays, paſſing through their extremities, may produce coloured circles, he did not doubt, but that theſe circles are thoſe very corona's. Otherwiſe ſome irregular and ill ſhaped circles would be ſeen.

Hevelius had remarked, that the circle which paſſes through the parhelia, frequently appears fainter above and below, and brighteſt next the parhelia. The reaſon of this M. Huygens thought to be very plain from his glaſs cylinder, ſince the rays which paſſed through the round tops and bottoms of them produced only a ſmall round image of the ſun, as round grains do; whereas thoſe which paſſed towards the ends of the cylindrical ſurfaces, where they juſt began to turn round, were formed into a broader and brighter pencil, like thoſe which produce the parhelia; and this muſt make the corona grow gradually brighter and brighter towards the parhelia, both above and below, and to be more intenſely coloured.

Having explained the cauſes of this corona, he proceeds to account for the two parhelia L and M, which appeared in the back part of the great white circle. And theſe, he thought, were produced, not by the reflexion, but by the refraction of the ſun's rays in the upright cylinders. The inner opaque cylinders, he obſerves, are ſo far from contributing to the production of theſe parhelia, that they ſometimes hinder their appearances. But yet this inner cylinder of ſnow is abſolutely neceſſary, whether the outward ſhell that contains it be made of water or of ice; for without the ſnow, the watery cylinders would be immediately changed into round drops, and the whole cylinders could not be ice, becauſe then they could not be every where tranſparent, without ſuppoſing them to have been quite liquid before they were frozen, which is inconſiſtent with a cylindrical figure.

He then ſhews, that theſe back parhelia muſt be in the large white circle; and determines, geometrically, that the diſtance between them muſt be 60 degrees. Indeed, in Scheiner's ſcheme, the diſtance of theſe parhelia exceeds 90 degrees; but it was neither meaſured, nor eſtimated; nor indeed, ſo much as mentioned, and therefore M. Huygens does not doubt but that it is drawn much too large. In the Roman phenomenon theſe parhelia are ſaid to have been white, whereas, according to this hypotheſis, they ought to have been coloured. M. Huygens ſuppoſes that the whiteneſs aroſe from the weakneſs of their light; on which account halo's ſometimes appear white, and ſometimes alſo the lateral parhelia, as appears by the accurate obſervations of Kechelius. But that theſe back parhelia do ſometimes appear coloured, is evident from the account we have of one in Mat. Paris's hiſtory; in which we are told that beſides the true ſun, four mock ſuns appeared red, in a great circle, of the colour of cryſtal. That two of theſe were back parhelia is evident from the figure, though it may be faulty in other reſpects.

The ſcarcity of cylinders, M. Huygens ſuppoſes, may be the reaſon that, in many obſervations, theſe parhelia have not appeared, though the white circle was plain enough to be ſeen. But there is another circumſtance which may hinder their appearance, viz. the inner cylinders of the back part of the white circle being too thick in proportion to the water that ſurrounds them. For then, the rays which ſhould produce the parhelia will be ſtopped; and he found by calculation, that, when the ſun is 25 degrees high, if the diameter of the opaque cylinder bear a greater proportion to that of the whole, than 590 to 1000, no back parhelia can appear.

That the back parhelia in the Roman phenomenon appeared rounder then the lateral ones, M. Huygens ſuppoſes to ariſe from this circumſtance, that though ſome cylinders in the arch LM, which lie not far from L and M, do ſend ſome refracted rays to the eye, yet they are not ſo numerous as thoſe which come from the cylinders at L and M; and therefore that theſe parhelia can have no tails that are far extended, like the lateral ones. Beſides the refractions through the cylinders that produced the back parhelia are more regular; for the rays are not confined in their courſe by the opaque cylinders within, but receive their directions from the refractions at the moſt exact figure and poliſh of the outward watery ſurface of the cylinders. For the dark cylinders within, not being exactly equal to one another, are the cauſe of the irregular, and inconſtant ſhape of the lateral parhelia. The reaſon of the fluctuating motion of the tail of the parhelion N, he ſuppoſes to be, that, ſometimes fewer, and ſometimes more cylinders were carried into that place; which, alſo, accounts for the corona DEF being ſometimes intire, and ſometimes open below, and that the parhelion K grew brighter while N grew duller.*

M. Huygens, having thus explained this famous Roman phenomenon, proceeds to conſider the circumſtances of another, that was ſeen by Hevelius on the 20th of February 1661; in which more ſuns and more circles were obſerved, and ſome of them in different poſitions from any that were obſerved in the Roman phenomenon. But, ſtill, they are ſo ſimilar to them, and his ſolution of them is ſo ſimilar, that it is not neceſſary to my purpoſe to follow my author much farther. I ſhall, however, give the figure of that of Hevelius, fig. 140, which is exceedingly beautiful, and ſeems to compriſe all the real varieties of this moſt magnificent ſpectacle, parts of which have often been ſeen ſeparately. And I ſhall then juſt hint at M. Huygens's explanation.

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* Smith's Opticks, Vol. 1, p. 220.

The greateſt difference between this appearance and that at Rome, conſiſts in the arches QGR and THS, which are convex towards the ſun, and which he accounts for by cylinders lying in a horizontal ſituation. The parhelia in theſe inverted arches, he ſays, are nothing more than the brighteſt parts of them; and therefore they never appear quite diſtinct nor much brighter than the adjoining parts of the arches. Accordingly, Hevelius, in all his obſervations of this kind, took notice, that they appeared rather dull, heavy, and faint; and he was not ſure whether there was a parhelion at H, or not. The reaſon why the middle part of the arch ſhould appear a little diſtinct from the reſt, he ſuppoſed to be this, that though there be great plenty of horizontal cylinders in the air, yet they may be ſo ſhort, as to have but little of a cylindrical ſurface, or perhaps may be like oblong ſpheroids. Beſides it is perfectly agreeable to the obſervations of Hevelius, that the higher the ſun or moon, and conſequently the corona's are elevated above the horizon, the flatter theſe inverted arches appear. It is confeſſed by M. Huygens, that the upper parts of theſe arches are repreſented by Hevelius as being much leſs than his calculations require them to be; but he imagines, that the ſight of the obſerver was impoſed upon, by the arches being ſo much elevated above the horizon; in conſequence of which they would appear to be parts of much leſſer circles than they really are.

Profeſſor Weidler, who took ſome pains to refute M. Huygens's account of halo's, approves of his hypotheſis to account for the horizontal ring in the phenomenon of the mock ſun by means of cylindrical frozen vapours.* Having once ſeen an inverted rainbow, which was part of the intire phenomenon of a parhelion he obſerved that it was but one degree broad, and that the diameter of the bow was but about half of that of the primary rainbow. He was of opinion that the ſun's rays, refracted and reflected as they are in the common bow, entered the eye at half the uſual angle, in conſequence of the eye being placed beyond the point in which the rays met with the axis of the bow. For thus, he ſays, the order of the colours is preſerved, and the bow is but half the ſize of the common one.§

M. Muſchenbroeck ſays, that nothing has been advanced concerning the cauſe of parhelia more probable than the hypotheſis of M. Huygens, except that frozen cylinders or ſpiculæ are always found tranſparent, and are never opaque at the center.‡

The coloured arcs that accompany parhelia, M. Muſchenbroeck thinks to be not at all different from the common corona's, ſo that if there be a quantity of icy *ſpiculæ* in a thin fog, the whole of this appearance will be produced, the corona's will be formed in the fog, and the parhelia in the ſpiculæ.

M. Fr. Mallet, deſcribing a parhelion which was ſeen in Sweden, obſerves that ſome of the particulars did not favour the hypotheſis of M. Huygens. 1. The diſtance between the ſun and the parhelion correſponding to L fig. 147, was diminiſhed as the ſun advanced in height, but did not grow larger as the ſun deſcended in the afternoon; but ſtill continued to decreaſe. 2. As the two circles which had the zenith for their common center, exactly touched thoſe which ſurrounded the ſun, he thought they muſt depend upon the ſame cauſe; but ſince the outermoſt of the two circles, which had the ſun for their center, was twice as broad as the other, he concluded that they had different cauſes.*

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* Phil. Tranſ. ab. Vol. 8, p. 513. § Ib. p. 514. ‡ Introductio, p. 1042.

Such are the *facts* and *opinions* that I have been able to collect concerning this great natural phenomenon. From all the circumſtances put together, and making due allowance for miſtakes in the obſervations, it ſhould ſeem that, when this appearance is complete, there are ſix intire circles viſible, three of which have the ſun, and three the zenith for their center; and that parhelia appear wherever any of theſe circles either interſect, or touch one another. Notwithſtanding the laboured hypotheſis of M. Huygens, it can hardly be ſuppoſed but that theſe circles, whether coloured or white, are formed in mere fog or vapour, which probably conſiſts of water in very ſmall and round globules; but theſe appearances have not yet been deduced from the properties of light, in theſe circumſtances, by any philoſopher who has yet written on the ſubject.

SECTION XII.

Observations and discoveries relating to VISION.

THE obſervations and diſcoveries which have been made to explain the phenomena of viſion within this period are ſo numerous, that I find it convenient to diſtribute the materials that I have collected relating to it, under more heads than I have hitherto found it neceſſary to do. The reader may not, perhaps, think that I have diſtributed all the articles with perfect accuracy; but the ſubjects are ſimilar, and conſiderably mixed, eſpecially thoſe of the ſame author; ſo that it was not eaſy to arrange them as I myſelf could have wiſhed. In any poſſible arrangement ſome natural connections muſt be broken.

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* Swed. Abhand. Vol. 25, p. 47.

CHAPTER I.

Observations on the effect of light upon the eye.

THE firſt article with which I ſhall preſent my reader on this ſubject, conſiſts of ſome ingenious obſervations of M. Buffon, who diſtinguiſhes the colours which belong to light, from thoſe which depend upon the *affections of the eye*, which he calls *accidental colours*, and whoſe object is to trace the connexion they have with thoſe that are natural.

Looking a conſiderable time on a red ſquare mark upon a white ground, a faint green was perceived to ſurround it, and when the eye was turned from it, the image of the ſquare itſelf became green. In the ſame manner yellow upon white produced a pale blue; green upon white produced a faint purple; blue upon white a pale red; black upon white a brighter white than the ground upon which it was placed; and white upon black produced a deeper black. Theſe experiments were made upon dead colours, but they ſucceeded better with thoſe which were ſhining.

Keeping his eye fixed a long time upon a red ſquare placed upon a white ground, he firſt perceived the border of faint green mentioned above; and continuing to look upon it, the middle of the ſquare began to change its colour, while the ſides became of a deeper red. Then retreating a little, and ſtill keeping his eye ſteadily upon it, each of the ſides of the deep red ſquare divided itſelf into two parts, forming a croſs of deep red. Continuing to look fixedly upon it, the whole appearance changed into a rectangle of the ſame heighth with the ſquare, but not a ſixth part ſo broad, and of ſo deep a red, that it quite dazzled his eyes, and he could ſupport the fatigue of the experiment no longer. Then turning his eye from the object upon a white coloured ground, inſtead of the red ſquare, he perceived the image of the rectangle, exactly defined, and of a lively green. This impreſſion remained a long time, without any change of colour, even after his eyes were ſhut. He obſerved ſimilar appearances when he looked, in the ſame manner, upon a ſquare that was yellow, black, or any other colour; the laſt impreſſion of a yellow ſquare being a blue rectangle, and of a white ſquare, a black rectangle. He engaged ſeveral of his friends to make theſe experiments, who all ſaw the ſame colours, and the ſame appearances.

He found that theſe accidental colours changed as they mixed with thoſe that were natural, and that they obſerved the rules above mentioned. For when an accidental green colour, produced by a natural red, fell upon a bright red ground, the green was changed into yellow; and if an accidental blue colour, produced by a lively yellow, was thrown upon a yellow ground, it became green, ſo that the colours which ariſe from the mixture of thoſe that are accidental with thoſe that are natural, are the ſame that ariſe from the mixture of natural colours only.[*]

M. Æpinus, whoſe diſcoveries in electricity have ſo much diſtinguiſhed him, made a number of ſeemingly accurate obſervations on the ſubject of accidental colours, from all which he draws the following concluſions. The vivid impreſſion which is made upon the eye by looking on the ſun, or any other luminous object, as bright as it can poſſibly bear, firſt aſſumes a yellow appearance, then green, and then blue; that is the nerves, upon the ceſſation of the action of the white rays, come to the ſame ſtate into which the action of yellow, green, and blue rays would bring them. Looking upon a white object, when the impreſſion made by the image of the ſun ſtill continued in his eye, he obſerved that it aſſumed a brown appearance, verging more or leſs towards red. He alſo obſerved that this image of the ſun extended to ſome diſtance on the retina, for that a ſpace beyond the boundary of the image was affected in the ſame manner as it would have been by the action of red rays; but when the image was thrown upon a white object, that border became blue.

In the courſe of theſe obſervations he took notice, that when he looked intently upon any white object, while the impreſſion of the ſolar image remained upon his eye, it would ſuddenly diſappear, and become viſible again, when he gave leſs attention to it, a phenomenon depending upon the affection of the nerves, and which he acknowledges he could not ſatisfactorily account for.[†]

Theſe obſervations are nearly agreeable to the reſult of thoſe of M. De La Hire, who ſays that, if the eye be kept ſhut after looking at the ſun, or any other luminous object, the image upon the retina will be firſt red, then yellow, then green, and laſt of all blue. This fact is curious, ſhewing the gradual weakening of the impreſſion of the light after the object is removed, and the order in which the different colours affect the eye with reſpect to their intenſity; but by no means proves what he is endeavouring to prove by it, viz. that the form of the eye is never changed in viſion.*

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[*] Ac. Par. 1743, M. p. 215.

[†] Nov. Com. Petrop. Vol. 10, p. 286.

That impreſſions made upon the eye are of a certain *continuance*, is evident from a variety of obſervations, ſome of which cannot have eſcaped the attention of any perſon; and ſince the perfection of aſtronomical obſervations depends much upon the accuracy of viſion, it occurred to M. D'Arcy, that the meaſuring of the continuance of theſe impreſſions upon the eye, in different circumſtances, was a proper object for philoſophical inquiry; and he, therefore, ſet about doing it in the moſt careful manner.

For this purpoſe he contrived a machine, which conſiſted of a croſs, turning horizontally upon its center, by means of a wheel and a weight, the velocity of which he could vary at pleaſure, and aſcertain to the utmoſt exactneſs. To view this machine, he placed an obſerver 28 toiſes from it, in a room into which no light was admitted but what came from the object (which was a live coal faſtened upon the croſs) and the experiments were made in the night time.

In theſe circumſtances, uſing every precaution that he could think of, he found that the coal ſeemed to make an uninterrupted circle, when it revolved in 8 thirds of a minute; and that no velocity leſs than this would anſwer the purpoſe. It made no difference at whatever diſtance from the center of the croſs the coal was placed, or whether the machine was viewed through a teleſcope, or in any other method that he could think of applying. The reſult was alſo the ſame, at whatever diſtance the machine was viewed. Our author, therefore, concluded, that the impreſſion made by the action of light upon the eye continues 8 thirds of a minute. He varied this experiment, by putting a covering upon the croſs, with an opening through which the live coal might be ſeen; and in this caſe he found that the appearance of the coal continued uninterrupted, when the revolution was compleated in 9 thirds of a minute.

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* Porterfield on the Eye, Vol. 1, p. 343.

M. D'Arcy acknowledges that his experiments were very imperfect, and he propoſed to try farther, whether 1. the different intenſities of the light would not occaſion a ſenſible variety in the duration of the impreſſion; 2. Whether different diſtances of the obſerver would not have ſome effect. For though when the whole circle was attended to, it appeared uninterrupted with the ſame velocity, to the obſerver at the diſtance mentioned above, and to himſelf when he was no more than three feet from it; yet when he attended to a part of the circle only, the velocity was not ſo great as was requiſite to give the ſame appearance to the obſerver; 3. Whether rays of different colours would not make impreſſions of different durations, but the bad weather prevented him from proſecuting theſe experiments. He was ſatisfied, however, from the few trials that he did make, that white bodies exhibit an annular appearance with the ſame velocity that was given to the live coal for that purpoſe. He alſo propoſed to have made the ſame experiments upon different perſons, but he obſerves that it will not be very eaſy to do it, and concludes with expreſſing his wiſhes, that ſome other perſons may conſider and reſume his obſervations.*

That the pupil contracts when the eye is expoſed to a ſtrong light was obſerved, we have ſeen, pretty early. This is, no doubt, the effect of light upon the eye. Dr. Hartley ſuppoſes that the light which enters at the pupil has great efficacy in contracting both the greater and leſs rings of the iris, as may be concluded, he ſays, from the immobility of the pupil in a gutta ſerena; alſo becauſe, on this ſuppoſition, the light which paſſes in at the pupil muſt, by contracting the leſs ring, become a check and guard againſt its own too free admiſſion, which is agreeable to the tenor of nature in like inſtances. The retina, he obſerves, extends to the greater ring, and may ſend ſome nervous fibres to it, and even to the iris.

One or both of theſe actions of light, he ſays, ſeem to increaſe the ſecretion and circulation of the aqueous humour in new born children, ſo as to fit the eye for viſion, which it is not fit for (through the deficiency and muddineſs of the aqueous humour) till ſome time after birth.§

D. Whytt alſo ſays that the contraction of the pupil is not performed by the action of light upon the iris, but upon the retina; ſince whatever intercepts the rays of light, ſo as to prevent their reaching the retina, cauſes a preternatural dilatation of the pupil; for that in a cataract, where the cryſtalline humour, being conſiderably opaque, intercepts a great part of the rays, the pupil loſes a good deal of its contractile power.‡ Suppoſing the ſtrongeſt impreſſion of light to be on the choroides, theſe obſervations equally ſuit the hypotheſis of the ſeat of viſion being at that membrance. This author alſo obſerves that the wideneſs of the pupil in a ſyncope, apoplexy, and confirmed gutta ſerena, ſhews that, in order to dilate the pupil to its largeſt ſize, no effort of the mind is neceſſary, but only the ſuperior contractile power of the longitudinal fibres of the uvea, when its circular muſcle is not excited into action by the ſtimulus of light on the retina.*

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* Ac. Par. 1765, M. p. 450. § Hartley's Obſervations on Man, Vol. 1, p. 219.

‡ Eſſay on vital and involuntary Motion, p. 112.

As the fibres and rings of the iris cannot be proved to be muſcular, and it is the opinion of ſome of the beſt anatomiſts, that they are only vaſcular, M. J. Weitbricht endeavours to account for the dilatation and contraction of the pupil, for the admiſſion of more or leſs light, by ſuppoſing that it depends upon the motion of the membrane that forms the iris within the aqueous humour, the aperture being narrow when it hangs near the cryſtalline, and wide when it is protruded towards the cornea, which only ſuppoſes a mechanical extenſion of the ring, and no proper muſcular power of its own. This motion of the iris, he ſays, may be affected by the flowing of the aqueous humour from one of the chambers that contains it to the other, which, he ſuppoſes, may be produced by the action of light upon the vitreous humour, or the veſſels which contain it, ſo as to preſs upon the cryſtalline, and make it to puſh the aqueous humour into the fore part of the eye. But he owns that he cannot ſatisfy himſelf as to the phyſical cauſe of this operation.§ Indeed this hypotheſis is ſuch as I ſhould not have mentioned except in the character of an hiſtorian.

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* Eſſay on vital and involuntary Motion, p. 112.

§ Com. Petrop. Vol. 13, p. 349.

CHAPTER

CHAPTER II.

OBSERVATIONS CONCERNING THE MANNER IN WHICH THE EYE CONFORMS ITSELF TO THE DISTINCT VIEW OF OBJECTS PLACED AT DIFFERENT DISTANCES.

THAT we are capable of viewing objects with nearly equal diſtinctneſs, though they are placed at conſiderably different diſtances, is evident; but the alteration which takes place in the eye for this purpoſe, or the mechaniſm by which this effect is produced, is not eaſily aſcertained.

It was the opinion of Kepler, as we have ſeen, that the contraction of the *proceſſus ciliares* changes the form of the eye, and by the elongation of it, places the cryſtalline at a greater diſtance from the retina; whereas Deſcartes imagined that the curvature of the cryſtalline itſelf ſuffers an alteration by the contraction of thoſe ligaments. This curious ſubject began to be more particularly, and more accurately diſcuſſed in the period preceding the diſcoveries of Newton; but ſince it was not thoroughly explained before the preſent period, I have reſerved my account of the whole progreſs of this controverſy to this place.

M. De la Hire, who conſidered this ſubject, as well as almoſt every other ſubject relating to viſion with the cloſeſt attention, was not, however, happy in his hypotheſis. He maintained that, in order to view objects at different diſtances, there is no alteration but in the ſize of the pupil, or the aperture of the eye; and he made a curious experiment, which he thought proved his aſſertion. He placed a ſmall black ſpot, both as near, and as far off, as it could be diſtinctly ſeen; and having two holes cut in a piece of paſteboard, of ſuch a ſize, and at ſuch a diſtance, as juſt to admit pencils of rays paſſing from this black ſpot to each eye, he interpoſed it between the eye and this object, after the perſon on whom the experiment was made had been looking upon it ſteadily for ſometime; upon which it immediately appeared double. From this he concluded, that the alteration which occaſioned the double viſion had not been made in the form of the eye, which had previouſly conformed itſelf to the diſtinct view of the object; whereas the ſize of the pupil might have been varied by the excluſion of the light from foreign objects. But to this it is juſtly objected, that the eye having been upon the ſtretch, in order to view this object at the neareſt or greateſt limit of diſtinct viſion, may well be ſuppoſed to have relaxed of its tenſion, while the paſteboard was adjuſting; and beſides, the apprehenſion, as I may ſay, of diſtance beyond the paſteboard is excluded by the interpoſition of it, and the idea, or bare remembrance of the diſtance, is not ſufficient to affect the eye. Indeed there is no doubt among philoſophers at preſent, but that we have a power of altering the form of our eyes, ſo as to make the rays of any pencil to converge at different diſtances from the pupil.*

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* Smith's Opticks. Remarks, p. 41.

In this laſt period of my hiſtory, M. Le Roi, a member of the Royal Academy at Montpelier, has attempted to defend the opinion of M. De la Hire, which had long been exploded by all philoſophers, viz. that the eye accommodates itſelf to the view of objects placed at different diſtances by the contraction or dilatation of the pupil only. He ſays that this accommodation does not conſiſt in the change of the place of the cryſtalline, by means of the ligamenta ciliaria, the ſtrength of which is very inadequate to ſuch a purpoſe. Beſides, he obſerves that they are not attached to the edges of the capſula, as has been ſuppoſed, but that they extend a conſiderable way along the anterior ſurface of it, without any cloſe adherence to it. He is alſo of opinion that theſe fibres are not muſcular, but are only ramified veſſels, which, according to all appearance, he ſays, anſwer no other purpoſe than that of ſecreting an aqueous humour, to lubricate the ſurface of the cryſtalline.*

That nothing was requiſite but the contraction of the pupil, in order to view the neareſt objects with diſtinctneſs, is evident, he ſays, from experiment. For when an object is placed ſo near, that the eye cannot bear as great a degree of contraction as is neceſſary for viewing it diſtinctly, the ſame end is obtained by an artificial pupil. For if a ſmall hole be made in a card, the neareſt object may be viewed through it, with the greateſt eaſe and diſtinctneſs.

That the variation of the pupil is ſufficient for the purpoſe of viewing objects of all diſtances, he alſo thought he could demonſtrate by experiment with an artificial eye. For when, with a large aperture, the images of near objects were confuſed, and ill defined upon the retina of this inſtrument, they became very diſtinct, and well defined, by contracting the aperture.

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* Ac. Par. 1755, M. p. 920.

This gentleman, however, does not ſeem to be aware of the reaſon why a narrow pupil leſſens the indiſtinctneſs of viſion, which is by confining the baſes of the pencils of rays which iſſue from the points of an object; ſo that whether they meet before they come to the retina, or converge towards ſome place beyond it, the circle of diſſipation, as it is called, or the ſpace which they cover upon the retina, is thereby contracted. The contraction of the pupil, therefore, tends to leſſen all indiſtinctneſs of viſion, whether the objects be too remote, or too near; and it may be made very evident by the help of a portable camera obſcura: whereas it is certain that the pupil is not contracted, but dilated, for the purpoſe of viewing objects that are very remote. Indeed, without a dilatation of the pupil in thoſe circumſtances, a ſufficient quantity of rays could not be admitted. When objects are near, and well illuminated, the contraction of the pupil may be ſufficient for viewing them diſtinctly, but there muſt be ſome other proviſion than this for remedying the indiſtinctneſs of objects that are very remote.

The moſt ſatisfactory diſcuſſion of this ſubject we owe to Dr. Porterfield, who proved, by a ſeries of experiments, in which an object was viewed through ſmall ſlits in a thin plate of iron, at a leſs diſtance than the diameter of the pupil (which therefore was of no uſe in this caſe) that we are poſſeſſed of a power of changing the conformation of our eyes, and of adapting them to various diſtances; and that this change always follows a ſimilar motion in the axes of viſion, with which it has been connected by uſe and cuſtom. One of theſe experiments, which will be ſufficient for the purpoſe, I ſhall recite, after a ſhort explanation of two *axioms*, which he makes uſe of, in order to draw his concluſions from them.

The firſt is, that, when an object ſeen with both eyes appears double, becauſe its diſtance is leſs than that to which the eyes are directed, upon covering either of the eyes, the appearance on the contrary ſide will vaniſh; and if it appear double becauſe its diſtance is greater than that to which the eyes are directed, upon covering either of the eyes, the appearance that is on the ſame ſide will vaniſh. The ſecond axiom is, that when an object appears double from its being ſeen with one eye, through two ſmall holes made in a card, or any other opaque thin body, if its diſtance be greater than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the ſame ſide will be made to vaniſh; and if its diſtance be leſs than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the contrary ſide will be made to vaniſh.

The reaſon of the former of theſe axioms will be evident to one who conſiders that the image of any object is in a right line from the eye by which it is perceived, and that thoſe images croſs one another when the eye is directed to a place beyond the object which has the double image. But, indeed, any perſon may ſoon ſatisfy himſelf with reſpect to this axiom by actual trial. The ſecond axiom will be fully explained in the account of the experiments of Dr. Motte at Dantzic, where it will be ſhewn, that when the ſame object is ſeen through two ſlits, the rays coming from it through a ſmall hole, croſs one another before they reach the retina, if the object be very remote, but after it, if it be too near for diſtinct viſion.

To try the power of the eye to conform itſelf to the diſtinct view of objects placed at different diſtances, independently of the contraction of the pupil, he took a ſmall plate of tin IK, fig. 149, in which he cut two parallel narrow ſlits, whoſe diſtance from one another did not exceed the diameter of the pupil. Theſe ſlits gave paſſage to more light than what could paſs through the holes, and therefore were fitter for his purpoſe, it being neceſſary that the object ſhould be clearly ſeen. This plate he held cloſe to his right eye B, in ſuch manner, as that the ſlits might have a vertical poſition; and having ſhut his left eye A, he viewed through theſe ſlits the ſmall object O, which alſo had a vertical poſition, and conſequently was parallel to the ſlits. In this experiment the object O was at ſuch a diſtance from the eye B, as to appear ſingle, when viewed in this manner, through the ſlits; but when both eyes were opened, and directed to a more diſtant point, ſuch as P, three appearances were ſeen *a*, *b*, and C, which appearances were nearer to or farther from each other, according as the point P was nearer to, or farther from the object O; and in covering the left eye A, the appearance *a* which was on the contrary ſide vaniſhed, which appearance did therefore belong to the eye A. And in covering the right eye B, the appearances on the contrary ſide, *b* and C, belonging to the eye B, vaniſhed; by which he was certain, from his firſt axiom, that the diſtance of the object O was leſs than that to which the eyes were directed. This being done, his next buſineſs was to examine whether theſe double appearances, *b*, and C, which were ſeen through the ſlits, did not alſo proceed from the object O being at a leſs diſtance than that to which the eye B was then accommodated; and upon trial he found it was ſo, for by covering either of the ſlits with his finger, the appearance on the contrary ſide was always made to vaniſh; agreeably to his ſecond axiom.

Having ſatisfied himſelf as to theſe particulars, he changed the direction of his eyes, and turned both of them inwards, towards a nearer point, ſuch as *x*, by which alſo three appearances were ſeen, *d*, *e*, and F; and theſe appearances were alſo nearer to, or farther from one another, according as the point *x* was nearer to, or farther from the object O; but they were always in a contrary order to thoſe that were ſeen when his eyes were directed as above. For the appearance F, ſeen by the left eye A, was on the left ſide, and the appearances *d* and *e*, which were ſeen through the ſlits, by the right eye B, were on the right ſide; whence he was certain that the diſtance of the object O was greater than that to which his eyes were directed. He then covered one of theſe ſlits with his finger, and found that the appearance which was on the ſame ſide did always vaniſh; from which, when compared with the ſecond axiom, it follows that the object O was then at a greater diſtance than that to which the eye was accommodated.

In making this, and all the following experiments, it was neceſſary that the object O ſhould be as conſpicuous as poſſible. What, upon trial, he found to anſwer beſt, was a narrow ſlit, made in a dark lantern, in which a lighted candle was put, to render it luminous.*

From theſe experiments he thought that he had ſufficiently proved what he propoſed; for when the eyes were directed to P, the object O was ſeen double through the ſlits; and by covering one of the ſlits, it appeared that the eye was adapted to too great a diſtance; and as the point P was brought nearer and nearer to the object O, theſe appearances approached nearer and nearer to one another continually, till at laſt, when the point P became very near to O, they coincided in one, at O; which ſhews that the eye was then adapted to its diſtance. But when the point P was moved along the line O *x*, from O to *x*, two appearances of the object O were again ſeen through the ſlits; which appearances, being in a contrary order from what were ſeen when the point P was on the other ſide of the object O, it follows that the eye was then adapted to too ſmall a diſtance. And as the point P, in its motion from O to *x*, receded farther and farther from O, theſe appearances receded farther and farther from one another continually. From all which it is evident, that there is a neceſſary connexion and dependence eſtabliſhed between thoſe motions, whereby the conformation of our eyes is changed, and that there are certain correſponding motions in the axes of viſion, which makes it impoſſible for us to direct our eyes to any object, within the limits of diſtinct viſion, without, at the ſame time, giving them that diſpoſition which is neceſſary for ſeeing diſtinctly at that diſtance.*

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* Porterfield on the Eye, Vol. 1 p. 411.

From other experiments, ſimilar to that above mentioned, it was manifeſt that the eye frequently miſtakes the diſtance of objects. For the ſame reaſon, it is that, when both eyes are open, and directed to the object, it appears ſingle at all diſtances, within the limits of diſtinct viſion, becauſe the eye is then accommodated to its diſtance, which is known to us by means of the angle which the optic axes make at the object.

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* Porterfield on the Eye, Vol. 1, p. 415.

Secondly, the judgment which the mind forms, with reſpect to the diſtance of objects, ſeen with only one eye through the ſlits, is not always the ſame, but is fluctuating and inconſtant; as he gathered from ſome experiments in which the object ſometimes appeared ſingle, and at other times double; and when it appeared double the diſtance betwixt the appearances was not conſtantly the ſame.

Thirdly, if the object ſeen through the ſlits, the other eye being ſhut, is not much beyond the neareſt limits of diſtinct viſion, when the mind miſtakes its diſtance, it imagines it farther off than it really is, as was evident from ſeveral of his experiments. But

Fourthly, when the object is not a great deal nearer than the greateſt diſtance of which it can be diſtinctly ſeen, when we miſtake its diſtance, we imagine it to be nearer than it really is; whence it appears double, becauſe it is too far off with reſpect to the conformation of the eye.*

From the principal of theſe experiments Dr. Porterfield obſerves, that an inſtrument may be contrived, which, from its uſe, in meaſuring the limits of diſtinct viſion, and in determining with great exactneſs the ſtrength or weakneſs of ſight, may be termed an *optometer*.§

That the eye does, by ſome conformation, adapt itſelf to the view of objects at different diſtances, ſeems to have been indiſputably proved by Dr. Porterfield; but among thoſe who ſuppoſe a conformation of eye for this purpoſe, independent of a variation in the aperture, it is by no means agreed in what it conſiſts.

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* Porterfield on the Eye, Vol. 1. p. 421. § Ib. p. 423.

Some have ſaid that the cryſtalline becomes more or leſs convex, for this purpoſe, by the action of certain muſcular fibres, which enter into its compoſition. But Dr. Porterfield obſerves, that, though the cryſtalline, when dry, appears to conſiſt of many thin concentric laminæ, or ſcales, as was mentioned before, their diſpoſition is but ill qualified for changing the figure of the cryſtalline; or, if they were ſo, it is not eaſy, he ſays, to prove that theſe fibres are muſcular, and capable of contraction.*

His own opinion is, that the cryſtalline has a motion, by means of the *ligamentum ciliare,* by which the diſtance between it and the retina is increaſed or diminiſhed, according to the different diſtances of objects. The ligamentum ciliare, he ſays, is an organ, the ſtructure and diſpoſition of which excellently qualify it for changing the ſituation of the cryſtalline, and removing it to a greater diſtance from the retina, when objects are too near for us; for that, when it contracts, it will not only draw the cryſtalline forwards, but it will alſo compreſs the vitreous humour lying behind it, ſo that it muſt preſs upon the cryſtalline, and puſh it towards the retina. In order to underſtand this, let C, fig. 150, repreſent the cryſtalline, and the curve lines *a o, a o,* will repreſent the ligamentum ciliare. Now it is eaſy to ſee, that when this ligament contracts, it muſt draw the cryſtalline forwards, in the direction of the right line *a o d, a o d,* by which means it will be brought nearer to the forepart of the eye.

He alſo obſerves, that the fibres which compoſe this ligament do not run in a ſtraight line, but form a hollow, and that therefore, when they contract, they muſt come nearer to the ſtraight lines *ao*, *ao*, by which means the concavity will become leſs, and the vitreous humour will be compreſſed, and thereby puſh the cryſtalline more forwards.

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* Porterfield on the Eye, Vol. 1, p. 442.

He adds, that the cryſtalline being moved forwards, it muſt, at the ſame time, preſs the aqueous humour againſt the cornea; by which means that membrane, which is flexible, will be rendered more convex, and enable us ſtill better to ſee near objects diſtinctly.*

That the ſituation of the cryſtalline is made uſe of, in conforming the eye to the diſtinct view of objects, placed at different diſtances, Dr. Porterfield thinks is very evident from what is obſerved concerning perſons who have cataracts couched; for the ſame lens is not uſeful to them for ſeeing all objects diſtinctly, but they are obliged to make uſe of glaſſes of different degrees of convexity, in proportion to the nearneſs of the object.§

To the objection of M. de la Hire, and others, among whom are the celebrated anatomiſts Haller and Zinn, ‡ that the ciliary ligament is not muſcular, and conſequently has no power of contraction, Dr. Porterfield obſerves, that they have been led into this miſtake by a wrong notion they have entertained about the colour of muſcles, as if they were always *red*. But this, ſays he, is not the caſe univerſally, for the muſcular fibres of the inteſtines and ſtomach have hardly any redneſs in their colour. It is alſo certain, he ſays, that the pupil contracts and dilates itſelf according as objects are more or leſs luminous, and yet none of the fibres which perform that action are in the leaſt red.||

* Porterfield on the Eye, Vol. 2, p. 447. § Ib. p. 434. ‡ Muſchenbroeck's Introductio, p. 749. || Ib. p. 450.

It has alſo been objected to our ſeeing diſtinctly at different diſtances by means of the motion of the cryſtalline, that, upon computation, this motion is found inſufficient to procure diſtinct viſion at the diſtances to which the ſight reaches; but Dr. Porterfield obſerves that, as all ſuch computations are founded upon the meaſures of the ſeveral parts of the eye, their diſtances from each other, and the refractive power of the humours, none of which it is poſſible to aſcertain with ſuch exactneſs that it may be depended on, this objection, is but of little weight; and beſides, ſuppoſing that the diſtance betwixt the cryſtalline and retina could not be ſufficiently increaſed by the motion of the cryſtalline, for enabling us to ſee diſtinctly at the leaſt diſtance the eye can reach to, yet what is wanting in this will be ſupplied by the greater convexity of the cornea. For when the cryſtalline is brought forward, its diſtance from the retina is not only increaſed, but the cornea itſelf is alſo rendered more convex, which had not been attended to by the authors of this objection.*

Dr. Jurin's hypotheſis concerning the method whereby we are able to view remote objects, deſerves to be recited. When the eye is to be ſuited to greater diſtances than 15 or 16 inches, he ſuppoſes the *ligamentum ciliare* to contract, ſo as to draw part of the anterior ſurface of the capſula of the cryſtalline, into which the fibres of it are inſerted, a little forwards and outwards, on which the water within the capſula muſt flow from under the middle, towards the elevated part of it; and the aqueous humour muſt flow from above the elevated part of the capſula to the middle. In conſequence of this the whole anterior ſurface, within the inſertion of the ligamentum ciliare, will be reduced

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* Porterfield on the Eye, Vol. 1, p. 447.

to a leſs convexity. When this contraction ceaſes, the capſula will return to its former ſituation, by its own elaſticity. The capſula, he ſays, being a very tender membrane, and containing water between its inner ſurface and the cryſtalline, can readily obey the effort of the ligamentum ciliare, though ſo weak a muſcle.

It may be objected to this hypotheſis, that unleſs the water within the capſula have a greater refractive power than the aqueous humour, the retiring of it from one place to another, to make room for that humour, will have no effect upon the pencils of rays. Dr. Jurin, however, not attending to this circumſtance, and ſeeming to conſider the water within the capſula, as having the ſame refractive power with the cryſtalline itſelf, attempts to ſhew by calculation, that this change in the convexity of it is quite ſufficient to extend the natural diſtance of diſtinct viſion from 15 inches, to 14 feet 5 inches,* without the leaſt motion of the cryſtalline itſelf, and a very ſmall one of the anterior ſurface of the capſula.†

M. Muſchenbroeck, or rather Albinus (whoſe anatomical obſervations on the eye he has publiſhed in his *Introductio, &c)* ſuppoſes that the change of conformation in the eye is performed by means of the *zona ciliaris*, in the following manner. In viewing a very near object, in conſequence of which the pencils of rays tend to a focus beyond the retina, the zona ciliaris, and the anterior membrane of the capſula, as alſo the vitreous humour, being driven forward by the compreſſion of the coats of the eye, puſh the cryſtalline, and make it recede from the retina. At the ſame time the cryſtalline, puſhing the aqueous humour into the cornea, makes it more prominent. Perhaps alſo, he ſays, the cryſtalline may be made rounder, ſo that, on three accounts, the pencils will come to their foci ſooner than otherwiſe. On the other hand, when the object is too remote for diſtinct viſion, ſo that the pencils come to their foci too ſoon, the zona ciliaris becomes tenſe, and, with the anterior membrane of the capſula, puſhes the cryſtalline farther within the vitreous humour. By this preſſure the cryſtalline becomes flatter, ſo that on theſe ſeveral accounts, the foci of the pencils are carried farther. He ſays it was formerly thought that the place of the cryſtalline was changed by the proceſſus ciliaris, but that this opinion was rejected, becauſe the proceſſus were not found to be muſcular, and detached from the cryſtalline.* It is much more probable, he ſays, that the form of the cryſtalline ſhould be changed by the action of the anterior membrane of the capſula, than by the proceſſus ciliares, which at the extremities are detached from it, and lie ſo obliquely, that they cannot act directly upon the cryſtalline.§

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* Several philoſophers ſpeak of the *fartheſt* as well as the *neareſt* limit of diſtinct viſion ; but this language is evidently improper ; ſince, if an object be large enough to ſubtend a ſufficient angle at the eye, it matters not at what diſtance it be placed, it being ſeen with equal diſtinctneſs. If perſons be not ſhort-ſighted, they can ſee by parallel rays, and ſome even with converging ones. What they mean by the *fartheſt limit of diſtinct viſion* ſeems to be the greateſt diſtance at which a book of a middle ſized print may be read. This is plainly Dr. Jurin's meaning, for he ſays, p. 141, that this limit varies with the ſize of the print.

† Eſſay on diſtinct and indiſtinct viſion, p. 143.

The zona ciliaris, and the anterior membrane of the capſula, can only puſh the cryſtalline into the vitreous humour one half of its own thickneſs, which, he ſhews, is not ſufficient to make viſion diſtinct at a competent extent of diſtance, and therefore concludes that ſome change muſt take place in the form of the cryſtalline, as he ſays Dr. Pemberton has well demonſtrated; ſince, though we generally ſee objects by means of diverging rays, yet in uſing microſcopes, we ſee by means of parallel, and ſometimes even converging rays.*

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* Muſchenbroeck's Introductio, Vol. 2, p. 759. § Ib. p. 760.

Since it is impoſſible that the ſclerotica ſhould be compreſſed in many animals, in which it is perfectly hard, and the eyes of all animals are, in the chief reſpects, ſimilar, he thinks it probable, that the proviſion for ſuiting the eye to different diſtances is the ſame in them all, and therefore that it does not depend upon the change of the ſclerotica in any of them.§

CHAPTER III.

Observations on SQUINTING.

SQUINTING is a circumſtance relating to viſion concerning which there has been a conſiderable variety of opinions among philoſophers. This defect was generally ſuppoſed to proceed from the want of a due correſpondence in the muſcles of the eyes, which, not acting in a proper concert with one another, are not able to point both eyes at one object; but M. De la Hire imagined, that this deformity did not ariſe from any depraved habit, or want of conſent in the muſcles of the eyes, but from a defect in the eye itſelf, having the moſt ſenſible part of the retina not placed in the axes of the eye, but at ſome diſtance from it, on the one ſide or on the other; and that, therefore, not the axis, but this more ſenſible part of the retina is turned towards the object on which the axes of the other eye is fixed, ſo that both axis are not directed to the ſame point.

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* Muſchenbroeck's Introductio, p. 760. § Ib. p. 761.

This, and other hypotheſes are clearly refuted by Dr. Jurin, who gives the following ſenſible account of this defect in viſion. When we look directly forwards at a diſtant object, the pupil of each eye lies in the middle of the aperture formed by the eye lids. In looking at near objects, the diſtance between the pupils is ſomething leſs, but ſtill it is always the ſame in all oblique directions of the eyes, as well as when we look ſtraight forwards; by which means the axes of both eyes are directed towards the ſame point, in both theſe caſes of looking at diſtant and near objects. But in thoſe who ſquint, when the pupil of the undiſtorted eye is ſeated in the middle of the aperture, as in looking directly forward, the pupil of the other eye is generally drawn cloſe to the noſe, and conſequently the diſtance between the two pupils continues the ſame in all oblique directions of the eyes, ſo that the two axes are never pointed at the ſame object, though the muſcles do ſo far act in concert with each other, as to move both eyes the ſame way at the ſame inſtant of time.

This vicious habit, he obſerves, may eaſily be contracted by a child, if he is often laid into his cradle, in ſuch a poſition as to be able to ſee either the light, or any other remarkable object, with one eye only. And when, by this means, he is brought to ſquint, and the habit is confirmed, he apprehended it would be in vain to attempt to cure him by his wearing tubes, or ſhells with ſmall holes in them to look through, which it ſeems, had been practiſed before.

The true method of cure he took to be the following. When the child is arrived at ſuch an age as to be capable of obſerving directions, place him, ſays he, directly before you, and let him cloſe the undiſtorted eye, and look at you with the other. When you find the axis of this eye fixed directly upon you, bid him endeavour to keep it in that ſituation, and open his other eye. You will now ſee the diſtorted eye turn away from you towards his noſe, and the axes of the other eye will be pointed towards you. But with patience and repeated trials, he will, by degrees, be able to keep his diſtorted eye fixed upon you, at leaſt for ſome little time, after the other is opened; and when you have brought him to keep the axis of both eyes fixed upon you, as you ſtand directly before him, it will be time to change his poſture, and to ſet him firſt a little to one ſide of you, and then to the other, and ſo to practiſe the ſame thing. And when, in all theſe ſituations, he can perfectly and readily turn the axes of both eyes towards you, the cure is effected. An adult perſon he ſays, may practiſe all this by a glaſs, without a director, though not ſo eaſily as with one; but the older he is, the more patience will be neceſſary.

Our author once attempted a cure after this manner, upon a young perſon about nine years of age, with promiſing hopes of ſucceſs, but was interrupted by his falling ill of the ſmall pox, of which he died.*

The buſineſs of ſquinting was far more accurately conſidered by M. Buffon, who, from a great number of obſervations, found that the true and original cauſe of it is an inequality in the goodneſs, or in the limits of diſtinct viſion in the two eyes. When one of the eyes, he ſays, is much weaker than the other, we do not direct it towards the object, but make uſe of the ſtronger eye only; for the ſame reaſon that we commonly make uſe of the right arm in preference to the left. When the eyes are equally good, we ſee more diſtinctly with both eyes than with one, by about a thirteenth part; but when the eyes are unequal in goodneſs, objects appear leſs diſtinct with both eyes than with one. If, for inſtance, the diſtance at which a perſon can read a ſmall print be from 8 to 20 inches with the ſtronger eye, and only from 8 to 15 with the weaker, diſtinct viſion will be limited to 7 inches, viz. from 8 to 15 for both eyes; and as the image in the good eye will be ſtronger than that in the weak one, the impreſſion upon them both will not be ſo diſtinct, as if the good eye only had been uſed. It is no wonder, therefore, that ſuch perſons chuſe to make uſe of one eye only, and turn the other aſide.*

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* Smith's Opticks. Remarks, p. 30.

Our author does not aſſert that this inequality in the goodneſs of the eyes is the only cauſe of ſquinting. He only ſays, that it is the general cauſe, ſo that others may be termed accidental ones with reſpect to it. All the perſons that he examined, if they ſquinted at all, did ſo from this cauſe, and this effect is ſo neceſſary, that he ſays, it is not poſſible to cure a perſon of ſquinting whoſe eyes differ much in point of goodneſs, except, perhaps, by firſt bringing them nearer to an equality, by means of glaſſes.§ Dr. Reid obſerves that, having examined above twenty perſons who ſquinted, he found in all of them a defect in the ſight of one eye. Four of them only had ſo much of diſtinct viſion in the weak eye as to be able to read with it, while the other was covered. The reſt ſaw nothing at all diſtinctly with one of the eyes.*

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* Ac. Par. 1743, M. P. 329. § Ib. p. 338.

In examining the limits of diſtinct viſion in ſeveral children who did not ſquint, M. Buffon found (contrary to the obſervations of Dr. Jurin above mentioned) that they did not ſee by any means either ſo far off, or ſo near, as adult perſons, ſo that, as we advance in age, the limits of diſtinct viſion are extended both ways; and this, he ſays, is one reaſon why children ſquint more than adult perſons. For if it require only $\frac{3}{10}$, or a leſs degree of inequality, to produce ſquinting when the limits of diſtinct viſion are ſmall, it will require $\frac{3}{8}$, or a greater inequality, to produce ſquinting when thoſe limits are greater; ſo that this defect may be expected to grow leſs with age.

When the eyes of thoſe perſons who ſquint do not differ much in point of goodneſs, he thought that the moſt ſimple, the moſt natural, and effectual method of curing it, would be to cover the good eye for ſome time. For that, in this caſe, the diſtorted eye would be obliged to act, and turn itſelf directly towards objects; and by degrees it would become natural to it. Some oculiſts, he was informed, had ſucceeded in this method; but before recourſe is had to it, he adviſes that the inequalities in the eyes be well aſcertained, becauſe it will never ſucceed if the difference in the eyes be very great. Our author having communicated his ſcheme to ſeveral perſons, and among others to M. Bernard de Juſſieu, to whom he read part of his Memoir, he had the ſatisfaction to find his opinion confirmed by an experiment of that gentleman, and which is recited by Mr. Allen in his *Synopſis Univerſæ Medicinæ*.*

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* Inquiry into the human Mind, p. 253.

The reaſon why thoſe perſons who ſquint generally turn the weak eye towards the noſe, he ſays, is that, in that ſituation, the direction of its axis is as diſtant as poſſible from that of the good eye; and beſides, in this place, the noſe conceals many objects from its view, ſo that this ſituation is the leaſt diſadvantageous to it of any other.†

In ſome caſes, our author obſerves, that the eyes have what is commonly called a *caſt* only, in conſequence of the optic angle being too great, or too ſmall, when they are of equal goodneſs. This, he ſays, is generally the reſult of habit, acquired very early, in conſequence of the miſmanagement of children, and that it is often eaſily cured. If the eye that ſquints be turned towards the temples, he ſays he has generally found that there is no great inequality in the goodneſs of the two, and that, in this caſe, the cauſe being only a vicious habit, the cure has been completed by covering the good eye for a fortnight only.§

Mr. Buffon concludes this curious memoir with obſerving, that a weak eye acquires ſtrength by exerciſe, and that many perſons, whoſe ſquinting he had thought to be incurable, on account of the great inequality in the goodneſs of their eyes, having covered their good eye a few minutes only, and conſequently being obliged to exerciſe their bad eye for that ſhort time, were themſelves ſurprized at the ſtrength it had acquired; ſo that meaſuring their view afterwards, he found it more extended, and judged the ſquinting to be curable. In order, therefore, to judge with any certainty of the inequality of goodneſs in the two eyes, and the poſſibility of the cure of ſquinting, it is neceſſary, he ſays, to cover the good eye for ſome time, in order to exerciſe the bad eye, and give it an opportunity of gaining ſtrength, after which it will be eaſier to judge whether the cure be poſſible or not.*

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* Ac. Par. 1743, M. p. 335. † Ib. p. 337. § Ib. p. 342.

After giving a good deal of attention to this ſubject, and after having examined the eyes of ſeveral perſons who ſquinted, Dr. Reid is of opinion, that the centers of the two eyes correſpond in them as in other perſons, ſo that if they could be brought to the habit of directing their eyes aright to an object, they would not only remove a deformity, but likewiſe improve their ſight.§

Several circumſtances relating to viſion, and more eſpecially to *ſquinting*, are well explained by ſome of Dr. Hartley's obſervations on the ſtructure of the eye. The following relate to the motion of the globe of the eye. The white tendinous expanſions of the four ſtraight muſcles reach, he ſays, as far as the cornea, and conſequently they are thereby expoſed, in a particular manner, to the action of light, when the eye is open, being covered by nothing but a thin membrane. However, the tendinous expanſions of the *adducens* and *abducens* are much more expoſed than thoſe of the *attollens* and *deprimens*; and if the eye be but a little open, the light cannot fall upon theſe laſt at all.

If a luminous object be placed upon the right ſide, ſo as that the light ſhall fall in a particular manner upon the tendinous expanſion of the abducens of the right eye, and of the adducens of the left, it may, by contracting theſe muſcles, make the eyes move in a congruous manner, turning them towards itſelf; and the tendency of the eyes to move towards the right ſide ought not to ceaſe till the adducens and abducens in each eye have an equal quantity of light fall upon them from the luminous object, i. e. till the optic axis be directed to it. In which caſe the eyes would be in æquilibrio, as far as they are under the influence of the light which falls upon the tendinous expanſions of the ſtraight muſcles.

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* Ac. Par. 1743, M. p. 345.

§ Inquiry into the human Mind, p. 257.

It agrees remarkably with this, that new born children move their eyes in a congruous manner, that the motions are chiefly to the right and left, ſcarce upwards and downwards at all, the eyelids being ſeldom ſo much opened as to expoſe the tendinous expanſions of the attollens and deprimens, and that their eyes are frequently turned towards luminous objects, ſuch as a candle, or a window.*

As the two oblique muſcles neither have tendinous expanſions expoſed to the light, nor adhere to the globe of the eye, except juſt at their inſertion, they cannot be under either of the influences here ſuppoſed to affect the ſtraight muſcles, but muſt be ſubject chiefly to thoſe which deſcend from the brain, thus acting almoſt uniformly, unleſs in particular agitations of the whole nervous ſyſtem. And this agrees well with the fact, and with the uſes generally aſſigned to theſe muſcles, viz. thoſe of keeping the eye in a moderate ſuſpenſion always, and drawing it out on eminent occaſions.

The circumſtances which occaſion ſquinting in young children agree well with the theory here propoſed. Thus, if a child be laid ſo into his cradle, as that one eye ſhall be covered, the external influences of light cannot operate upon it. And if this be often repeated, eſpecially while the aſſociation, which confirms the congruity of the motions, is weak, the eye which is covered will obey the influences which deſcend from the brain, and turn upwards and inwards for the moſt part.*

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* Obſervations on Man, Vol. 1, p. 215.

CHAPTER IV.

MISCELLANEOUS AFFECTIONS OF THE EYE.

DR. Jurin's obſervations on the changes that are made in the eye by *age* are very curious, and well deſerve a place in this ſection, of miſcellaneous affections of the eye. In the eyes, he obſerves, as well as in all other parts of the body, the muſcles, by conſtant exerciſe, are enabled to contract with more ſtrength, with more eaſe, and to a greater degree, and that theſe powers are loſt by a diſuſe of exerciſe. Alſo the elaſtic parts of the eye, like thoſe of the reſt of the body, if they be often and long kept in tenſion, by degrees loſe ſomething of their elaſticity; and if they are ſeldom exerciſed, they alſo grow ſtiff, and are not ſo eaſily diſtended.

Theſe cauſes, he ſays, will account for thoſe changes of the eye which habit and cuſtom introduce. For this reaſon perſons who are much accuſtomed to view remote objects ſee better at great diſtances than other people, as is often the caſe with travellers, ſailors, ſportſmen, &c.

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* Hartley's Obſervations on Man, Vol. 1, p. 217.

On the other hand, perſons who are much accuſtomed to view objects at ſmall diſtances, as ſtudents in general, watchmakers, engravers, painters in miniature, &c. ſee better at ſmall diſtances, and not ſo well at great diſtances, as the reſt of mankind.

In children the pupil is uſually more dilated than in grown perſons. The reaſon of this he apprehends to be, that in children the cornea is extremely flexible, ſo as to be very eaſily bent by its muſcular ring into any curvature that is neceſſary for ſeeing diſtinctly in reading, and conſequently their pupil has leſs occaſion to contract for diſtinct viſion. But in grown perſons the cornea is ſtiffer, ſo that they have more occaſion to contract the pupil; and in elderly perſons the cornea grows ſtill more rigid, ſo that they can hardly read without ſpectacles, unleſs the print be very large, or the light be very ſtrong, ſo as to cauſe a great contraction of the pupil. It is for this reaſon that they are obliged to hold the candle between their eye and the paper on which they read; and their doing ſo is a certain ſign that they begin to want ſpectacles.

Children read much nearer than grown perſons, both, as our author obſerves, becauſe their eye is ſmaller, and becauſe their cornea is more flexible; and the leaſt diſtance at which any eye can ſee diſtinctly is proportional to the length of the eye.

Elderly perſons ſee better at a great diſtance than younger people. The fact our author thinks is pretty generally acknowledged, but if it be queſtioned may be thus examined. Let a perſon whoſe ſight for near objects is impaired by age obſerve the moon at 3 or 4 days old, and take notice whether the illuminated part appears to be of a larger diameter than the dark part, and how much is the exceſs; and let him recollect, if he can, what was the appearance of thoſe two parts of the moon when he was much younger. Our author was perſuaded that he would think the exceſs of the light part above the dark to be either none at all, or much leſs than formerly. This, at leaſt, was his own caſe. He well remembered that formerly the diameter of the light part of the moon much exceeded that of the dark part, whereas, at the time that he wrote, the limbs of thoſe two parts appeared nearly to unite in the ſame circle.

The cornea, he obſerves, being of a rarer texture, and more expoſed to the air, will, in length of time, ſhrink a little more than the ſclerotica, and by that means grow a little flatter than it was before, and therefore old people do not ſee ſo well at ſmall diſtances as younger perſons; and this, together with the rigidity of the cornea, may carry out the neareſt limit of perfect viſion from 3 or 4 inches, as in children, and from about 5 or 6 inches in young adult perſons to 20, 30, or 40 inches, or a greater diſtance. And in this caſe the eye has no aſſiſtance in viewing near objects, but only from the contraction of the pupil, and this is not ſufficient for diſtinct viſion, unleſs in a ſtrong light.

Our author concludes theſe remarks with obſerving, that as the juices of grown animals are ſtronger, and more loaded with animal ſalts and oils than thoſe of younger ones, it is probable that the refraction of the coats and humours of the eye becomes ſomething greater as we advance in age, than when we are children. For all ſulphureous ſubſtances refract more ſtrongly *cæteris paribus*, than others, and M. Petit obſerves that the cryſtalline humour, which is at firſt perfectly colourleſs, has, in adult perſons, a yellowiſh tinge, which grows ſtronger as they advance in years. The gradual increaſe of this yellowiſh tinge, and the increaſing refraction which depends upon it, our author ſuppoſes, may, in ſome meaſure, counteract the increaſing ſtiffneſs and flatneſs of the cornea; and that otherwiſe we ſhould grow long ſighted, and want ſpectacles ſooner than we do.*

M. Æpinus obſerves, that from his infancy his eyes were ſubject to be affected in a very particular manner, for when ever he looked upon any white object, it had the appearance of ſomething reſembling tranſparent threads, very much entangled with one another, and having little knots in them. Alſo, among theſe were a kind of nebulous round ſpots. The ſeat of this diſeaſe he ſeems pretty clearly to prove to have been in the vitreous humour.§ But as this affection of the eye does not appear to be common, I ſhall be excuſed in not reciting his very particular obſervations and reaſoning upon it.

In this chapter I ſhall introduce ſuch obſervations as I have met with, relating to the varieties of viſion with one or two eyes.

It was the opinion of Sir Iſaac Newton and others, that objects appear ſingle becauſe the two optic nerves unite before they reach the brain. But Dr. Porterfield ſhews, from the obſervation of ſeveral anatomiſts, that the optic nerves do not mix, or confound their ſubſtance, being only united by a cloſe coheſion; and objects have appeared ſingle where the optic nerves were found to be disjoined.†

Dr. Briggs ſuppoſed that ſingle viſion was owing to the equal tenſion of the correſponding parts of the optic nerves, whereby they vibrated, in a ſynchronous manner. But, beſides ſeveral improbable circumſtances in this account, Dr. Porterfield ſhews that facts do by no means favour it.

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* Eſſay on diſtinct and indiſtinct Viſion, p. 146.

§ Nov. Com. Petrop. Vol. 10, p. 290.

† Vol. 2, p. 285.

To account for this phenomenon, this ingenious writer ſuppoſes that, by an original law in our natures, we imagine objects to be ſituated ſomewhere in a right line drawn from the picture of it upon the retina, through the center of the pupil. Conſequently, the ſame object appearing to both eyes to be in the ſame place, the mind cannot diſtinguiſh it into two. In anſwer to an objection to this hypotheſis, from objects appearing double when one eye is diſtorted, he ſays the mind miſtakes the poſition of the eye, imagining that it had moved in a manner correſponding to the other, in which caſe the concluſion would have been juſt. In this he ſeems to have recourſe to the power of habit, though in words he diſclaims that hypotheſis.

This principle however, has generally been thought to be ſufficient to account for this appearance. Originally, every object making two pictures, one in each eye, is imagined to be double; but, by degrees, we find that when two correſponding parts of the retina are impreſſed, the object is but one; but if thoſe correſponding parts be changed, by the diſtortion of one of the eyes, the object muſt again appear double as at the firſt. This ſeems to be verified by Mr. Cheſelden, who informs us that a gentleman who, from a blow on his head had one eye diſtorted, found every object to appear double; but by degrees the moſt familiar ones came to appear ſingle again, and in time all objects did ſo, without any amendment of the diſtortion.* A caſe ſimilar to this is mentioned by Dr. Smith.§

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* Porterfield on the Eye, Vol. 2, p. 315. § Opticks Remarks, p. 31.

On the other hand Dr. Reid is of opinion, that the correſpondence of the centers of the two eyes, on which ſingle viſion depends, does not ariſe from cuſtom, but from ſome natural conſtitution of the eye and of the mind. He makes ſeveral juſt objections to the caſe of Mr. Foſter, recited by Dr. Smith and others, and thinks that the caſe of the young man couched by Cheſelden, who ſaw ſingly with both eyes, immediately upon receiving his ſight, is nearly deciſive in proof of his ſuppoſition. He alſo found that three young gentlemen, whom he endeavoured to cure of ſquinting, ſaw objects ſingly, as ſoon as ever they were brought to direct the centers of both their eyes to the ſame object, though they had never been uſed to do ſo from their infancy; and he obſerves that there are caſes, in which, notwithſtanding the fulleſt conviction of an object being ſingle, no practice of looking at it will ever make it appear ſo, as when it is ſeen through a multiplying glaſs. *

We are indebted to Dr. Jurin for the following curious experiments to determine whether an object ſeen by both eyes appears brighter than when ſeen with one only.

He laid a ſlip of clean white paper directly before him on a table, and applying the ſide of a book cloſe to his right temple, ſo as that the book was advanced conſiderably more forward than his face, he held it in ſuch a manner, as to hide from his right eye that half of the paper which lay to his left hand, while the left half of the paper was ſeen by both eyes, without any impediment.

Then, looking at the paper with both eyes, he obſerved it to be divided, from the top to the bottom, by a dark line, and the part which was ſeen with one eye only was manifeſtly darker than

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* Inquiry, p. 267.

that which was ſeen with both eyes; and, applying the book to his left temple, he found, by the reſult of the experiment, that both his eyes were of equal goodneſs.

He then endeavoured to find to what degree this exceſs of brightneſs amounted; and comparing it with the appearance of an object illuminated partly by one candle and partly by two, he was ſurpriſed to find that an object ſeen with both eyes is by no means twice as luminous as when it is ſeen with one only; and after a number of trials, by which he made the proportion leſs and leſs continually, he found that when one paper was illuminated by a candle placed at the diſtance of 3 feet, and another paper by the ſame candle at the ſame diſtance, and by another candle, at the diſtance of 11 feet, the former ſeen by both eyes, and the latter with one eye only, appeared to be of equal whiteneſs; ſo that an object ſeen with both eyes appears brighter than when it is ſeen with one only by about a 13th part. But he acknowledges that it is difficult to make this experiment exactly. *

He then proceeded to inquire whether an object ſeen with both eyes appears any thing larger than when ſeen with one only, but he concluded that it did not, except on account of ſome particular circumſtances, as in the caſe of the binocular teleſcope, and the concave ſpeculum.†

M. Du Tour maintains that the mind attends to no more than the image made in one eye at a time, and produces ſeveral curious experiments in favour of this hypotheſis; but, as M. Buffon obſerves, it is a ſufficient anſwer to this hypotheſis, how ingeniouſly ſoever it may be ſupported, that we ſee more diſtinctly with two eyes than one; and that when a round object is near

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* Smith's Opticks, Remarks, p. 107. † Ib. p. 109.

us we plainly ſee more of the ſurface in one caſe than in the other. There are, alſo, other facts, which clearly prove the contrary of what is maintained by M. Du Tour. * However, as there is ſomething in M. Du Tour's experiments that may excite the attention of the curious, I ſhall recite ſome of the principal of them. Upon one ſide of a ſheet of paſteboard he glued a circle of blue taffeta, an inch in diameter, and on the other ſide exactly oppoſite to it another circle of yellow taffeta, of the ſame diameter. This paſteboard he held vertically before his noſe, in ſuch a manner, that with his right eye he could only perceive the blue circle, and with his left the yellow. In this ſituation, when both his eyes were open, the ſpot never appeared green, as it would have done by a mixture of the two colours; but always either blue or yellow, as the impreſſion made upon his eye by the one or the other colour happened to be the ſtronger. And ſometimes the circle appeared partly blue and partly yellow, probably becauſe one part of the circle might be more advantageouſly ſituated with reſpect to one of his eyes than the other. From this he thought it might be concluded, that the mind cannot be affected at the ſame time by two correſponding points of two images.†

As this experiment requires a poſition of the eye that is rather painful, the ſame gentleman, in a ſubſequent paper, propoſed another, which may be tried with more eaſe. He took two circles of glaſs, an inch in diameter, one blue, and the other yellow, through which, placed together, objects appeared green. Theſe glaſſes he fixed in a tube of paſteboard, black on the inſide, three or four inches long, and of the ſame diameter with

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* Ac. Par. 1743, M. p. 334.

† Memoires Preſentés, p. 514.

the glaſſes. One of theſe tubes he applied to his right eye, and the other to his left; and looking through them on a piece of white paper, at four or five feet from him, it always appeared either blue or yellow, but never green. He alſo ſays, that if a priſm be held vertically before one eye only, while both of them are open, the colours of the refracted light, which falls upon one eye, will not mix with the unrefracted light that falls upon the other eye, but the objects are thoſe the images of which are painted in one eye only. Sometimes indeed they are mixed, but never confuſed.*

Laſtly, in an appendix to another paper, he mentions a very ſimple experiment, but which he thinks deciſive in favour of his doctrine, viz. that of two images of an object, the mind is affected only by one of them at the ſame time. If two round holes, 12 or 15 lines in diameter, ſeparated by an interval of four or five lines, be made in a ſheet of white paper, and the edges of one of the holes be painted red, and the other white, a perſon ſtanding at the diſtance of 4 or 5 feet from an object, of a middling ſize, with this ſheet of paper held in ſuch a manner, that to one of his eyes the object ſhall appear in the center of the red circle, and to the other in that of the white, it will never appear in both the circles at a time, though both of them will always be very diſtinguiſhable; nor will it appear behind the interval that ſeparates them, but always in the middle of one of the circles only.§

With reſpect to ſingle viſion with two eyes, Dr. Hartley obſerves, that it deſerves particular attention, that the optic nerves of men, and ſuch other animals as look the ſame way with both eyes, unite in the *Sella Turcica* in a ganglion, or little brain, as one may call it, peculiar to themſelves, and that the aſſociations between ſynchronous impreſſions on the two retinas muſt be made ſooner, and cemented ſtronger on this account; alſo that they ought to have a much greater power over one another's images, than in any other part of the body. And thus an impreſſion made on the right eye alone, by a ſingle object, may propagate itſelf into the left, and there raiſe up an image almoſt equal in vividneſs to itſelf; and conſequently when we ſee with one eye only, we may, however, have pictures in both eyes.*

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* Memoires Preſentés, Vol. 4, p. 499.

§ Ib. Vol. 5, p. 677.

A curious deception in viſion, ariſing from the uſe of both eyes, was obſerved and accounted for by Dr. Smith. It is a common obſervation, he ſays, that objects ſeen with both eyes appear more vivid, and ſtronger than they do to a ſingle eye, eſpecially when both of them are equally good. A perſon not ſhort-ſighted may ſoon be convinced of this fact by looking attentively at objects that are pretty remote, firſt with one eye, and then with both. This obſervation gave occaſion to the conſtruction of the binocular teleſcope, in the uſe of which the phenomenon is ſtill more ſtriking.

Beſides this, our author obſerves, that there is another phenomenon obſervable with this inſtrument, which is very remarkable. In the foci of the two teleſcopes there are two equal rings, as uſual, which terminate the pictures of the objects there formed, and conſequently the viſible area of the objects themſelves. Theſe equal rings, by reaſon of the equal eye glaſſes, appear equal, and equally diſtant when ſeen ſeparately by each eye; but when they are ſeen with both eyes, they appear much larger, and more diſtant alſo ; and the objects ſeen through them do alſo appear much larger, though circumſcribed by their united rings, in the ſame places as when they were ſeen ſeparately.

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* Hartley's Obſervations on Man, Vol. 1. p. 207.

Endeavouring to explain this odd appearance, he obſerved another ſtill more ſurpriſing. Having opened the points of a pair of compaſſes ſomewhat wider than the interval of his eyes, with his arm extended, he held the head or joint in his hand, with the points outwards, and equidiſtant from his eyes, a little higher than the joint. Then fixing his eyes on a remote object, lying in the line that biſected the intervals of the points, he firſt perceived two pairs of compaſſes, each leg being doubled, with their inner legs croſſing each other. But, by compreſſing the legs with his hand, the two inner points came nearer to each other, and when they united, the two inner legs alſo intirely coincided, and biſected the angle under the outward ones ; and they appeared more vivid, thicker, and longer, ſo as to reach from his hand to the remoteſt object in view, even in the horizon itſelf. This appearance continued the ſame, to whatever collateral objects he directed his eyes, nor would it vaniſh by variouſly inclining the plane of the legs to the horizon, or by any other means than looking directly at them.

The like appearance, he ſays, will happen when two equal round ſlices of cork, or any two equal ſurfaces, are ſtuck upon the points of the compaſſes, In fig. 151, the points of the compaſſes are *ab*, the head or joint *c*, the eyes of the obſerver *d*, and *e*, the imaginary leg *cf*, biſecting the angle *acb*.

The reaſon of the phenomenon he apprehended to be this. By ſhutting the eyes *d*, *e*, alternately, it will be evident, he ſays, that the points *d*, *a*, *f*, are in one ſtraight line, and the points *e*, *b*, *f* in another; and ſo the reaſon of the apparent union of the points *a* and *b*, at the object *f*, is becauſe their pictures are upon the ſame points of the retina as thoſe of the object itſelf. For the ſame reaſon, while the legs of the compaſſes are gradually compreſſed, if the eye be kept fixed upon the imaginary point of the united legs, this point will appear to approach gradually; which ſhews why any two points of the legs, as *g* and *h*, equidiſtant from the extremes, appear nearer and nearer to the eyes, as they lie nearer and nearer to the head of the compaſſes.*

The ſame writer obſerves, that the phenomenon of the enlarged circle of the viſible area in the binocular teleſcope, may be ſeen very plainly in looking at diſtant objects through a pair of ſpectacles, removed from the eyes about 4 or 5 inches, and held ſteady at that diſtance. The two innermoſt of the 4 apparent rings, which hold the glaſſes, will then appear united in one larger, and more diſtant ring, than the two outermoſt, which will hardly be viſible, unleſs the ſpectacles be farther removed. §

Dr. Jurin, holding a book pretty cloſe to his eye, when it was illuminated by a candle, and he was directing his eyes towards the cieling of the room beyond them, part of which he could ſee with one eye, part with the other, and part with both, while he was amuſing himſelf with obſerving the different ſhadows they formed, was ſtruck with the appearance of the middle and darkeſt part of the ſhadow ſeeming to be in the form of a ſolid pyramid, which extended itſelf towards any object to which his eyes were directed. Having particularly deſcribed the appearance, he conſiders minutely every part of it, and endeavours to account for it.* This phenomenon ſeems to reſemble what Dr. Smith obſerved, with reſpect to the compaſſes above mentioned.

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* Smith's Opticks, Vol. 2, p. 387.

§ Ib. Remarks, p. 107.

A curious circumſtance relating to the effect of one eye upon the other, was noticed by M. Æpinus, who obſerved that, when he was looking through a hole made in a plate of metal, about the tenth part of a line in diameter with his left eye, both the hole itſelf appeared larger, and alſo the field of view ſeen through it was more extended, whenever he ſhut his right eye; and both theſe effects were more remarkable, when that eye was covered with his hand. He found conſiderable difficulty in meaſuring this augmentation of the apparent diameter of the hole, and of the field of view; but at length he found that, when the hole was half an inch, and the tablet which he viewed through it was three feet from his eye, if the diameter of the field, when both his eyes were open, was 1, it became $1\frac{1}{2}$ when the other eye was ſhut, and nearly 2 when his hand was laid upon it.

Upon examining this phenomenon, it preſently appeared to depend upon the enlargement of the pupil of one eye, when the other is cloſed, the phyſical or anatomical cauſe of which he did not pretend to aſſign; but he obſerves that it is wiſely appointed by divine providence, in order that, when one eye fails, the field of view in the other may be extended. That this effect ſhould be more ſenſible when the eye is covered with the hand is owing, he obſerves, to the eye lids not being impervious to the light.† But the enlargement of the pupil does not enlarge the field of view, except in looking through a hole, as in this particular caſe; and therefore perſons who are blind of one eye can derive no advantage from this circumſtance. Before we applaud the wiſdom of providence, in any part of the conſtitution of nature, we ſhould be very ſure that we do not miſtake concerning the effects of that conſtitution.

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* See Smith's Opticks, Remarks p. 109.

† Nov. Com. Petrop. Vol. 7, p. 303.

A great deal has been written by Gaſſendi, Le Clerc, Muſchenbroeck, and Du Tour, concerning the place to which we refer an object viewed by one or both eyes. But I do not think the ſubject of conſequence enough to recite what they have advanced. Any perſon may preſently ſatisfy himſelf with reſpect to every thing belonging to this circumſtance, either by experiment, holding his finger before his eyes, and looking at it and an object beyond it, or by figures, in which lines repreſenting the optic axes may be made to croſs one another at different diſtances from the eye. Thoſe who would chuſe to ſee what has been written on this ſubject, I refer to a paper of M. Du Tour, in the *Memoires Preſentés*, Vol. 3, p. 514, and to the authors which he there quotes.

CHAPTER V.

Observations on DISTINCT and INDISTINCT VISION.

NO philoſopher has taken ſo much pains with the theory of *diſtinct and indiſtinct viſion*, or has ſo happily explained a great number of caſes relating to it, as Dr. Jurin in his Eſſay on this ſubject, publiſhed at the end of Dr. Smith's Opticks.

He ſhews that objects may be ſeen with ſufficient diſtinctneſs, though the pencils of rays iſſuing from the points of them do not unite preciſely in another point on the retina; but that, ſince, in this caſe, pencils from every point either meet before they reach the retina, or tend to meet beyond it, the light that comes from them muſt cover a circular ſpot upon it, and will, therefore, paint the image larger than perfect viſion would repreſent it. It follows from hence, that every object placed either too near or too remote for perfect viſion, will appear larger than it is, by a penumbra of light, cauſed by the circular ſpaces, which are illuminated by pencils of rays proceeding from the extremities of the object. All the varieties occaſioned by this circumſtance, our author traces with great accuracy, and he applies his obſervations upon it to the explanation of many phenomena in viſion.

Upon this principle he ſhews, that in the new moon of three or four days old, the illuminated part muſt appear too broad in proportion to the obſcure part, and muſt, likewiſe, ſeem to extend more outwards, or to have a greater diameter than the obſcure part. For, in fig. 152, if CABDE repreſent the true image of the whole moon, ABDIA the true image of the enlightened part, and AIDEA the true image of the obſcure part, FGH will be the outer edge of the annular penumbra of the whole moon, and LGMKL the whole appearance of the enlightened part conſiſting of the true image, and the penumbra added to it. In this caſe, the whole appearance of the illuminated part is manifeſtly too broad, in proportion to the obſcure part, ſince to the true image of the illuminated part is added, not only the annular penumbra on the outſide, but alſo another equal to it on the inſide; whereas the obſcure part is leſſened by the ſpace which this laſt penumbra takes up. For a ſimilar reaſon, the light of the obſcure part diminiſhes outwards, eſpecially where it is contiguous to the ſtronger light of the cuſps. Conſequently, it muſt appear of a leſs diameter than the illuminated part. For the ſame reaſon, in an eclipſe of the ſun or moon, the bright part muſt appear too broad in proportion to the dark part, which will make the eclipſe appear leſs than it really is.

That this effect ariſes from the cauſe here aſſigned, and not from the principle that has ſometimes been made uſe of to account for ſuch like phenomena, viz. that a light object affects the retina to a greater diſtance than a dark one, our author ſays, will plainly appear from the following experiment. Let a repreſentation of the new moon, fig. 153, or the circle, fig. 154, half black and half white, be viewed at a diſtance proper for perfect viſion, and the two figures will appear in their juſt proportion; but if they be viewed at a diſtance too ſmall or too remote for perfect viſion, which may eaſily be done by the help of glaſſes, in both caſes the white part will entrench on the dark part, and alſo extend outwards beyond it. To make this experiment ſucceed clearly, the figure muſt not be bounded with a black line, as is here repreſented, but the whole circle muſt be cut in white paper, the dark part blackened with ink, and the paper laid upon a black ground.

The other planets, which, by reaſon of their greater diſtance, appear much ſmaller than the moon, Dr. Jurin obſerves, will appear fainter, but much larger by indiſtinct than by perfect viſion, and their diameters will appear enlarged in a much greater proportion than that of the moon. By this means, he ſays, we may account for the *radii adventitii*, which Mr. Horrox ſays impoſed ſo far upon all the aſtronomers before him, as to make them think the apparent diameters of the planets nine or ten times greater than they really are.*

On account of theſe luminous *penumbræ*, occaſioned by the pencils of rays from remote objects occupying a circular ſpace on the retina, it is evident that two ſtars will appear to be nearer together than they really are; and if they be very near, will appear to be but one, but brighter than either of them taken alone; becauſe part of the two faint images will coincide, and in that place will be nearly twice as luminous as the reſt of the image, ſo that the two ſtars will have the ſame appearance as if one brighter ſtar appeared in the middle of the ſpace occupied by the two ſtars. For the ſame reaſon, a ſtar approaching very near to the edge of a planet, may appear within its limb, as if the planet was tranſparent, and the other was ſeen through it; which accounts for ſome obſervations of aſtronomers to that purpoſe, and alſo for Mars or Venus, &c. appearing within the limb of the moon. The ſame appearance of a ſtar within the limb of the moon, may happen to a perſon who obſerves with a teleſcope, if the inſtrument be not a good one, or if the eye of the obſerver have not the exact conformation neceſſary for perfect viſion.†

When objects are large, they will appear tolerably diſtinct at a much leſs diſtance, than ſmall objects, becauſe the penumbræ will not interfere ſo much. For this reaſon a large print may be read much nearer to the eye than a ſmall one. In this caſe the former will appear only ill defined, but ſufficiently diſtinct, when the latter is quite indiſtinct, the penumbra of one letter interfering with that of another, and thereby making marks altogether unlike any that are in the book.*

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* Eſſay, p. 125. † Ib. p. 125.

I muſt not cloſe theſe obſervations of Dr. Jurin, on *penumbræ*, without obſerving, with our author afterwards, that the diſperſed light in this caſe is of different denſities; and with Mr. Robins, who makes ſome ſevere but judicious remarks on Dr. Jurin, that the whole circle made by the confuſed image of any print, will be proportioned to the diameter of the pupil of the eye, which limits the whole pencil.†

The ſmalleſt diſtance of perfect viſion, or that in which the rays of a ſingle pencil are collected into a phyſical point on the retina, in the generality of eyes, Dr. Jurin, from a number of obſervations, ſtates at 5, 6, or 7 inches. The greateſt diſtance of diſtinct viſion he found to be more difficult to determine; but by conſidering the proportion of all the parts of the eye, and the refractive power of each, together with the interval that may be diſcerned between two ſtars the diſtance of which is known, he fixes it, in ſome caſes, at 14 feet 5 inches, though Dr. Porterfield had confined it to 27 inches only, with reſpect to his own eye.§ For a reaſon of this difference of opinion, and a neceſſary remark on this ſubject, I muſt refer my reader to a note on p. 650.

When viſion is indiſtinct, Dr. Jurin thinks there are two methods of rendering it diſtinct. One is for the eye to apply the ſame power whereby it conforms itſelf to the view of objects placed at different diſtances, and the other the contraction of the pupil, which is chiefly made uſe of in a ſtrong light, and which will ſometimes render the other means altogether unneceſſary.* In a weak light, he ſays, the pupil is ſo far from contracting, that there is rather a neceſſity for dilating it, to take in more light. § But upon this Dr Whytt obſerves, that, in the ſame or a leſs degree of light, the pupil will be contracted, in order to view a nearer or a ſmaller object. Let any perſon, ſays he, with his back to the light, firſt look at an object of a lively colour, at the diſtance of three or four feet from his eyes, and afterwards at a dark one, at the diſtance of a foot, and the pupil will be obſerved to become ſenſibly narrower when he looks at the nearer object, though the quantity of light reflected from it be leſs.†

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* Eſſay, p. 133. † Robins's Remarks, p. 279. § Ib. p. 134.

In the laſt part of his Eſſay Dr. Jurin diſcuſſes the curious queſtion which had been the occaſion of a debate between Dr. Hooke and Hevelius, about the leaſt viſible angle. Dr. Hooke aſſerted that no object could well be ſeen if it ſubtended an angle leſs than a minute; and if the object be round, as a black circular ſpot upon a white ground, or a white circle upon a black ground, it follows from an experiment made by Dr. Smith, that this rule is near the truth; and from thence he calculates that the diameter of the picture of ſuch leaſt viſible point upon the retina is the 8000th part of an inch, which he therefore calls a ſenſible point of the retina.‡ On the other hand M. Courtivron concluded, from his experiments, that the ſmalleſt angle of viſion is 40 ſeconds.||

Dr. Jurin enters much farther into this curious ſubject, and ſays, that there are caſes in which a much leſs angle than one minute can be diſcerned by the eye; and in order to throw light upon the ſubject, he obſerves that, in order to our perceiving the impreſſion made by an object upon any of our ſenſes, it muſt either be of a certain degree of force, or of a certain degree of magnitude.

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* Robins's Remarks, p. 145. § Ib. † Eſſay on Vital and Involuntary Motions, p. 133. ‡ Smith's Opticks, Vol. 1, p. 31. || Ac. Par. 1752, H. p. 200.

For this reaſon, a ſtar which appears only as a lucid point through a teleſcope, not ſubtending ſo much as an angle of one ſecond, is viſible to the eye, though a white or black ſpot of 25 or 30 ſeconds, is not to be perceived. Alſo a line, of the ſame breadth, with the circular ſpot, will be viſible at ſuch a diſtance as the ſpot is not to be perceived at, becauſe the quantity of impreſſion from the line is greater than from the ſpot, and a longer line is viſible at a greater diſtance than a ſhorter line of the ſame breadth. He found by experiment that a ſilver wire could be ſeen when it ſubtended an angle of 3 ½ ſeconds, and a thread of ſilk when it ſubtended an angle of 2 ½ ſeconds.

This greater viſibility of a line than of a ſpot, of the ſame breadth, within the limits of perfect viſion, ſeems to ariſe only from the greater quantity of the impreſſion; but without the limits of perfect viſion, our author obſerves, that another cauſe concurs, whereby the difference of viſibility between the line and the ſpot is rendered much more conſiderable. For the impreſſion upon the retina made by the line is then not only much greater, but alſo much ſtronger than that of the ſpot; becauſe the faint image, or penumbra, of any one point of the line, when the whole is placed beyond the limits of diſtinct viſion, will fall within the faint image of the next point, and thereby much increaſe the light that comes from it.*

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* Ac. Par. 1752, H. p. 149.

In ſome caſes this accurate obſerver found the cauſe of indiſtinct viſion to be the unſteadineſs of the eye, as our being able to ſee a ſingle black line upon a white ground, or a ſingle white line upon a black ground, and not a white line between two black ones on a white ground. In viewing either of the former objects, if the eye be imperceptibly moved, all the effect will be, that the object will be painted upon a different part of the retina; but wherever it is painted, there will be but one picture, ſingle, and unconfounded with any other. But in viewing the other, if the eye fluctuate ever ſo little, the image of one or other of the black lines will be ſhifted to that part of the retina which was before poſſeſſed by the white line, and this muſt occaſion ſuch a dazzle in the eye, that the white line cannot be diſtinctly perceived, and diſtinguiſhed from the black lines; which, by a continual fluctuation, will alternately occupy the ſpace of the white line, whence muſt ariſe an appearance of one broad dark line, without any manifeſt ſeparation.

By trying this experiment with two pins, of known diameters, ſet in a window, againſt the ſky light, with a ſpace between them equal in breadth to one of the pins, he found that the diſtance between the pins could hardly be diſtinguiſhed when it ſubtended an angle leſs than 40 ſeconds, though one of the pins alone could be diſtinguiſhed under a much leſs angle. But though a ſpace between two pins cannot be diſtinguiſhed by the eye, when it ſubtends a leſs angle than 40 ſeconds, it would be a miſtake to think that the eye muſt neceſſarily commit an error of 40 ſeconds, in eſtimating the diſtance between two pins, when they are much farther from one another. For if the ſpace between them ſubtend an angle of one minute, and each of the pins ſubtend an angle of 4 ſeconds, which is greater than the leaſt angle the eye can diſtinguiſh, it is manifeſt that the eye may judge of the place of each pin within 2 ſeconds at the moſt, and conſequently the error committed in taking the angle between them cannot at the moſt exceed 4 ſeconds, provided the inſtrument be ſufficiently exact. And yet upon the like miſtake, he ſays, was founded the principal objection of Dr. Hooke againſt the accuracy of the celeſtial obſervations of Hevelius.

A black ſpot upon a white ground, or a white ſpot upon a black ground, he ſays, can hardly be perceived by the generality of eyes, when it ſubtends a leſs angle than one minute. And if two black ſpots be made upon white paper, with a ſpace between them, equal in breadth to one of their diameters, that ſpace is not to be diſtinguiſhed, even within the limits of perfect viſion, under ſo ſmall an angle as a ſingle ſpot, of the ſame ſize, can be diſtinguiſhed. To ſee the two ſpots diſtinctly, therefore, the breadth of the ſpace between them muſt ſubtend an angle of more than a minute. It would be very difficult, he ſays, to make this experiment accurately, within the limits of perfect viſion; becauſe the objects muſt be extremely ſmall; but by a rude trial, made with ſquare bits of white paper, placed upon a black ground, he judged that the leaſt angle under which the interval between the two objects could be perceived, was at leaſt a 4th part greater than the leaſt angle under which a ſingle object can be perceived. So that an eye which cannot perceive a ſingle object under a leſs angle than one minute, will not perceive the interval between two ſuch objects under a leſs angle than 75 ſeconds.

Without the limits of perfect viſion, the diſtance at which a ſingle object ceaſes to be perceivable will be much greater, in proportion, than the diſtance at which a ſpace of equal breadth between two ſuch objects ceaſes to be perceivable. For without theſe limits, the image of each of the objects will be attended with a penumbra, and the penumbræ of the two near objects will take up part of the ſpace between them, and thereby render it leſs perceivable; but the penumbra will add to the breadth of the ſingle object, and will thereby make it more perceivable, unleſs its image be very faint.*

Upon the ſame principles he likewiſe accounts for the *radiation of the ſtars,* whereby the light ſeems to project from them different ways at the ſame time.§

M. Tobias Mayer made many experiments in order to aſcertain the ſmalleſt angle of viſion in a variety of reſpects. He began with obſerving at what diſtance a black ſpot was viſible on white paper, and found that, when it could barely be diſtinguiſhed, it ſubtended an angle of about 34 minutes. When black lines were diſpoſed with intervals broader than themſelves, they were diſtinguiſhed at a greater diſtance than they could be when the objects and the intervals were of an equal breadth.

seconds?

In all theſe caſes it made no difference whether the objects were placed in the ſhade, or expoſed to the ſtrong light of the ſun; but when the degrees of light were ſmall, their differences had a conſiderable effect, though by no means in proportion to the differences of the light. For if an object was illuminated to ſuch a degree as to be juſt viſible, at the diſtance of nine feet, it would be viſible at the diſtance of four feet, though the light was diminiſhed above 160 times. It appeared, in the courſe of theſe experiments, that common day-light is, at a medium, equal to that of 25 candles, placed at the diſtance of one foot from the object.*

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* Eſſay, p. 151. § Ib. p. 159.

Another caſe of indiſtinct viſion Dr. Jurin endeavours to explain, but it muſt be owned very injudiciouſly, by Sir Iſaac Newton's hypotheſis of the fits of eaſy reflexion and tranſmiſſion. The obſervation was firſt made by M. De la Hire, but our author contrived to make the appearance much more remarkable, as follows. Take a parallel ruler, ſays he, and opening it to a ſmall aperture, hold it directly before your eye, ſo as to look at the ſky light through it; if the diſtance at which you hold it be the neareſt at which you can ſee it diſtinctly, the aperture will appear as one luminous line. But if you bring the ruler nearer to your eye, the aperture will appear double, or as two luminous lines; and as you vary the aperture, or the diſtance of the ruler from your eye, you will ſee the appearance not only of two, three, four, five, &c. luminous and dark parallel lines alternately, but of a greater number than you can count, eſpecially when you look through the aperture at the flame of a candle.

The parallel ruler, opened to a ſmall aperture, alſo affords the beſt means of trying this experiment *beyond* the bounds of diſtinct viſion, by ſetting it in the window againſt the ſky light, and looking at it through a convex glaſs, if a perſon be long ſighted, or with the naked eye, if he be ſhort ſighted. For then there will be all the ſame appearances of a multiplicity of images as before.†

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* Com. Gotting, Vol. 4, p. 97.

† Eſſay, p. 156.

Theſe caſes of a multiplicity of images of the ſame object ſeen indiſtinctly, as well as others ſimilar to them, mentioned by Dr. Jurin, p. 166, are by Mr. Robins aſcribed to ſome corrugation or inequality of ſurface, to which that part in the eye which is changed for the different diſtances of objects is ſubject, in its extream tenſion either way, whereby the image of an object, looked at out of the limits of diſtinct viſion, is multiplied. From this multiplicity of images, he ſays, that two parallels, as each is multiplied, are at firſt ſeen as four lines, or even more; but at length the two innermoſt unite into one; and in a circle, by a more multifarious appearance, a ſpot in the center is in one ſituation, produced by the union of the different images in ſome one part.*

It is true, he adds, that a narrow line of light, ſuch as the interval between the edges of a parallel ruler, brought very near together, will appear bordered with two other lines of light, when held either too near or too remote for diſtinct viſion. If Dr. Jurin's account of this experiment be juſt, he obſerves that the ſtars ought not to appear radiated, but ſurrounded with concentric circles of light, as the forementioned line of light is bordered with parallel lines. A very ſmall pin hole in a piece of paper, being held up againſt the light, firſt at a diſtance from the eye at which it may be ſeen diſtinctly, and then, while the eye is directed to it, drawn gradually nearer, will, he ſays, as ſoon as it ceaſes to be ſeen diſtinctly, appear radiated, like a ſtar, not ſurrounded with luminous circles. If the appearances in this experiment be duly attended to, they will account, he ſays, for the lines of light in the other obſervation; for theſe rays will moſt of them, if not all, have a lucid ſpeck in them, ſo that if a ſlit be cut in the paper, through the pin hole, of the breadth of the hole; each of theſe ſpecks will become a line of light, parallel to the ſlit.*

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* Robins's Tracts, Vol. 2, p. 280.

Dr. Jurin concludes his obſervations with relating what appears upon attentively viewing a light ſurface terminated by a dark one; when a light border will ſometimes appear within the dark one, and a dark border within the light one. This he aſcribes to the impreſſion made upon the eye by the light remaining ſome ſenſible time, as in looking at the ſun, &c. For, upon the leaſt unavoidable nutation of the head, ſome part of the retina, which had been occupied by the dark and faint lucid lines without the image, would now be impreſſed by the light of the white part; and this ſudden tranſition from darkneſs to light would cauſe the light upon that part of the retina to appear ſtronger than upon the other parts, which had all along received the light of the white ſurface, that is a luminous border would appear brighter than the reſt of the ſurface. And when the eye is a little diverted to one ſide, in order to conſider this luminous border more attentively, by this means a part of the retina, which was before poſſeſſed by part of the image of the black ſurface, would now be impreſſed with the light of the white ſurface, and by having undergone a greater degree of darkneſs than the former part, it would be affected with a ſtill ſtronger ſenſation of light; that is, the luminous border will not only appear broader, but ſtronger than before.†

That we ſee moſt clearly when the pupil is dilated, is evident, becauſe the eye then admits a greater quantity of light; but we ſee the moſt diſtinctly when the pupil is contracted, becauſe, as has been obſerved already, the divergence of rays, not accurately united upon the retina, is limited by the pupil;[*] and alſo, as Dr. Porterfield adds, becauſe all foreign light is thereby more effectually excluded, and the eye is a more perfect camera obſcura.[†] It may perhaps be added, that the aberration of the rays, occaſioned by the curvature of the humours of the eye, is thereby leſſened. Thus the contraction of the aperture of a teleſcope makes objects appear leſs luminous, but more diſtinct; becauſe the aberration of the rays, occaſioned by the curvature of the lens, is thereby leſſened.

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* Robins's Tracts, Vol. 2, p. 296.

† Eſſay, p. 170.

It was the opinion of Paulus Ægineta, Honoratus Faber, and Gaſſendi, and indeed of all the phyſicians for many years, that the contraction of the pupil makes objects appear leſs. But this, as Dr. Porterfield ſhews, is contrary to the evident principles of opticks,[§] and we may add, it is contrary to what we experience in the uſe of teleſcopes, the magnifying power of which is not affected by leſſening their apertures.

Several circumſtances relating to diſtinct and indiſtinct viſion, are well explained by H. J. De la Motte, a phyſician in Dantzick, in conſequence of endeavouring to verify an experiment of Scheiner mentioned above, in which a diſtant object appeared multiplied when it was ſeen through ſeveral holes, made with the point of a pin in a card, not farther diſtant from one another than the diameter of the pupil; but, notwithſtanding all his labour, he was not able to ſucceed, till a friend of his happening to call upon him, he deſired him to make the trial; when it anſwered perfectly. His friend was ſhort ſighted, whereas he himſelf was not. Giving his friend a concave glaſs, and placing it cloſe to the card, the object appeared but one. Theſe circumſtances enabled Dr. Motte to ſet the whole affair in a very clear point of light.

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[*] This however is only the caſe with eyes that are bad and which have not a power of conforming themſelves to the diſtinct view of objects placed at different diſtances.

[†] Porterfield on the Eye, Vol. 2, p. 176. [§] Ib. p. 178.

When the object is at ſuch a diſtance, and the eye of ſuch a form, that, without any ſtraining, pencils of rays coming from it would meet upon the retina, the object will never appear more than one, through whatever number of holes they may be tranſmitted to the pupil. The parts of the card that intercept the light, in this caſe, will only thin the pencils, and conſequently make the object leſs luminous. But if the eye be obliged to alter its figure, in order to ſee the object diſtinctly, the interpoſition of the card, by ſuggeſting a different idea or ſenſation of the diſtance, reſtores the eye to its natural conformation; and then the pencils of rays that are tranſmitted through the ſeveral holes, either do not come to a focus upon the retina, or elſe before they reach it, in both which caſes there will be as many different impreſſions upon it as there are holes in the card; with this difference, that, if they tend to a focus beyond the retina, the covering of a hole on the right hand will conceal an image that appeared to the right; whereas, if they come to a focus before they reach the retina, the covering of a hole to the right hand will conceal an image that appeared to the left, on account of the rays having croſſed one another before they reached the retina. This is ſo exceedingly obvious, that I do not think it neceſſary to explain it by any figure. Thoſe who chuſe to ſee it done, in a great variety of caſes, may conſult the Memoir from which this account is extracted.*

CHAPTER VI.

Observations on the apparent PLACE, DISTANCE, MAGNITUDE and MOTION of objects.

MY reader will certainly wiſh to ſee in what manner Dr. Barrow's hypotheſis, concerning the *apparent place* of objects, and the difficulty which he ſtarted with reſpect to it, were taken up by other philoſophers.

The perſon who firſt took much notice of it was the ingenious Dr. Berkley, Biſhop of Cloyne, who diſtinguiſhed himſelf ſo much by the objections which he ſtarted to the reality of a material world, and by his oppoſition to the Newtonian doctrine of fluxions. In his *Eſſay on a new theory of viſion,* he obſerves that the circle formed upon the retina, by the rays which do not come to a focus, produce the ſame confuſion in the eye, whether they croſs one another before they reach the retina, or tend to do it afterwards; and therefore that the judgment concerning diſtance will be the ſame in both the caſes, without any regard to the place from which the rays originally iſſued; ſo that in this caſe, as, by receding from the lens, the confuſion, which always accompanies the nearneſs of an object, increaſes, the mind will judge that the object comes nearer.*

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* Verſuche et Abhandlungen, &c. Dantzig, Vol. 2, p. 209.

But, ſays Dr. Smith, if this be true, the object ought always to appear at a leſs diſtance from the eye than that at which objects are ſeen diſtinctly, which is not the caſe; and to explain this appearance, as well as every other in which a judgment is formed concerning diſtance, he maintains that we judge of it by the apparent magnitude of objects only, or chiefly, ſo that, ſince the image grows larger as we recede from the lens, through which it is viewed, we conceive the object to come nearer. He alſo endeavours to ſhew that, in all caſes in which glaſſes are uſed, we judge of diſtance by the ſame ſimple rule; from which he concludes univerſally, that the apparent diſtance of an object ſeen in a glaſs, is to its apparent diſtance ſeen by the naked eye, as the apparent magnitude to the naked eye is to its apparent magnitude in the glaſs. §

But that we do not judge of diſtance merely by the angle under which objects are ſeen, is an obſervation as old as Alhazen, who mentions ſeveral inſtances, in which, though the angles under which objects appear be different, the magnitudes are univerſally and inſtantaneouſly deemed not to be ſo.‡ And Mr. Robins clearly ſhews the hypotheſis of Dr. Smith, to be contrary to fact in the moſt common and ſimple caſes. In microſcopes, he ſays, it is impoſſible that the eye ſhould judge the object to be nearer than the diſtance at which it has viewed the object itſelf, in proportion to the degree of magnifying. For when the microſcope magnifies much, this rule would place the image at a diſtance, of which the ſight cannot poſſibly form any opinion, as being an interval from the eye at which no object can be ſeen. In general, he ſays, he believes that whoever looks at an object through a convex glaſs, and then at the object itſelf, without the glaſs, will find it to appear nearer in the latter caſe, though it be magnified in the glaſs; and in the ſame trial with the concave glaſs, though by the glaſs the object be diminiſhed, it will appear nearer through the glaſs than without it.

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* Eſſay on Viſion, p. 30. § Opticks, Vol. 1, p. 52. ‡ Robins's Tracts, Vol. 2, p. 247. Alhazen Lib. [illegible], N. 36.

But the moſt convincing proof that the apparent diſtance of the image is not determined by its apparent magnitude is the following experiment. If a double convex glaſs be held upright before ſome luminous object, as a candle, there will be ſeen two images, one erect, and the other inverted. The firſt is made ſimply by reflexion from the neareſt ſurface, the ſecond by reflexion from the farther ſurface, the rays undergoing a refraction from the firſt ſurface both before and after the reflexion. If this glaſs has not too ſhort a focal diſtance, when it is held near the object, the inverted image will appear larger than the other, and alſo nearer; but if the glaſs be carried off from the object, though the eye remain as near to it as before, the inverted image will diminiſh ſo much faſter than the other, that, at length, it will appear very much leſs than it, but ſtill nearer. Here, ſays Mr. Robins, two images of the ſame object are ſeen under one view, and their apparent diſtances immediately compared; and here it is evident that thoſe diſtances have no neceſſary connection with the apparent magnitude. He alſo ſhews how this experiment may be made ſtill more convincing, by ſticking a piece of paper on the middle of the lens, and viewing it through a ſhort tube.*

As I propoſe to dwell as little as poſſible upon fallacious principles in philoſophy, I omit a variety of arguments that Dr. Smith produces for his opinion, and alſo the refutation of them by Mr. Robins. What perplexed Dr. Smith, Mr. Robins conjectures to have been the conſideration, that the apparent magnitude of very diſtant objects is neither determined by the magnitude of the angle only under which they are ſeen, nor is the exact proportion of that angle compared with their true diſtance, but is compounded alſo with a deception concerning that diſtance; in ſo much that, if we had no idea of difference in the diſtance of objects, each would appear in magnitude proportional to the angle under which it was ſeen; and if our apprehenſion of the diſtance was always juſt, our idea of their magnitude would be, in all diſtances, unvaried; but in proportion as we err in our conception of their diſtance, the greater angle ſuggeſts a greater magnitude. Dr. Smith not being appriſed of this compound effect, has ſometimes explained the apparent magnitude by the viſual angle only, at other times by that angle compared with the diſtance; and by that means, ſays Mr. Robins, he becomes inconſiſtent with himſelf.†

Montucla alſo, who ſeems not to have been acquainted with Mr. Robins's writings, declares himſelf diſſatisfied with Dr. Smith's hypotheſis concerning apparent diſtance, and makes ſome remarks upon the ſubject, which are not unworthy of our notice.

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* Robins's Tracts, Vol. 2, p. 230. † Ib. p. 251.

He thinks that, contrary to what is advanced by Dr. Smith, and other Opticians, he can demonſtrate, what we have ſeen to have been aſſerted by Mr. Robins, that when an object is ſeen through a convex lens, it is imagined to be farther off than when it is ſeen with the naked eye. Let a convex lens, he ſays, be placed upon a written paper, or any other object, and if it be removed to a greater diſtance, it will appear to go farther off, in proportion as it grows bigger. If this do not convince them, he thinks the following experiment, by which he brought many perſons to think as he did upon the ſubject, cannot fail to give them ſatisfaction. He bade them look downwards, through a convex lens, upon the edge of a table, and then endeavour to touch it with their finger; and there was no perſon who did not reach too low for that purpoſe, whereas they would not have reached low enough, if they had imagined it to be nearer than before.

He alſo thought that he could obſerve, in the experiment of Dr. Barrow, a circumſtance that overthrew the hypotheſis of Dr. Smith. For, according to him, when a perpendicular line, plunged into a veſſel of water, is viewed obliquely, all its parts appear diminiſhed, in proportion to its depth; ſo that they ought to appear beyond the perpendicular; whereas, according to Dr. Barrow, the appearance of the line within the water is a curve, which falls on this ſide the perpendicular. Upon the whole, Montucla concludes that there is ſomething which he cannot account for in the experiment of Dr. Barrow, and invites philoſophers to give more attention to the ſubject.*

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* Montucla, Vol. 2, p. 602.

Had our author written a little later, he would have ſeen ſome new light thrown upon the ſubject by M. Bouguer, who adopts the general maxim of Dr. Barrow, in ſuppoſing that we refer objects to the place from which the pencils of rays ſeemingly converge at their entrance into the pupil. But when rays iſſue from below the ſurface of a veſſel of water, or any other refracting medium, he finds that there are always two different places of this ſeeming convergence; one of them of the rays that iſſue from it in the ſame vertical circle, and therefore fall with different degrees of obliquity upon the ſurface of the refracting medium, and another of thoſe that fall upon the ſurface with the ſame degree of obliquity, entering the eye laterally with reſpect to one another. Sometimes, he ſays, that one of theſe images is attended to by the mind, and ſometimes the other; and different images may be obſerved by different perſons. An object plunged in water affords an example he ſays of this duplicity of images.

If BA *b*, fig. 155, be part of the ſurface of water, and the object be at O, there will be two images of it, in two different places; one at G, on the cauſtic by refraction, and the other at E, in the perpendicular AO, which is as much a cauſtic as the other line. The former image is viſible by the rays ODM, O *d m*, which are one higher than the other, in their progreſs to the eye; whereas the image at E is made by the rays ODM,O *ef*, which enter the eye laterally. This, ſays he, may ſerve to explain the difficulty of Father Tacquet, Barrow, Smith, and many other authors, and which Newton himſelf conſidered as a very difficult problem, though it might not be abſolutely inſoluble.*

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* Traité d' Optique, p. 104.

G. W. Krafft has ably ſupported the opinion of Dr. Barrow, that the place of any point, ſeen by reflexion from the ſurface of any medium, is that in which rays iſſuing from it, infinitely near to one another, would meet;* and conſidering the caſe of a diſtant object, viewed in a concave mirror, by an eye very near to it, when the image, according to Euclid and other writers, would be between the eye and the object, and the rule of Dr. Barrow cannot be applied, he ſays that, in this caſe, the ſpeculum may be conſidered as a plane, the effect, being the ſame, only the image is more obſcure. §

Dr. Porterfield gives a diſtinct and comprehenſive view of the natural methods of judging concerning the diſtance of objects, adding, to the five methods which have been recited from M. De la Hire, the conformation of each eye, which has been treated of above. As he makes ſeveral pertinent remarks upon each of them, and on the fallacies that we are liable to when any of them fail us, I ſhall briefly recite ſome of his obſervations.

The conformation of the eye, he obſerves, can be of no uſe to us with reſpect to objects that are placed without the limits of diſtinct viſion. As the object, however, does then appear more or leſs confuſed, according as it is more or leſs removed from thoſe limits, this confuſion aſſiſts the mind in judging of the diſtance of the object; it being always eſteemed ſo much the nearer, or the farther off, by how much the confuſion is greater. But this confuſion hath its limits alſo, beyond which it can never extend; for when an object is placed at a certain diſtance from the eye, to which the breadth of the pupil bears no ſenſible proportion, the rays of light that come from a point in the object, and paſs the pupil, are ſo little diverging, that they may be conſidered as parallel. For a picture on the retina will not be ſenſibly more confuſed, though the object be removed to a much greater diſtance.*

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* Com. Petrop. Vol. 12, p. 252.

§ Ib. p. 256.

The moſt univerſal, and frequently the moſt ſure means of judging of the diſtance of objects is, he ſays, the angle made by the optic axes. For our two eyes are like two different ſtations, by the aſſiſtance of which diſtances are taken; and this is the reaſon why thoſe perſons who are blind of one eye, ſo frequently miſs their mark in pouring liquor into a glaſs, ſnuffing a candle, and ſuch other actions as require that the diſtance be exactly diſtinguiſhed. To convince ourſelves of the uſefulneſs of this method of judging of the diſtance of objects, he directs us to ſuſpend a ring in a thread, ſo that its ſide may be towards us, and the hole in it to the right and left hand; and taking a ſmall rod, crooked at the end, retire from the ring two or three paces, and having with one hand covered one of our eyes, to endeavour with the other to paſs the crooked end of the rod through the ring. This, ſays he, appears very eaſy, and yet, upon trial, perhaps once in 100 times, we ſhall not ſucceed, eſpecially if we move the rod a little quickly.§

Our author obſerves, that by perſons recollecting the time when they began to be ſubject to the miſtakes above mentioned, they may tell when it was that they loſt the uſe of one of their eyes; which many perſons are long ignorant of, and which may be a circumſtance of ſome conſequence to a phyſician. Of this he once availed himſelf in curing a gutta ſerena, which he found, by this means, to have been of no long ſtanding.* The uſe of this ſecond method of judging of diſtances De Chales limited to 120 feet, beyond which, he ſays, we are not ſenſible of any difference in the angle of the optic axes.†

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* Vol. 2, p. 387. § Vol. 1, p. 105.

A third method of judging of the diſtance of objects, conſiſts in their apparent magnitudes, on which ſo much ſtreſs was laid by Dr. Smith. From this change in the magnitude of the image upon the retina, we eaſily judge of the diſtance of objects, as often as we are otherwiſe acquainted with the magnitude of the objects themſelves; but as often as we are ignorant of the real magnitude of bodies, we can never, from their apparent magnitude, form any judgment of their diſtance.

From this we may ſee why we are ſo frequently deceived in our eſtimates of diſtance, by any extraordinary magnitudes of objects ſeen at the end of it; as, in travelling towards a large city, or a caſtle, or a cathedral church, or a mountain larger than ordinary, we fancy them to be nearer than we find them to be. This alſo is the reaſon why animals, and all ſmall objects, ſeen in valleys, contiguous to large mountains, appear exceedingly ſmall. For we think the mountain nearer to us than if it was ſmaller; and we ſhould not be ſurpriſed at the ſmallneſs of the neighbouring animals, if we thought them farther off. For the ſame reaſon, we think them exceedingly ſmall when they are placed upon the top of a mountain, or a large building, which appears nearer to us than they really are, on account of their extraordinary ſize.§

Dr. Jurin clearly accounts for our imagining objects, when ſeen from a high building, to be ſmaller than they are,

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[*] Vol. 2, p. 390. [†] Ib. p. 394. [§] Ib. p. 396.

and ſmaller than we fancy them to be when we view them at the ſame diſtance on level ground. It is, ſays he, becauſe we have no diſtinct idea of diſtance in that direction, and therefore judge of things by their pictures upon the eye only, but cuſtom will enable us to judge rightly even in this caſe.

Let a boy, ſays he, who has never been upon any high building, go to the top of the Monument, and look down into the ſtreet, the objects ſeen there, as men and horſes, will appear ſo ſmall as greatly to ſurpriſe him. But 10 or 20 years after, if, in the mean time, he has uſed himſelf, now and then, to look down from that, and other great heights, he will no longer find the ſame objects to appear ſo ſmall. And if he was to view the ſame objects from ſuch heights as frequently as he ſees them upon the ſame level with himſelf in the ſtreets, he ſuppoſes that they would appear to him juſt of the ſame magnitude from the top of the Monument, as they do from a window one ſtory high. For this reaſon it is that ſtatues placed upon very high buildings ought to be made of a larger ſize than thoſe which are ſeen at a nearer diſtance; becauſe all perſons, except architects, are apt to imagine the height of ſuch buildings to be much leſs than it really is.*

The fourth method by which Dr. Porterfield ſays, that we judge of the diſtance of objects, is, the force with which their colour ſtrikes upon our eyes. For if we be aſſured that two objects are of a ſimilar and like colour, and that one appears more bright and lively than the other, we judge that the brighter object is the nearer of the two.§

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* Smith's Opticks, Remarks, p. 51.

§ Vol. 2, p. 396.

The fifth method conſiſts in the different appearance of the ſmall parts of objects. When theſe parts appear diſtinct, we judge that the object is near, but when they appear confuſed, or when they do not appear at all, we judge that it is at a greater diſtance. For the image of any object, or part of an object, diminiſhes as the diſtance of it increaſes.

The ſixth, and laſt method, by which we judge of the diſtance of objects, is that the eye does not repreſent to our mind one object alone, but at the ſame time all thoſe that are placed betwixt us and the principal object, whoſe diſtance we are conſidering; and the more this diſtance is divided into ſeparate and diſtinct parts, the greater it appears to be. For this reaſon, diſtances upon uneven ſurfaces appear leſs than upon a plane; for the inequalities of the ſurfaces, ſuch as hills, and holes, and rivers, that lie low and out of ſight, either do not appear, or hinder the parts that lie behind them from appearing; and ſo the whole apparent diſtance is diminiſhed by the parts that do not appear in it. This is the reaſon that the banks of a river appear contiguous to a diſtant eye, when the river is low and not ſeen.*

Dr. Porterfield very well explains ſeveral fallacies in viſion depending upon our miſtaking the diſtances of objects. Of this kind, he ſays, is the appearance of parallel lines, and long viſta's, conſiſting of parallel rows of trees; for they ſeem to converge more and more, as they are farther extended from the eye. The reaſon of this, he ſays, is becauſe the apparent magnitudes of their perpendicular intervals are perpetually diminiſhing, while, at the ſame time, we miſtake their diſtance. Hence we may ſee why, when the two parallel rows of trees ſtand upon an aſcent, whereby the more remote parts appear farther off than they really are, becauſe the line that meaſures the length of the viſtas now appears under a greater angle than when it was horizontal, the trees, in ſuch a caſe, will ſeem to converge leſs, and ſometimes, inſtead of converging, they will be thought to diverge.

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* Vol. 2, p. 408.

For the ſame reaſon that a long viſta appears to converge more and more, the farther it is extended from the eye, the remoter parts of a horizontal walk, or a long floor, will appear to aſcend gradually; and objects placed upon it, the more remote they are, the higher they will appear, till the laſt be ſeen on a level with the eye; whereas the cieling of a long gallery appears to deſcend towards a horizontal line, drawn from the eye of the ſpectator. For this reaſon, alſo, the ſurface of the ſea, ſeen from an eminence, ſeems to riſe higher and higher the farther we look; and the upper parts of high buildings ſeem to ſtoop, or incline forwards over the eye below, becauſe they ſeem to approach towards a vertical line proceeding from the ſpectator's eye; ſo that ſtatues on the top of ſuch buildings, in order to appear upright, muſt recline, or bend backwards.*

Our author alſo ſhews the reaſon why a windmill, ſeen from a great diſtance, is ſometimes imagined to move the contrary way from what it really does, by our taking the nearer end of the ſail for the more remote. The uncertainty we ſometimes find in the courſe of the motion of a branch of lighted candles, turned round at a diſtance, is owing, he ſays, to the ſame cauſe, as alſo our ſometimes miſtaking a convex for a concave ſurface, more eſpecially in viewing ſeals, and impreſſions with a convex glaſs, or a double microſcope; and laſtly that, upon coming in a dark night into a ſtreet, in which there is but one row of lamps, we often miſtake the ſide of the ſtreet they are on.*

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* Vol. 2, p. 383.

Far more light was thrown upon this curious ſubject by M. Bouguer, whoſe name my reader will be glad to meet with again in the courſe of this hiſtory, and I wiſh it had been in my power to give him that pleaſure much oftener.

The proper method of drawing the appearance of two rows of trees that ſhall appear parallel to the eye, is a problem which has exerciſed the ingenuity of ſeveral philoſophers and mathematicians. That the apparent magnitude of objects decreaſes with the angle under which they are ſeen, has always been acknowledged. It is alſo acknowledged that it is only by cuſtom and experience that we learn to form a judgment both of magnitudes and diſtances; but in the application of theſe maxims to the above mentioned problem, all perſons, before M. Bouguer, made uſe of the *real* diſtance inſtead of the *apparent* one; by which only the mind can form its judgment. And it is manifeſt that, if any circumſtances contribute to make the diſtance appear otherwiſe than it is in reality, the apparent magnitude of the object will be affected by it, for the ſame reaſon that, if the magnitude be miſapprehended, the idea of the diſtance will vary.

For want of attending to this diſtinction, Tacquet pretended to demonſtrate, that nothing can give the idea of two parallel lines (rows of trees for inſtance) to an eye ſituated at one of their extremities, but two hyperbolical curves, turned the contrary way; and M. Varignon maintained that, in order to make a viſta appear of the ſame width, it muſt be made narrower, inſtead of wider, as it recedes from the eye.

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* Vol. 2, p. 384.

M. Bouguer obſerves that very great diſtances, and thoſe that are conſiderably leſs than them, make nearly the ſame impreſſion upon the eye. We, therefore, always imagine great diſtances to be leſs than they are, and for this reaſon the ground plan of a long viſta always appears to riſe. The viſual rays come in a determinate direction, but as we imagine that they terminate ſooner than they do, we neceſſarily conceive that the place from which they iſſue is elevated. Every large plane, therefore, as AB, fig. 156, viewed by an eye at O, will ſeem to lie in ſuch a direction as A *b*, and conſequently lines, in order to appear truly parallel, on the plane AB, muſt be drawn ſo as that they would appear parallel on the plane A *b*, and be from thence projected to the plane AB.*

To determine the inclination of the apparent ground plane A *b* to the true ground plane AB, our ingenious author directs us to draw upon a piece of level ground, two ſtraight lines of a ſufficient length (for which purpoſe lines faſtened to ſmall ſticks are very convenient) making an angle of 3 or 4 degrees with one another. Then a perſon, placing himſelf within the angle, with his back towards the angular point, muſt walk backwards and forwards till he can fancy the lines to be parallel. In this ſituation, a line drawn from the point of the angle through the place of his eye, will contain the ſame angle with the true ground plane which this does with the apparent one.

M. Bouguer then ſhews other more geometrical methods of determining this inclination, and ſays that, by theſe means he has often found it to be 4 or 5 degrees, though ſometimes only 2 or 2½ degrees. The determination of this angle, he obſerves, is variable, depending upon the manner in which the ground is illuminated, and the intenſity of the light. The colour of the ſoil is alſo not without its influence, as well as the particular conformation of the eye, by which it is more or leſs affected by the ſame degree of light, and alſo the part of the eye on which the object is painted. When, by a ſlight motion of his head, he contrived that certain parts of the ſoil, the image of which fell towards the bottom of his eye, ſhould fall towards the top of the retina, he always thought that this apparent inclination became a little greater.

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* Ac. Par. 1755, M. 156.

But what is very remarkable, and what, he ſays, he can aſſure his reader may be depended upon, is that, if he look towards a riſing ground, the difference between the apparent ground plan, and the true one, will be much more conſiderable; ſo that they will ſometimes make an angle of 25 or 30 degrees. Of this he had made frequent obſervations. Mountains, he ſays, begin to be inacceſſible when their ſides made an angle from 35 to 37 degrees with the horizon, as then it is not poſſible to climb them but by means of ſtones or ſhrubs, to ſerve as ſteps to fix the feet on. In theſe caſes, both he and his companions always agreed that the apparent inclination of the ſide of the mountain was 60 or 70 degrees.

Theſe deceptions are repreſented in fig. 157, in which, when the ground plan, AM, or AN, are much inclined, the apparent ground plan A*m*, or A*n* makes a very large angle with it. On the contrary, if the ground dips below the level, the inclination of the apparent to the true ground plan diminiſhes, till, at a certain degree of the ſlope, it becomes nothing at all; the two plans AP and A*p* being the ſame, ſo that parallel lines drawn upon them would always appear ſo. If the inclination below the horizon is carried beyond the ſituation AP, the error will increaſe; and what is very remarkable, it will be on the contrary ſide; the apparent plan A r being always below the true plan AR, ſo that, if a perſon would draw upon the plan AR lines that ſhall appear parallel to the eye, they muſt be drawn converging, and not diverging, as is uſual on the level ground, becauſe they muſt be the projections of two lines imagined to be parallel, on the plan A *r*, which is more inclined to the horizon than AR.*

Theſe remarks, he obſerves, are applicable to different planes expoſed to the eye at the ſame time. For if BH, fig. 158 be the front of a building, at the diſtance of AB from the eye, it will be reduced in appearance to the diſtance A *b*; and the front of the building will be *bh*, rather inclined towards the ſpectator, unleſs the diſtance be inconſiderable.

After making a great number of obſervations upon this ſubject, our author concludes, that when a man ſtands upon a level plane, it does not ſeem to riſe ſenſibly but at ſome diſtance from him. The apparent plane, therefore, has a curvature in it, at that diſtance, the form of which is not very eaſy to determine; ſo that a man ſtanding upon a level plane, of infinite extent, will imagine that he ſtands in the center of a baſon. This is alſo, in ſome meaſure, the caſe with a perſon ſtanding upon the level of the ſea.

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* Vol. 2, p. 160.

He concludes with obſerving, that there is no difficulty in drawing lines according to theſe rules, ſo as to have any given effect upon the eye, except when ſome parts of the proſpect are very near the ſpectator, and others very diſtant from him; becauſe, in this caſe, regard muſt be had to the conical or conoidal figure of a ſurface. A right line paſſing at a ſmall diſtance from the obſerver, and below the level of his eye, in that caſe almoſt always appears ſenſibly curved at a certain diſtance from the eye; and almoſt all figures, in this caſe, are ſubject to ſome complicated optical alteration, to which the rules of perſpective have not as yet been extended. If a circle be drawn near our feet, and within that part of the ground which always appears level to us, it will always appear to be a circle, and, at a very conſiderable diſtance, it will appear an ellipſe; but between thoſe two ſituations, it will not appear to be either the one or the other, but will be like one of thoſe ovals of Deſcartes, which is more curved on one of its ſides than the other.*

On theſe principles a parterre, which appears diſtorted when it is ſeen in a low ſituation, appears perfectly regular when it is viewed from a balcony, or any other eminence. Still, however, the apparent irregularity takes place at a greater diſtance, while the part that is near the ſpectator is exempt from it.† If AB, fig. 159, be the ground plane, and A *a* be a perpendicular, under the eye, the higher it is ſituated, at O, to the greater diſtance will T, the place at which the plane begins to have an apparent aſcent along T *b*, be removed.

All the varieties that can occur with reſpect to the viſible *motion* of objects, are ſo ſuccinctly ſummed up by Dr. Porterfield, under eleven heads, that I cannot do better than preſent my reader with them.

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* Vol. 2, p. 166. † Ib. p. 164.

An object moving very ſwiftly is not ſeen, unleſs it be very luminous. Thus a cannon ball is not ſeen if it is viewed tranſverſely ; but if it be viewed according to the line it deſcribes, it may be ſeen, becauſe its picture continues long on the ſame place of the retina ; which, therefore, receives a more ſenſible impreſſion from the object.

2. A live coal ſwung briſkly round in a circle appears a continued circle of fire, becauſe the impreſſions made on the retina by light, being of a vibrating and conſequently of a laſting nature, do not preſently periſh, but continue till the coal performs its whole circuit, and returns again to its former place.

3. If two objects, unequally diſtant from the eye, move with equal velocity, the more remote one will appear the ſlower ; or, if their celerities be proportional to their diſtances, they will appear equally ſwift.

4. If two objects, unequally diſtant from the eye, move with unequal velocities, in the ſame direction, their apparent velocities are in a ratio compounded of the direct ratio of their true velocities, and the reciprocal one of their diſtances from the eye.

5. A viſible object moving with any velocity appears to be at reſt, if the ſpace deſcribed in the interval of one ſecond be imperceptible at the diſtance of the eye. Hence it is that a near object moving very ſlowly, as the index of a clock, or a remote one very ſwiftly, as a planet, ſeems to be at reſt.

6. An object moving with any degree of velocity will appear at reſt, if the ſpace it runs over in a ſecond of time be to its diſtance from the eye as 1 to 1400.

7. The eye proceeding ſtraight from one place to another, a lateral object, not too far off, whether on the right or left, will ſeem to move the contrary way.

8. The eye proceeding ſtraight from one place to another, and being ſenſible of its motion, diſtant objects will ſeem to move the ſame way, and with the ſame velocity. Thus, to a perſon running eaſtwards, the moon on his right hand appears to move the ſame way, and with equal ſwiftneſs, for by reaſon of its diſtance, its image continues fixed upon the ſame place of the retina, from whence we imagine that the object moves along with the eye.

9. If the eye and the object move both the ſame way, only the eye much ſwifter than the object, the laſt will appear to go backwards.

10. If two or more objects move with the ſame velocity, and a third remain at reſt, the moveable ones will appear fixed, and the quieſcent in motion, the contrary way. Thus clouds moving very ſwiftly, their parts ſeem to preſerve their ſituation, and the moon to move the contrary way.

11. If the eye be moved with great velocity, lateral objects at reſt appear to move the contrary way. Thus to a perſon ſitting in a coach, and riding briſkly through a wood, the trees ſeem to retire the contrary way; and to people in a ſhip, &c. the ſhores ſeem to recede.*

At the concluſion of theſe obſervations, our author endeavours to explain another phenomenon of motion, which, though very common and well known, had not, as far as he knew, been explained in a ſatisfactory manner. It is this. If a perſon turns ſwiftly round, without changing his place, all objects about will ſeem to move round in a circle the contrary way; and this deception continues not only while the perſon himſelf moves round, but which is more ſurpriſing, it alſo continues for ſometime after he ceaſes to move, when the eye as well as the object is at abſolute reſt.

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* Porterfield on the Eye, Vol. 2, p. 422.

The reaſon why objects appear to move round the contrary way, when the eye turns round, is not ſo difficult to explain; for though, properly ſpeaking, motion is not ſeen, as not being in itſelf the immediate object of ſight, yet by the ſight we eaſily know when the image changes its place on the retina, and thence conclude that either the object, the eye, or both are moved. But by the ſight alone we can never determine how far this motion belongs to the object, how far to the eye, or how far to both. If we imagine the eye at reſt, we aſcribe the whole motion to the object, though it be truly at reſt. If we imagine the object at reſt, we aſcribe the whole motion to the eye, though it belongs intirely to the object; and when the eye is in motion, though we are ſenſible of its motion, yet, if we do not imagine that it moves ſo ſwiftly as it really does, we aſcribe only a part of the motion to the eye, and the reſt of it we aſcribe to the object, though it be truly at reſt. This laſt, he ſays, is what happens in the preſent caſe, when the eye turns round; for though we are ſenſible of the motion of the eye, yet we do not apprehend that it moves ſo faſt as it really does; and therefore the bodies about appear to move the contrary way, as is agreeable to experience.

But the great difficulty ſtill remains, viz. Why, after the eye ceaſes to move, objects ſhould, for ſometime, ſtill appear to continue in motion, though their pictures on the retina be truly at reſt, and do not at all change their place. This, he imagined, proceeds from a miſtake we are in with reſpect to the eye, which, though it be abſolutely at reſt, we nevertheleſs conceive it as moving the contrary way to that in which it moved before; from which miſtake, with reſpect to the motion of the eye, the objects at reſt will appear to move the ſame way which the eye is imagined to move; and conſequently, will ſeem to continue their motion for ſome time after the eye is at reſt.*

CHAPTER VII.

OBSERVATIONS ON THE HORIZONTAL MOON.

THE obſervations of the *antients*, on the ſubject of the *horizontal moon*, have been recited already, and we have ſeen that a good foundation was laid by them for an explanation of this remarkable phenomenon. Much more light, however, has been thrown upon this ſubject in later times, and eſpecially in this laſt period of my hiſtory, ſo that now nothing ſeems to be wanting to the complete illuſtration of it.

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* Vol. 2, p. 424.

It was the opinion of Alhazen, and moſt of the antients, that the reaſon why the ſun and moon appear ſo extraordinarily large near the horizon, is that they are conceived to be much farther from us than when they are near the meridian, and that the reaſon why the ſky near the horizon is imagined to be ſo far from us is the diviſion of the ſpace interpoſed betwixt the horizon and the eye, by a great number of terreſtrial objects. Some farther improvement was made in this explanation by Mr. Hobbes, though he fell into ſome miſtakes in his application of geometry to this ſubject. For he obſerves that this deception operates gradually, from the very zenith to the horizon; and that if the apparent arch of the ſky be divided into any number of equal parts, thoſe parts, in deſcending towards the horizon, will gradually ſubtend a leſs and leſs angle.* And he was the firſt who expreſsly conſidered the vaulted appearance of the ſky, as a real portion of a circle.§

One would wonder that ſo rational an hypotheſis as this ſhould ever loſe its credit, eſpecially after being maintained by writers of reputation, and for a great number of years. But it was generally imagined that the actual perception of thoſe objects which divide the ſpace that is interpoſed betwixt the eye and the horizon was neceſſary, in order to its ſuggeſting the idea of its extraordinary diſtance, and this general miſconception afforded a handle for ſeveral objections. Thus Father Gouye, thinking to overturn this opinion, obſerves that the horizontal moon appears equally large acroſs the ſea, where there are no objects to produce the effect aſcribed to them;‡ and Mr. Molyneux ſays, that if the commonly received hypotheſis be true, we may at any time increaſe the apparent magnitude of the moon, even in the meridian; for, in order to divide the ſpace betwixt it and the eye, we need only to look at it from behind a cluſter of chimnies, the ridge of a hill, or the top of a houſe. He alſo makes the ſame obſervation with Father Gouye, above mentioned, and farther obſerves, that when the ſight of all the intermediate objects is cut off, by looking through a tube, the imagination is not helped, the moon being ſtill conceived to be as large as before. He then endeavours to refute the opinions of Hobbes, and Gaſſendi upon the ſubject; but he concludes without advancing any hypotheſis of his own.*

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* Robins's Tracts, Vol. 2, p. 241. § Ib. p. 244. ‡ Ac. Par. 1700, p. 11.

Dr. Deſaguliers very well illuſtrated the doctrine of the horizontal moon, upon the ſuppoſition of our imagining the viſible heavens to be only a ſmall portion of a ſpherical ſurface, and conſequently ſuppoſing the moon to be farther from us in the horizon than near the zenith; and by ſeveral ingenious contrivances he demonſtrated how liable we are to ſuch deceptions. In the preſence of ſeveral ſpectators, he placed two equal candles, at a conſiderable diſtance from one another; which, as the diſtance was diſtinctly perceived, continued to appear equal to them all; but taking off their attention for a little time, he removed the farther candle; and, in the ſame line in which it had appeared, ſubſtituted another, much leſs than it, but nearer; when the ſpectators, being informed that the candle was changed, but taking it for granted that the new one was put in the ſame place, always pronounced that it was as large as the other. He made the ſame experiment with equal ſucceſs when he made uſe of ivory balls inſtead of candles.§

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* Phil. Tranſ. ab. Vol. 1, p. 221.

§ Ib. Vol. 8, p. 130,

But the writer to whom we are obliged for the moſt complete illuſtration of this curious ſubject, is Dr. Smith, who, without ſeeming to imagine that any juſt ideas on the ſubject had occurred to any perſon before himſelf, took it up as from the beginning; particularly refuting the hypotheſis of Biſhop Berkley, who ſuppoſed that the moon appears larger near the horizon, becauſe her appearance is then fainter, and her beams affect the eye leſs. As Dr. Smith's account of this phenomenon is very full and ſatisfactory, and he has added ſeveral ingenious illuſtrations upon the ſubject, I ſhall preſent my reader with a pretty large account of it.

If the ſurface of the earth were perfectly plane, he ſays that the diſtance of the viſible horizon from the eye would ſcarce exceed 5000 times the heighth of the eye above the ground, or the diſtance of five miles (ſuppoſing the height of the eye to be between five and ſix feet) and all objects placed beyond that diſtance would appear in the viſible horizon. All objects and clouds, likewiſe, placed at any diſtance beyond this muſt, conſequently, if they be viſible at all, appear to be in the horizon. Hence, he ſays, if we ſuppoſe a vaſt wall to be built at the extremity of the plane, beyond the point of viſible diſtance, it will not appear ſtraight, but circular, as if built upon the circumference of the horizon; and, if continued infinitely, would make a perfect ſemicircle. If now this round plane, with the wall upon it, be imagined to be raiſed, till it come perpendicular to the reſt of the plane, on which a perſon ſtands, the wall will appear like the concave figure of the clouds over his head. But though the wall in the horizon appear in the ſhape of a ſemicircle, yet the cieling will not, but much flatter; becauſe the horizontal plane was a viſible ſurface, which ſuggeſted the idea of the ſame diſtances quite round the eye, but in the vertical plane, extended between the eye and the cieling, there is nothing that affects the ſenſe with an idea of its parts. Conſequently, the apparent diſtances of the higher parts of the cieling will be gradually diminiſhed. Now when the ſky is quite over caſt with clouds of equal gravities, they will all float in the air at equal heights above the earth, and conſequently will compoſe a ſurface reſembling a large cieling, as flat as the viſible ſurface of the earth. Its concavity, therefore, is not real, but apparent; and when the heights of the clouds are unequal, ſince their real ſhapes and magnitudes are all unknown, the eye can ſeldom diſtinguiſh the unequal diſtances of thoſe clouds which appear in the ſame directions, unleſs when they are very near us, or are driven by contrary currents of the air. So that the viſible ſhape of the whole ſurface remains alike in both caſes. And when the ſky is either partly overcaſt, or perfectly free from clouds, it is a fact that we retain much the ſame idea of its concavity, as when it was quite overcaſt. But if any one thinks that the reflexion of light from the pure air is alone ſufficient to ſuggeſt that idea, he would not diſpute it.

The concavity of the heavens appears to the eye, which is the only judge of an apparent figure, to be a leſs portion of a ſpherical ſurface than an hemiſphere. In other words, he ſays, the center of the concavity is much below the eye, and by taking a medium among ſeveral obſervations, he found the apparent diſtance of its parts at the horizon was generally between three and four times greater than the apparent diſtance of its parts over head.*

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* Smith's Opticks, Vol. 1, p. 63.

This he determined by meaſuring the actual height of ſome of the heavenly bodies, when, to his eye, they ſeemed to be half way between the horizon and the zenith. In this caſe their real altitude was only 23 degrees. When the ſun was but 30 degrees high, the upper arch always appeared leſs than the under, and he thought that it was always greater when the ſun was about 18 or 20 degrees high.

M. Folkes obſerved to our author, that the apparent diſtance of the ſun or moon at any given place might be computed within certain limits, by conceiving a perpendicular to fall upon the ground from any of the heavenly bodies, when they ſeemed to be half way between the horizon and the zenith, and by taking notice of ſome mark where it is imagined to fall; upon which he found that ſeveral perſons ſtanding together would agree in their judgments very nearly, then having meaſured that diſtance, and taken the altitude of the heavenly bodies, their apparent diſtance from it might eaſily be found by a caſe in plane trigonometry.*

Mr. Robins ſhews a method of determining the apparent concavity of the ſky in a more accurate and geometrical method; by which it appears that, if the altitude of any of the heavenly bodies be 20 degrees at the time that it ſeems to be half way between the horizon and the zenith, the horizontal diſtance will hardly be leſs than four times the perpendicular diſtance; but if that altitude be 28 degrees, it will be little more than two and an half.§

Dr. Smith, having, in the manner above mentioned, determined the apparent figure of the ſky, is able to give a moſt ſatisfactory explanation of the phenomenon of the horizontal moon, and other ſimilar appearances in the heavens. For ſuppoſing the arch ABC, fig. 160, to repreſent that apparent concavity, he found that the diameter of the ſun and moon would ſeem to be greater in the horizon than at any propoſed altitude, meaſured by the angle AOB, in the proportion of its apparent diſtances OA,OB. The numbers that expreſs theſe proportions he reduced into the following table, anſwering to the correſponding altitudes of the ſun or moon, which are alſo exactly repreſented to the eye in the figure, in which the moons, placed in the quadrantal arch FG, deſcribed about the center O, are all equal to each other, and repreſent the body of the moon at the heights there noted, and the unequal moons in the concavity ABC, are terminated by the viſual rays that come from the circumference of the real moon, at thoſe heights, to the eye at O. The diameters of thoſe unequal moons, at A and B, do, therefore, bear the ſame proportion to each other, as their apparent diſtances OA,OB; and they muſt appear in the very ſame proportion that they really have in this concave, becauſe we judge all objects in the heavens to be in this very ſurface; ſo that the appearance to the eye is exactly the ſame, as if ſeveral moons were painted upon a real ſurface ABC, in the proportions here aſſigned; in which caſe we ſhould certainly judge the real magnitudes of the larger paintings of the lower moon to be really larger, though the viſible

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* Remarks, p. 53.

§ Robins's Tracts, Vol. 2, p. 245.

The Sun or Moon's Altitude in Degrees.	Apparent Diameters or Diſtances.
00	100
15	68
30	50
45	40
60	34
75	31
90	30

ſible magnitudes of them all, anſwering to their equal images upon the retina, were exactly equal.

For the ſame reaſon, our author obſerves, that all other objects and diſtances of ſtars in the heavens, as well as the ſun and moon, muſt ſeem to be greater in the horizon than in higher ſituations; and it is well known they do ſo. Hence he deduces another experimental proof, that his proportions of the apparent magnitudes of the moon are exactly aſſigned. For this purpoſe he directs us to take notice of the diſtance of any two ſtars that lie very near each other, and as high as poſſible, and at the ſame time pick out two other ſtars ſituated as low as poſſible, whoſe diſtance from each other ſeems to be equal to that of the two higher ſtars, and then find their real diſtances, by a globe or by calculation, which will ſhew that the ſtars near the horizon are much nearer than we imagine them to be with reſpect to the diſtance of the two other ſtars.*

Dr. Smith alſo obſerves that the apparent concave of the ſky being leſs than an hemiſphere, is the cauſe that the breadths of the colours in the inward and outward rainbows, and the interval between the bows, appear leaſt at the top, and greateſt at the bottom; and by an eſtimate of the apparent breadths of the inward rainbow, at two different heights, made by a friend, he determined the apparent concavity of the ſky to be much the ſame as by the former methods. He alſo took it to be owing to the ſame cauſe, that a halo about the ſun or moon does not appear circular, and concentric to the ſun or moon, but oval and excentric, with its longeſt diameter perpendicular to the horizon, and extended from the moon farther downwards than upwards, as Sir Iſaac Newton has deſcribed one of them.*

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* Smith's Opticks, Vol. 2, p. 64.

This theory of the horizontal moon is alſo confirmed by the appearances of the tails of comets, which, whatever be their real figure, magnitude, and ſituation, in abſolute ſpace, do always appear to be an arch of the concave ſky; and in farther confirmation of it, he gives us Mr. Cotes's explanation of the optical appearance of a remarkable meteor, ſeen in the year 1716.§

To Dr. Smith's explanation of the appearance of the horizontal moon, I muſt add his account of an exactly ſimilar appearance, through a lens, where the ſuppoſed magnitude varies with the ſuppoſed diſtance, when the angle it makes with the eye is the ſame. Let the object PQR, fig. 161, be a wafer ſtuck upon a card, or white paper, fixed upon a board; or it may be any round diſtinct ſpot, whoſe diameter is at leaſt three or four times leſs than the diameter of the aperture of the lens AB. Then let this object and the lens be both fixed upon a longer board, MN, at an interval from each other exactly equal to the focal diſtance of the lens. When the board MN is firmly fixed, upon a level with the eye of the obſerver, let him recede for the ſpace of four or five focal diſtances from the lens, till the wafer, ſeen juſt behind it, ſeems to fill its aperture, or near it. Let him then imagine this apparent magnitude and diſtance of the wafer from his eye to repreſent the apparent magnitude and diſtance of the horizontal moon; and while he is returning gradually towards the lens, the apparent magnitude of the wafer will ſeem to decreaſe, in proportion to the decreaſe of its apparent diſtance from his eye. Theſe two phenomena, he ſays, agree in the only circumſtance that creates the difficulty in explaining the appearances of the moon, viz. that each of the viſual angles, under which the wafer and the moon appear, continue invariable. Nor could he think of any one circumſtance, immediately perceivable by the ſight, in which theſe two phenomena do not agree, ſince the refractions of the rays at the lens do no more affect the eye than if they had come ſtraight through a plane glaſs, from an increaſing and decreaſing wafer placed juſt behind it.*

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* Smith's Opticks, Vol. 2, p. 66. § Ib. p. 67.

It may be objected to this theory of the horizontal moon, that ſince the horizon, ſeen from the top of a mountain, appears more diſtant than it does from below, the horizontal moon ſhould alſo appear larger from above than below, which Dr. Smith believes to be contrary to experience; but he ſays, it muſt be conſidered that any preſent proſpect of the moon and the horizon together cannot ſo much ſuggeſt a new idea of a certain determinate magnitude of her diſc (the power of eſtimation by the eye being too weak, and inaccurate for this purpoſe) as it revives an old eſtabliſhed idea of it, reſulting from a long experience of innumerable proſpects, of various moons and horizons, ſeen from various ſituations. Our idea of the magnitude of the horizontal moon is, therefore, about a medium, taken from all our obſervations, and is ſo rooted in our minds, as not to be altered, but only confirmed by any ſingle one. The ſame may be ſaid of our idea of the moon at every particular degree of altitude. For this reaſon the former ideas may continue much the ſame, though the preſent view of the horizon be quite intercepted by the interpoſition of any near objects, and even that of the ambient ſky, alſo by the ſides of a tube, when the moon is ſeen through it. For we retain in memory very diſtinct ideas of the magnitudes of the moon for a long time. Nevertheleſs, in a long courſe of experience, if it happen very rarely that the magnitude of the picture of the horizontal moon is conſiderably altered by any means whatever, this powerful cauſe will immediately alter our eſtabliſhed idea of her uſual magnitude, as any perſon will find by viewing the moon through a ſpectacle glaſs, moved a little from his eye.*

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* Smith's Opticks, Remarks, p. 49.

Upon Dr. Smith's communicating his determination of the figure of the ſky to Mr. Folkes, this gentleman obſerved to him, that the ſky had frequently appeared of a conchoidal ſhape, having little or no concavity near the horizon, which Dr. Smith himſelf alſo thought is frequently very evident; but he obſerves that neither the proportion of the apparent diſtances of its vertical and horizontal parts, nor the method of determining it is ſenſibly altered thereby.§

Beſides this general cauſe of the appearance of the horizontal moon, Dr. Smith acknowledges that, at different times, the moon appears of different magnitudes even in the ſame horizon, and now and then of a ſize extraordinarily large. This, he was inclined to believe, is chiefly owing to an extraordinary largeneſs of her picture upon the retina, which, in the preceding general theory, was ſuppoſed to be invariable. This, he ſays, might beſt be examined, by taking the diameter of the moon with a micrometer, or by noting the year and day of the month, together with the heights of the barometer and thermometer.

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* Remarks, p. 54. § Ib. p. 52.

For if it ſhould appear, by many ſuch obſervations, that the largeſt horizontal moons generally happen at her perigee, in the warmeſt ſummer evenings, the barometer being low, and the thermometer high; ſince theſe cauſes are independent of one another, and all conſpire to enlarge the picture of the moon, we may reaſonably conclude that theſe extraordinary moons are chiefly owing to the concurrence of theſe three circumſtances.* But ſince the difference in the apparent magnitude of the moons is not increaſed $\frac{1}{16}$th part of the whole in conſequence of her being in her perigee, and the enlargement of the image in all the other caſes here mentioned it very inconſiderable, it is probable that when the moon is imagined to be ſo much larger than uſual, the imagination is farther impoſed upon by ſome circumſtances, which have not been attended to.

CHAPTER VIII.

Miscellaneous FALLACIES IN VISION.

A VARIETY of fallacies in viſion have been already explained, under the preceding heads of this ſection, and ſeveral of thoſe that furniſh materials for this part might have been arranged under ſome of them; but rather than every thing under any particular title ſhould not manifeſtly belong to it, I have reſerved theſe obſervations for a miſcellaneous chapter; which, as it contains ſome curious particulars, will, I hope, give my reader, a little entertainment.

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* Remarks, p. 53.

Mr. Molyneux propoſed a famous problem concerning viſion, which has occaſioned a good deal of ſpeculation, and controverſy among philoſophers and metaphyſicians. It is whether a man, who, when he was blind, had handled both a globe, and a cube, ſo as readily to diſtinguiſh them by the touch, could diſtinguiſh them by his eye, after recovering his ſight. The propoſer determined this queſtion in the negative, and Mr. Locke ſeems to have aſſented to his determination, upon the principle that ideas of ſight and touch have no natural relation to one another; but that all the connection they have is an arbitrary aſſociation of ideas, the ſame that words have with things. But Dr. Jurin obſerves, that the blind man having the globe and the cube ſhewn to him, and being told that they were the globe and cube, if he viewed them attentively, and eſpecially if he walked round them, could not but obſerve that, both to the touch and the ſight, the globe preſented the very ſame appearance; whereas the cube exhibited a conſiderable variety, whether it was ſeen or felt in different directions; and by this means might be able to pronounce which was the globe, and which the cube.*

I cannot help aſſenting to Dr. Jurin's ſolution. It is certain that pictures upon the retina give no idea of any thing more than flat ſurfaces; but then theſe have the ſame differences in the eye that they have in reality, by which they may, therefore, be diſtinguiſhed from one another; ſo that a circle might be known from a ſquare, though a ſquare might not be diſtinguiſhed from a cube, nor a flat circle from a globe, at leaſt without attentively conſidering the change of their appearance in different ſituations.

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* Smith's Opticks. Remarks, p. 27.

As Cheſſelden's account of the young man whom he couched, is often quoted upon this ſubject, it would be to ſuppreſs an important part of the evidence, not to recite it, I ſhall, therefore do it, as Dr. Smith has done, in his own words. I alſo chuſe to inſert this relation, becauſe it has, on other accounts, a near connection with my ſubject, and becauſe it cannot fail to be very pleaſing to my reader.

Though this gentleman was *blind*, as is ſaid of all perſons who have ripe cataracts, yet they are never ſo blind, from that cauſe, but that they can diſcern day from night; and for the moſt part, in a ſtrong light, diſtinguiſh black, white, and ſcarlet; but they cannot perceive the ſhape of any thing. For the light by which theſe perceptions are made being let in obliquely, through the aqueous humour, or the interior ſurface of the cryſtalline (by which the rays cannot be brought to a focus upon the retina) they can diſcern in no other manner than a ſound eye can through a glaſs of broken jelly, where a great variety of ſurfaces refract the light ſo differently, that the ſeveral diſtinct pencils of rays cannot be collected by the eye into their proper foci; wherefore the ſhape of an object, in ſuch a caſe, cannot be at all diſcerned, though the colour may. Thus it was with this young gentleman, who, though he knew theſe colours aſunder, in a good light, yet when he ſaw them after he was couched, the faint ideas he had of them before were not ſufficient for him to know them by afterwards, and therefore he did not think them the ſame which he had before known by thoſe names. Now ſcarlet he thought the moſt beautiful of all others, and of the reſt the moſt gay were the moſt pleaſing; whereas the firſt time he ſaw black, it gave him great uneaſineſs; yet after a little time he was reconciled to it, but ſome months after ſeeing, by accident, a negro woman, he was ſtruck with great horror at the ſight.

When he firſt ſaw, he was ſo far from making any judgment about *diſtances*, that he thought all objects whatever touched his eyes (as he expreſſed it) as what he felt did his ſkin, and he thought no objects ſo agreeable as thoſe which were ſmooth and regular, though he could form no judgment of their ſhape, or gueſs what it was in any object that was pleaſing to him. He knew not the ſhape of any thing, nor any one thing from another, however different in ſhape or magnitude; but upon being told what things were, the form of which he before knew from feeling, he would carefully obſerve them, that he might know them again; but having too many objects to learn at once, he forgot many of them, and, as he ſaid, at firſt he learned to know, and again forgot a thouſand things in a day. The Doctor ſays, that having often forgot which was the cat, and which the dog, he was aſhamed to aſk; but catching the cat (which he knew by feeling) he was obſerved to look at her ſtedfaſtly, and then ſetting her down ſaid, "ſo Puſs, I ſhall know you another time." He was very much ſurpriſed that thoſe things which he had liked beſt did not appear moſt agreeable to his eyes, expecting thoſe perſons would appear moſt beautiful that he loved moſt, and thoſe things to be moſt agreeable to his ſight, which were ſo to his taſte. It was thought he would ſoon know what things pictures repreſented, but it appeared afterwards that he did not. For about two months after he was couched, he diſcovered at once, that they repreſented ſolid bodies, when till that time he had conſidered them only as party coloured planes, or ſurfaces diverſified with variety of paint; but even then he was no leſs ſurprized, expecting the pictures would feel like the things they repreſented; and was amazed when he found thoſe parts, which, by their light and ſhadow, appeared now round and uneven, felt only flat, like the reſt, and aſked which was the lying ſenſe, feeling, or ſeeing.

Being ſhewn his father's picture, in a locket, at his mother's watch, and being told what it was, he acknowledged it to be a likeneſs; but was greatly ſurprized that a large face could be expreſſed in ſo little room; ſaying that it ſeemed as impoſſible to him, as to put a buſhel of any thing into a pint.

At firſt he could bear very little light, and he thought every thing to be extremely large; but upon ſeeing things that were larger, he conceived the firſt to be leſs, not being able to imagine any lines beyond the bounds which he ſaw. The room he was in, he ſaid, he knew to be but part of the houſe, yet he could not conceive that the whole houſe could look larger. Before he was couched, he expected little advantage from ſeeing worth undergoing an operation for, except reading and writing; for he ſaid he thought he could have no more pleaſure in walking abroad than he had in the garden, which he could do ſafely and readily. And even blindneſs, he obſerved, had this advantage, that he could go any where in the dark, much better than thoſe who can ſee; and after he came to ſee, he did not ſoon loſe this advantage, ſo as to deſire a light to go about the houſe in the night.

He ſaid every new object gave him new delight, and the pleaſure was ſo great, that he was not able to expreſs it; but his gratitude to his operator was exceedingly great, not being able to ſee him for ſome time without tears of joy in his eyes, and other marks of affection. A year after his firſt ſeeing, being carried upon Epſom downs, where he had an extenſive proſpect, he was exceedingly delighted with it, and called it a *new kind of ſeeing*.

When he was couched of his other eye, he ſaid that objects at firſt appeared large to that eye; but not ſo large as they did at firſt to the other; and looking upon the ſame object with both eyes, he thought it looked about twice as large, as with the firſt couched eye only, but not double, as far as could be diſcovered.

Mr. Cheſſelden brought ſeveral other perſons to their ſight, who had no remembrance of ever having ſeen, and they all gave the ſame account of their *learning to ſee*, as they called it, with the young perſon above mentioned, though not in ſo many particulars; and they all had this in common, that, having never had occaſion to move their eyes, they knew not how to do it, and at firſt could not direct them to any particular object; but in time they acquired that power, though by ſlow degrees.*

The moſt extraordinary circumſtance in this account is that objects did not appear double upon their firſt looking at them with both eyes, ſince a variety of other obſervations ſeem to prove that it is only by a ready aſſociation of ideas, in conſequence of much experience, that we conclude an object is ſingle, when there is a diſtinct picture of it in each eye. Dr. Reid, however, as we have ſeen, is of a different opinion, and doth not fail to avail himſelf of theſe obſervations of Chiſſelden in favour of his hypotheſis.

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* Smith's Opticks, Vol. 1, p. 42.

M. Le Cat well explains a remarkable deception, by which a perſon ſhall imagine an object to be on the oppoſite ſide of a board, when it is not ſo, and alſo inverted, and magnified. It is illuſtrated by fig. 162, in which D repreſents the eye, and CB a large black board, pierced with a ſmall hole. E is a large white board, placed beyond it, and ſtrongly illuminated, and *d* a pin, or other ſmall object, held betwixt the eye and the firſt board. In theſe circumſtances, the pin ſhall be imagined to be at F, on the other ſide of the board, where it will appear inverted, and magnified; becauſe what is, in fact, perceived, is the ſhadow of the pin upon the retina; and the light that is ſtopped by the upper part of the pin, coming from the lower part of the enlightened board, and that which is ſtopped by the lower part coming from the upper part of the board, the ſhadow muſt neceſſarily be inverted, with reſpect to the object.*

There is a curious phenomenon relating to viſion, which ſome perſons have aſcribed to the inflexion of light, but which Mr. Melville explains in a very different and very ſimple manner.

When any opaque body is held at the diſtance of three or four inches from the eye, ſo that a part of ſome more diſtant luminous object, ſuch as the window, or the flame of a candle, may be ſeen by rays paſſing near its edge, if another opaque body, nearer to the eye, be brought acroſs from the oppoſite ſide, the edge of the firſt body will ſeem to ſwell outwards, and meet the latter; and in doing ſo will intercept a portion of the luminous object that was ſeen before.

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* Traité des Sens, p. 298.

This appearance he explains in the following manner: Let AB, fig. 163, repreſent the luminous object, to which the ſight is directed, CD the more diſtant opaque body, GH the nearer, and EF the diameter of the pupil. Join ED, FD, EG, FG, and produce them till they meet AB in K, N,M, and L. It is plain that the parts AN, MB of the luminous object cannot be ſeen. But taking any point *a* between N and K, and drawing *a* D *d*, ſince the portion *d* F of the pupil is filled with light flowing from that point, it muſt be viſible. Any point *b*, between *a* and K, muſt fill *f* F, a greater portion of the pupil, and therefore muſt appear brighter. Again, any point *c*, between *b* and K, muſt appear brighter than *b*, becauſe it fills a greater portion *g* F with light. The point K itſelf, and every other point in the ſpace KL, muſt appear very luminous, ſince they ſend intire pencils of rays EKF, ELF to the eye; and the viſible brightneſs of every point from L, towards M, muſt decreaſe gradually, as from K to N; that is, the ſpaces KN, LM will appear as dim, ſhadowy borders, or fringes, adjacent to the edges of the opaque bodies.

When the edge G is brought to touch the right line KF, the penumbra's unite, and as ſoon as it reaches NDF, the above phenomenon begins; for it cannot paſs that right line without meeting ſome line, *a* D *d*, drawn from a point between N and K, and by intercepting all the rays that fall upon the pupil, render it inviſible. In advancing gradually to the line KDE, it will meet other lines *b* D *f*, *c* D *g*, &c. and therefore render the points *b*, *c*, &c. from N to K, ſucceſſively inviſible, and therefore the edge of the fixed opaque body CD muſt ſeem to ſwell outwards, and cover the whole ſpace NK, while GH, by its motion, covers MK. When GH is placed at a greater diſtance from the eye, CD continuing fixed, the ſpace OP to be paſſed over, in order to intercept NK, is leſs, and therefore, with an equal motion of GH, the apparent ſwelling of CD muſt be quicker, which is found true by experience.

If ML repreſent a luminous object, and REFQ any plane expoſed to its light, the ſpace FQ will be intirely ſhaded from the rays, and the ſpace FE will be occupied by a penumbra, gradually darker, from E to F. Let now GH continue fixed, and CD move parallel to the plane EF, and as ſoon as it paſſes the line LF, it is evident that the ſhadow QF will ſeem to ſwell outwards, and when CD reaches ME, ſo as to cover with its ſhadow the ſpace RE, QF, by its extenſion, will cover FE. This is found to hold true likewiſe by experiment.*

Dr. P. F. Gmelin of Wurtemberg, gives an account of ſome remarkable fallacies in viſion, that occurred to him in looking through teleſcopes, and compound microſcopes, the protuberant parts appearing depreſſed, and the depreſſed parts protuberant, in ſome caſes and not in others, at ſome times alſo and not at others, and to ſome eyes and not to others. After a great variety of trials, the conſtant effects that he obſerved were the following. As often as he viewed any object riſing upon a plane, of what colour ſoever, provided it was neither white nor ſhining, with the eye and the optical tube directly oppoſite to it, the elevated parts appeared depreſſed, and the depreſſed parts elevated. This happened in his viewing a ſeal, as often as he held the tube perpendicularly, and brought it in ſuch a manner, that its whole ſurface almoſt covered the laſt glaſs of the tube; and the ſame happened with the compound microſcope. But as often as he viewed any other objects, hanging perpendicularly from a plane, in ſuch a manner, that the tube was ſupported in a horizontal ſituation directly oppoſite to it, the ſame thing always happened; and the appearance was not altered when the object hung obliquely, or even horizontally.* At laſt he diſcovered a method of making objects always appear with their natural convexity, which was by directing his ſight not towards the center of the convexity, but at firſt to the edges of it only, and then gradually taking in the whole. But why theſe things ſhould ſo happen, he did not pretend to determine.§ For a general reaſon of theſe appearances, ſee the obſervation of Dr. Porterfield above mentioned.

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* Edinb. Eſſays, Vol. 2, p. 55.

It is obſerved in Birch's hiſtory of the Royal Society, Vol. 2, p. 348, that a compound microſcope of a new conſtruction, being produced at one of their meetings, and ſeveral of their members looking at a guinea through it, ſome of them imagined the image to be depreſſed, while others thought it to be emboſſed.

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* Phil. Tranſ. ab. Vol. 10, p. 31. § Ib. p. 32.

SECTION XIII.

Improvements in OPTICAL INSTRUMENTS.

THE greateſt improvement in optical inſtruments within this period, by means of original principles, is that of Mr. Dollond, of which an account has already been given in a preceding ſection, in which his diſcoveries in the ſcience of optics were explained. But, beſides the obligation we are under to Mr. Dollond for correcting the abberration of the rays of light in the focus of object glaſſes, ariſing from their different refrangibility, he made another conſiderable improvement in teleſcopes, viz. by correcting, in a great meaſure, both this kind of aberration, and alſo that which ariſes from the ſpherical form of lenſes, by an expedient of a very different nature; viz. increaſing the number of eye glaſſes.

If any perſon, ſays he, would have the viſual angle of a teleſcope to contain 20 degrees, the extreme pencils of the field muſt be bent or refracted in an angle of 10 degrees; which, if it be performed by one eye glaſs, will cauſe an aberration from the figure, in proportion to the cube of that angle; but if two glaſſes are ſo proportioned and ſituated, as that the refraction may be equally divided between them, they will each of them produce a refraction equal to half the required angle; and therefore, the aberration being in proportion to the cube of half the angle taken twice over, will be but a fourth part of that which is in proportion to the cube of the whole angle; becauſe twice the cube of 1 is but $\frac{1}{4}$ of the cube of 2; ſo the aberration from the figure, where two eye glaſſes are rightly proportioned, is but a fourth of what it muſt unavoidably be, where the whole is performed by a ſingle eye glaſs. By the ſame way of reaſoning, when the refraction is divided between three glaſſes, the aberration will be found to be but the ninth part of what would be produced from a ſingle glaſs; becauſe three times the cube of 1 is but one ninth of the cube of 3. Whence it appears that, by increaſing the number of eye glaſſes, the indiſtinctneſs which is obſerved near the borders of the field of a teleſcope may be very much diminiſhed, though not intirely taken away.

The method of correcting the errors ariſing from the different refrangibility of light is of a different conſideration from the former. For, whereas the errors from the figure can only be diminiſhed in a certain proportion according to the number of glaſſes, in this they may be intirely corrected by the addition of only one glaſs; as we find in the aſtronomical teleſcope, that two eye glaſſes, rightly proportioned, will cauſe the edges of objects to appear free from colours, quite to the borders of the field. Alſo in the day teleſcope, where no more than two eye glaſſes are abſolutely neceſſary for erecting the object, we find, that by the addition of a third, rightly ſituated, the colours, which would otherwiſe make the image confuſed, are intirely removed. This, however, is to be underſtood with ſome limitation; for though the different colours into which the extreme pencils muſt neceſſarily be divided, by the edges of the eye glaſſes, may, in this manner, be brought to the eye in a direction parallel to each other; ſo as, by the humours of the eye, to be made to converge to a point on the retina; yet, if the glaſſes exceed a certain length, the colours may be ſpread too wide, to be capable of being admitted through the pupil or aperture of the eye; which is the reaſon that, in long teleſcopes, conſtructed in the common manner, with three eye glaſſes, the field is always very much contracted.

Theſe conſiderations firſt ſet Mr. Dollond on contriving how to enlarge the field, by increaſing the number of eye glaſſes, without any hindrance to the diſtinctneſs or brightneſs of the image; and though others had been about the ſame work before, yet obſerving that ſome five glaſs teleſcopes which were then made, would admit of farther improvement, he endeavoured to conſtruct one with the ſame number of glaſſes in a better manner, which ſo far anſwered his expectations, as to be allowed by the beſt judges to be a conſiderable improvement on the former.

Encouraged by this ſucceſs, he reſolved to try if he could not make ſome farther enlargement of the field, by the addition of another glaſs, and by placing and proportioning the glaſſes in ſuch a manner as to correct the aberrations as much as poſſible, without any detriment to the diſtinctneſs; and at laſt he obtained as large a field as is convenient or neceſſary, and that even in the longeſt teleſcopes that can be made.

Theſe teleſcopes, with ſix glaſſes, having been well received, and ſome of them being gone to foreign parts, it ſeemed a proper time to the author, to ſettle the date of his invention; on which account he drew up a letter, which he addreſſed to Mr. Short, and which was read at the Royal Society March 1, 1753.*

M. Euler, who, in a memoir of the academy at Berlin for the year 1757, p. 323, &c. had calculated the effects of all poſſible combinations of lenſes in teleſcopes and microſcopes, publiſhed another long memoir on the ſubject of theſe teleſcopes, ſhewing with preciſion of what advantages they are naturally capable. It may be ſeen in the *Miſcellanea Taurinenſia*, Vol. 3, part 2, p. 92.

Various other attempts were made within this period to ſhorten, and otherwiſe improve teleſcopes. Among theſe I muſt juſt mention that of Mr. Caleb Smith, who, after giving much attention to the ſubject, thought that he had found it poſſible to rectify the errors which ariſe from the different degrees of refrangibility, on the principle that the ſines of refraction, or rays differently refrangible are to one another in a given proportion, when their ſines of incidence are equal; and the method which he propoſed for this purpoſe was to make the ſpeculums of glaſs inſtead of metal, the two ſurfaces having different degrees of concavity.§ But I do not find that this ſcheme was ever executed, nor is it probable, for reaſons which have been mentioned, that any advantage could be made of it.

But the moſt effectual ſervice that was done to aſtronomy within this period, by the powers of mechaniſm, was the eaſy conſtruction of *reflecting teleſcopes*, which was accompliſhed by John Hadley, Eſq; in 1723. In this year he preſented to the

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* Phil. Tranſ. ab. Vol. 48, p. 103.

§ Ib. Vol. 8, part 1, p. 114.

Royal Society a teleſcope which he had conſtructed upon Newton's plan, after it had been neglected fifty years. The radius of the ſphere to which the principal ſpeculum was ground was ten feet 5 $\frac{1}{4}$ inches, and conſequently its focal length was 62 $\frac{5}{8}$ inches. In the Philoſophical Tranſactions abridged, Vol. 6, p. 147, may be ſeen a drawing and deſcription of a very ingenious, but complex apparatus by which it was managed. The report that was made concerning the goodneſs of this teleſcope, by Mr. Pound and Dr. Bradley, Savilian profeſſor of Aſtronomy, who examined it together, was greatly in its favour.

One of theſe teleſcopes, in which the focal length of the large mirror was not quite 5 $\frac{1}{4}$ feet, they compared with the famous Huygenian teleſcope, the focal length of whoſe object glaſs is 123 feet, and they found that the former would bear ſuch a charge, as to make it magnify the object as many times as the latter with its due charge; and that it repreſented objects as diſtinctly, though not altogether ſo clear and bright; which they thought might be occaſioned partly by the difference of their apertures, that of the Huygenian being ſomewhat larger, and partly by ſeveral little ſpots in the concave ſurface of the object metal, which did not admit of a good poliſh.

Notwithſtanding this difference in the brightneſs of the objects, they were able, with this reflecting teleſcope, to ſee whatever they had hitherto diſcovered by the Huygenian, particularly the tranſits of Jupiter's ſatellites, and their ſhades over the diſk of Jupiter, the black liſt in Saturn's ring, and the edge of the ſhade of Saturn caſt on his ring.

They had alſo ſeen with it ſeveral times the five ſatellites of Saturn, in viewing of which this teleſcope had the advantage of the Huygenian, at the time that they compared them. For it being in ſummer, and the Huygenian teleſcope being managed without a tube, the twilight prevented them from ſeeing, in this, ſome of theſe ſmall objects, which at the ſame time they could diſcern with the reflecting teleſcope. Other obſervations were made with this excellent inſtrument on Jupiter and Saturn, which confirmed their good opinion of it.*

Upon the whole, they ſay that Mr. Hadley had convinced them, that this ſcheme of a teleſcope was not a mere theory, and they expreſſed their hopes that in a ſhort time a method might be found either of preſerving the concave mirror from tarniſhing, or of clearing it eaſily when it was tarniſhed; or elſe of making a good concave ſpeculum of glaſs, quick-ſilvered on the back ſide; and they ſay that when any of theſe ſhall be diſcovered, they ſhould not doubt but that the old dioptric teleſcope would for the moſt part be laid aſide, and the catoptric one be chiefly uſed by practical aſtronomers, ſince they are managed with ſo much more eaſe.§

The firſt reflecting teleſcopes which Mr. Hadley made were of the Newtonian conſtruction, but this being found inconvenient, for many purpoſes, he afterwards applied himſelf to make them in the Gregorian form, in which the large mirror is perforated. This ſcheme he completed in the year 1726; but it appears that in 1734 his reflecting teleſcopes, and all thoſe that were made in London, were exceeded by thoſe made by Mr. Short, who was at that time in Scotland, but afterwards removed to London. He firſt made his ſpeculums of glaſs, following the advice of Sir Iſaac Newton, but finding the light in them leſs than he expected,* and meeting alſo with great difficulty in finiſhing them, he applied himſelf to the improvement of metallic ſpeculums; and by giving particular attention to the curvature of them, he was able to give them greater apertures than other workmen could do; and by a more accurate adjuſtment of the ſpeculums, &c. he greatly improved the whole inſtrument. By ſome that he made, in which the larger mirror was 15 inches focal diſtance, he and ſome other perſons were able to read in the Philoſophical Tranſactions at the diſtance of 500 feet; and they ſeveral times ſaw the five ſatellites of Saturn together, which very much ſurprized Mr. Maclaurin, who gave this account of it, till he found that M. Caſſini had ſometimes ſeen them all with a 17 foot refracting teleſcope.§

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* Phil. Tranſ. Ab. Vol. 6, p. 153. § Ib. p. 153.

As the Gregorian reflecting teleſcope is that which has been chiefly in uſe from the dates above mentioned to the preſent time, I ſhall in this place introduce a ſhort deſcription of it, and give an idea of its magnifying power.

Let TYYT, fig. 164 be a braſs tube, in which LL is a metalic concave ſpeculum, perforated in the middle, and EF a leſs concave mirror, ſo fixed, as to be moved nearer to or farther from the larger. Let AB repreſent a very remote object, from each part of which iſſue pencils of rays, thoſe from the two extremities croſſing one another before they enter the tube. Theſe rays falling upon the larger mirror, are reflected from it ſo as to form an inverted image of the object in KH. From this image the rays, iſſuing as from an object, fall upon the ſmall mirror

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* Mr. Short found by experience that the light reflected from the beſt glaſs ſpeculums he could make was conſiderably leſs than the light reflected from metallic ſpeculums. He alſo found that glaſs was exceedingly liable to change its form by its own weight.

§ Smith's Opticks, Remarks, p. 81.

EF, the center of which is at *e*, ſo that they would meet in their foci at QQ, and there form an erect image. But ſince an eye in that place could ſee but a ſmall part of an object; in order to bring rays from more diſtant parts of it into the pupil, they are intercepted by the plano-convex lens MN, by which means a ſmaller erect image is formed at PV, which is viewed through the meniſcus SS, by an eye at O. This lens both makes the rays of each pencil parallel, and likewiſe magnifies the image. At the place of the image PV all the foreign rays are intercepted by the perforated partition ZZ. When a nearer object is viewed by this teleſcope, the ſmall ſpeculum is removed to a greater diſtance from the larger.*

The magnifying power of this teleſcope is eſtimated in the following manner: Let LD, fig. 165, be the larger mirror, having its focus at I, and FF the ſmall mirror, with its focus at G. Let *b b* be a ray of light coming from the lower extremity of a viſible object, and which, after being reflected from *b* to F, parallel to the axis of the mirror DK, is reflected to N, at the extremity of the lens MN, by which it would have been refracted to K; but by the interpoſition of another lens SS, is brought to O. The angle G *b* F, under which the object is viewed by the naked eye is to SOT, under which it is viewed with the teleſcope, in the ratio of G *b* F to IF *i*, which is equal to *n* IN of *n* IN to NK *n*; and of NK *n* to SOT. But G *b* F is to IF *i* as DI is to GA; *n* IN is to *n* KN, as *n* K is to I *n*; and NK *n* is to SOT, as TO is to TK; ſo that G *b* F is to SOT, as DI × *n* K × TO is to GA × I *n* × TK§

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* Muſchenbroeck's Introductio, Vol. 2, p. 821. § Ib. p. 822.

We may perhaps conceive a clearer idea of the magnifying power of a reflecting teleſcope, by conſidering it as a teleſcope and microſcope united; for we may conſider the object mirror LL fig. 166, forming the image of a diſtant object KH in its focus *e*, and the ſmall ſpeculum EF placed at its focal diſtance from the image, will give diſtinct viſion of it by parallel rays, to an eye at X; which is the function of the eye glaſs, and magnifies the object in the proportion of their focal lengths, which is all that is performed in a common teleſcope. But if the image be now looked upon as a microſcopic object, it is removed a little from the focus *e* of the ſmall ſpeculum, ſo far as to cauſe a large image of it QQ, to be formed near the eye, which is the effect of an object glaſs in the microſcope. This image is diſtinctly viewed in the focus of an eye glaſs or two, which is all that is performed in a common microſcope.*

To Mr. Short we are indebted for the excellent contrivance of an *Equatorial Teleſcope*, or, as he likewiſe called it, a *portable obſervatory*; for with it pretty accurate obſervations may be made with very little trouble, by thoſe who have no building adapted to the purpoſe. The inſtrument conſiſts of an ingenious piece of machinery, by the help of which a teleſcope mounted upon it may be directed to any degree of right aſcenſion or declination, ſo that, the place of any of the heavenly bodies being known, they may be found without any trouble, even in the day time. Alſo, being made to turn parallel to the equator, any object is eaſily kept in view, or recovered, without moving the eye from its ſituation. By this inſtrument, Mr. Short informs us, that moſt of the ſtars of the firſt and ſecond magnitude have been ſeen even at mid-day, and the ſun ſhining bright, as alſo Mercury, Venus, and Jupiter. Saturn and Mars are not ſo eaſy to be ſeen on account of the faintneſs of their light, except when the ſun is but a few hours above the horizon.* This particular effect depends upon the teleſcope excluding almoſt all the light, except what comes from the object itſelf, and which might otherwiſe efface the impreſſion made by its weaker light upon the eye. Any teleſcope of the ſame magnifying power would have the ſame effect, could we be ſure of pointing it right. For the ſame reaſon, alſo, it is that ſtars are viſible in the day time from the bottom of a deep pit.

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* Martin's Opticks, p. 272.

In order to enable us to ſee the fixed ſtars in the day time, it is neceſſary to exclude the extraneous light as much as poſſible. For this reaſon the greater magnifying power of any teleſcope is uſed, the more eaſily a fixed ſtar will be diſtinguiſhed in the day time; the light of the ſtar remaining the ſame, in all magnifying powers of the ſame teleſcope, but the ground upon which it is ſeen becoming darker by increaſing the magnifying power; and the viſibility of a ſtar depends very much upon the difference between its own light and that of the ground upon which it is ſeen. A fixed ſtar will be very nearly equally viſible with teleſcopes of very different apertures, provided the magnifying power remains the ſame.

If a comet, or any other heavenly body, be viewed through this equatorial teleſcope, properly rectified, it is ſeen immediately, by the help of the ſame machinery, what is its true place in the heavens. Other aſtronomical problems may alſo be ſolved by it, with great eaſe and certainty. See a deſcription of this inſtrument Phil. Tranſ. ab. Vol. 10, p. 154. Several improvements have been made in the machinery of this teleſcope by later mathematical inſtrument makers.

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* Phil. Tranſ. ab. Vol. 10, p. 156.

M. Æpinus propoſes to bend the tubes of long teleſcopes, at right angles, fixing a plane mirror in the angle, in order to make them more commodious for viewing objects near the zenith of the obſerver; and he gives particular inſtructions how to make them in this form, eſpecially when they are furniſhed with micrometers.* I am, alſo informed that a little plane ſpeculum is ſometimes placed betwixt the laſt eye glaſs and the eye, in reflecting teleſcopes, at an angle of 45° for the ſame purpoſe.

After the happy execution of the reflecting teleſcope, it was natural to expect that attempts would alſo be made to render a ſimilar ſervice to *microſcopes*. Accordingly we find two plans of this kind, the firſt of which was that of Dr. Robert Barker, a particular deſcription of which may be ſeen Phil. Tranſ. ab. Vol: 8, part 1, p. 120. This inſtrument differs in nothing from the reflecting teleſcope excepting the diſtance of the two ſpeculums, in order to adapt it to thoſe pencils of rays which enter the teleſcope diverging; whereas they come from very diſtant objects nearly parallel to one another.

The conſtruction of this microſcope is more particularly repreſented fig. 167, in which, inſtead of the lens d e f, there is placed a ſmall ſpeculum *d e f*; ſo that the object *acb* being placed above it, at a little greater diſtance from the focus *g*, has its image ACB, formed by reflexion, as in the other caſe it was by refraction, through the lens d f. Now if we ſuppoſe the focal diſtance of the object ſpeculum *d e f*, and lens d e f the ſame,

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* Nov. Com. Petrop. Vol. 9, p. 488.

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the effect of the microſcope will, in other reſpects, be the ſame alſo.

For the diſtance of the object *a b*, above the ſpeculum, will be equal to the diſtance of the object a b below the lens, in order that the image may be formed at the ſame diſtance C *e*. The poſition of the object will be inverted; for all the rays flowing from the point *a*, will be reflected by the ſpeculum to the point A, in the ſame manner as if they came by refraction through the lens from the point a. Thus the part *b* in the object will be reflected to the focus B in the image, which, therefore, is inverted. The power of magnifying will alſo be the ſame in both. For ſince the image AB and the object *a b* are ſeen under equal angles from the vertex *e* of the ſpeculum, the triangles *a e b*, and A *e* B will be ſimilar, and therefore AB : *ab* :: C *e* : *c e*; but in the other it is AB : a b :: C e : c e. But the latter ratio of theſe analogies is the ſame in both, and conſequently the firſt is ſo too.

This microſcope is not ſo eaſy to manage as the common ſort. For viſion by reflexion, as it is much more perfect, ſo it is far more difficult than that by refraction. Nor is this microſcope ſo uſeful for any but very ſmall, or tranſparent objects. For the object, being between the ſpeculum and image, would, if it were large and opaque, prevent a due reflexion.*

In Dr. Smith's reflecting microſcope there are two reflecting mirrors, one concave and the other convex, and the image is viewed by a lens. To explain it, let AD, fig. 168, be a large concave ſpeculum, and *a d* a ſmall convex one, each perforated in the middle with the holes BC, *b c*. Both theſe are ſegments of the ſame ſphere, or ground on tools of an equal radius, viz. of 2 inches, that ſo the focal diſtance of each ſpeculum may be juſt one inch.

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* Martin's Opticks, p. 217.

Theſe two ſpeculums are placed at the diſtance of about 1 ½ inch from each other, that ſo an object OPQ, being placed a little below the ſmaller ſpeculum, might be between the focus F and center E of the larger ſpeculum. Things thus circumſtanced, the rays PA,PD, which flow from the point P in the object, on the ſpeculum AD, will be reflected towards a focus *p*, where an image *o p q* would be formed, if the rays were not intercepted by the convex ſpeculum *a d*; and the point *p* being nearer than its focus *f*, the rays A *a*, D *d*, which tend or converge towards it, will be reflected to a focus *P*, where the laſt image, *OPQ*, will be formed, to be viewed through the eye glaſs G, by the eye at I.*

This microſcope, though far from being executed in the beſt manner, performed, Dr. Smith ſays, nearly as well as the very beſt refracting microſcopes; ſo that he did not doubt but that it would have excelled them, if it had been executed properly. Dr. Smith's own account of this inſtrument may be ſeen in his Opticks Remarks, p. 94.

In 1738 or 1739 M. Lieberkuhn made two capital improvements in microſcopes, by the invention of the *ſolar microſcope*, and the *microſcope for opaque objects*. When he was in England, in the winter of 1739, he ſhewed an apparatus of his own making, for each of theſe purpoſes, to ſeveral gentlemen of the Royal Society, as well as to ſome opticians, particularly Mr. Cuff in Fleet-ſtreet, who took great pains to improve them.

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* Martin's Opticks, p. 220.

The ſolar microſcope, as made by Mr. Cuff, was compoſed of a tube, a looking glaſs, a convex lens, and a Wilſon's microſcope. The tube is of braſs, near two inches in diameter, fixed in a circular collar of mahogany; which, turning round at pleaſure, in a ſquare frame, may be eaſily adjuſted to a hole in the ſhutter of a window, in ſuch a manner, that no light can paſs into the room, but through that tube. Faſtened to the frame by hinges, on the ſide that goes without the window, is a looking glaſs, which, by means of a jointed braſs wire, coming through the frame, may be either moved vertically or horizontally, to throw the ſun's rays through the braſs tube into the darkened room. The end of the braſs tube, without the ſhutter, has a convex lens, to collect the rays, and bring them to a focus; and on the end within the room Wilſon's pocket microſcope is ſcrewed, with the object to be examined applied to it, in a ſlider. The ſun's rays being directed by the looking glaſs through the tube upon the object, the image or picture of the object is thrown diſtinctly and beautifully upon a ſcreen of white paper, and may be magnified beyond the imagination of thoſe who have not ſeen it.*

The *microſcope for opaque objects* remedies the inconvenience of having the dark ſide of an object next the eye. For by means of a concave ſpeculum of ſilver, highly poliſhed, in the center of which a magnifying lens is placed, the object is ſo ſtrongly illuminated, that it may be examined with all imaginable eaſe and pleaſure. A convenient apparatus of this kind, with four different ſpeculums and magnifiers of different powers, was brought to perfection by Mr. Cuff.§

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* Phil. Tranſ. ab. Vol. 8, p. 127. § Ib. p. 128.

M. Lieberkuhn made conſiderable improvements in his ſolar microſcope, particularly in adapting it to the view of opaque objects; but in what manner this end was effected, M. Æpinus, who was highly entertained with the performance, and who mentions the fact, was not able to recollect; and the death of the ingenious inventor prevented his publiſhing any account of it himſelf. M. Æpinus invites thoſe perſons who came into the poſſeſſion of M. Lieberkuhn's apparatus to publiſh an account of this inſtrument, but whether they have done it or not, I have not yet been informed.

This improvement of M. Lieberkuhn's induced M. Æpinus himſelf to attend to the ſubject, and by this means produced a very valuable improvement in this inſtrument. For by throwing the light upon the foreſide of any object, by means of a mirror, before it is tranſmitted through the object lens, all kinds of objects are equally well repreſented by it. It is a recommendation of this improvement, that the body of the common ſolar microſcope is retained, and only an addition made of two braſs plates CA and BA, fig. 169, joined by a hinge, and kept at a proper diſtance by a ſcrew. The plate gives a view of them as they are annexed to a Wilſon's microſcope, which is uſed in the common ſolar microſcope. A ſection of theſe plates, and of all the neceſſary parts of the inſtrument, may be ſeen in fig. 170, where *c a* repreſent rays of the ſun converging from the illuminating lens, and falling upon the mirror *a b*, which is fixed to the nearer of the braſs plates. From this they are thrown upon the object, at *e f*, and thence are tranſmitted through the object lens at K; and a perforation in the farther plate, upon a ſcreen, as uſual. The uſe of the ſcrew *n* is to vary the diſtance of the two plates, and thereby to adjuſt the mirror to the object with the greateſt exactneſs. M. Æpinus thinks that ſeveral more improvements may be made in this curious inſtrument, particularly by uſing more object glaſſes, and he promiſes to give more attention to it.*

M. Euler propoſed a ſcheme to introduce viſion by reflected light into the magic lantern and ſolar microſcope, by which many inconveniences, to which thoſe inſtruments are ſubject, might be avoided. For this purpoſe, he ſays, that nothing is neceſſary but a large concave mirror, perforated as for a teleſcope, and that the light be ſo ſituated, that none of it may paſs directly through the perforation, ſo as to fall on the images of the objects upon the ſcreen. He propoſes to have four different machines, for objects of different ſizes; the firſt for thoſe of ſix feet long, the ſecond for thoſe of one foot, the third for thoſe of two inches, and the fourth for thoſe of two lines. An idea of this contrivance is given in fig. 171, in which OD repreſents the concave mirror, E the object, *l*, *l* the lights, and A the lens, through which the rays are tranſmitted to the ſcreen.§

Several improvements were made in the apparatus to the ſolar microſcope; as adapted to view opaque objects, by M. Zeiher, who made one conſtruction for the larger kind of objects, and another for the ſmall ones; but for the drawings and deſcriptions of them both, I muſt refer my reader to Nov. Com. Petrop. Vol. 10, p. 299.

Mr. Martin, having conſtructed a ſolar microſcope of a larger ſize than common, for his own uſe, the illuminating lens being 4 ½ inches in diameter, and all the other parts of the inſtrument in proportion, found that, by the help of an additional part, which he does not deſcribe, he could ſee even opaque objects very well. If he had made the lens any larger, he was aware that the heat produced at the focus would be too great for the generality of objects to bear. The expence of this inſtrument, he ſays, does not much exceed the price of the common ſolar microſcope. See his *ſhort account of an opaque ſolar microſcope*, ſubjoined to the deſcription of his *Graphical Perſpective*.

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* Nov. Com. Petrop. Vol. 9, p. 326.

§ Ib. Vol. 3, p. 363.

The ſmalleſt globules, and conſequently the greateſt magnifiers for microſcopes, that have yet been executed, were made by T. Di Torre of Naples, who, in 1765, ſent four of them to the Royal Society. The largeſt of them was only two Paris points in diameter, and it was ſaid to magnify the diameter of an object 640 times. The ſecond was the ſize of one Paris point, and the third was no more than half of a Paris point, or the 144th part of an inch in diameter, and was ſaid to magnify the diameter of an object 2560 times. One of theſe globules was wanting when they came into the hands of Mr. Baker, to whoſe examination they were referred by the Royal Society. This gentleman, ſo famous for his ſkill in microſcopes, and his extraordinary expertneſs in managing them, was not able to make any uſe of theſe. With that which magnifies the leaſt he was not able to ſee any object with ſatisfaction, and he concludes his account with expreſſing his hopes only, that, as his eyes had been much uſed to microſcopes, they were not injured by the attention he had given to them, though he believed there were few perſons who would not have been blinded by it.*

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* Phil. Tranſ. Vol. 56, p. 67.

This conſtruction of a teleſcope with ſix eye glaſſes led M. Euler to a ſimilar conſtruction of microſcopes, by introducing into them ſix lenſes, one of which admits of ſo ſmall an aperture, as to ſerve inſtead of a diaphragm, to exclude all foreign light, though, as he ſays, it neither leſſens the field of view, nor the brightneſs of objects.*

The improvement of all dioptric inſtruments is greatly impeded by inequalities in the ſubſtance of the glaſs, of which they are made, but though many attempts have been made to make glaſs without that imperfection, none of them have been hitherto quite effectual. M. A. D. Merklein, having found ſome glaſs which had been melted when a building was on fire, and which proved to make excellent object glaſſes for teleſcopes, concluded that its peculiar goodneſs aroſe from its not having been diſturbed when it was in a fluid ſtate; and therefore he propoſed to take the metal out of the furnace in iron veſſels, of the ſame form that was wanted for the glaſs; and after it had been perfectly fluid in thoſe veſſels, to let it ſtand to cool, without any diſturbance.§ But this is not always found to anſwer.

The magnifying power of any teleſcope may be eaſily found by *experiment*, in the ſame manner as that of a microſcope, mentioned before, viz. by looking with one eye through the teleſcope, upon an object of known dimenſions, and at a given diſtance, and throwing the image upon another object ſeen with the naked eye. In this method Mr. Haukſbee, Mr. Folkes, and Dr. Jurin, tried the magnifying power of Mr. Haukſbee's teleſcope, of the particular proceſs of which an account may be ſeen in Smith's Opticks, Remarks, p. 79. But the eaſieſt method of all is to meaſure the diameter of the aperture of the object glaſs, and that of the little image of it, which is formed at the place of the eye. For the proportion between theſe gives the ratio of the magnifying power, provided no part of the original pencil be intercepted by the bad conſtruction of the teleſcope. For in all caſes the magnifying power of teleſcopes or microſcopes is meaſured by the proportion of the diameter of the original pencil, to that of the pencil which enters the eye.

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* Nov. Com. Petrop. Vol. 12, p. 195.

§ Ac. Cæſ. Vol. 4, p. 507.

The MICROMETERS, of which an account has been given in the hiſtory of the preceding period, have ſeveral conſiderable defects. In particular, it is not eaſy to meaſure with them objects that are in motion, or thoſe which are too large to come within the field of view; ſo that the diameters of the ſun and moon cannot well be meaſured with them, to any great degree of exactneſs. Yet theſe were the only micrometers with which the heavenly bodies were meaſured in all the preceding periods of this hiſtory; but in this period a method was found of meaſuring the apparent magnitude of the object free from the inconveniences above mentioned, by means of a teleſcope furniſhed with two object glaſſes. This ingenious method was hit upon about the ſame time both by Mr. Servington Savery, and the celebrated M. Bouguer.

In this inſtrument, both object glaſſes are of equal focal diſtance, and placed one of them by the ſide of the other; ſo that the ſame eye glaſs may ſerve for them both. By this means two diſtinct images of an object are formed in the focus of the eye glaſs; and ſince the diſtance of theſe images depends upon the diſtance at which the two object glaſſes are placed from one another, it may be meaſured with great accuracy. Nor is it neceſſary that the whole diſc of the ſun or moon come within the field of view; ſince, if the images of only a ſmall part of the diſc be formed by each object glaſs, the whole diameter may eaſily be computed, by their poſition with reſpect to one another. For if the object be large, the images will approach towards, or perhaps even lay over one another. And the object glaſſes being moveable, the two images may always be brought exactly to touch one another, and the diameter may be computed from the known diſtance of the centers of the two glaſſes.*

Another advantage attending this inſtrument is that by having a common micrometer in the focus of the eye glaſs, when the two images of the ſun or moon are made in part to cover one another, that part which is common to both the images may be meaſured with great exactneſs, as being viewed upon a ground that is only one half leſs luminous than itſelf; whereas, in general, the heavenly bodies are viewed upon a dark ground, and, on that account, are imagined to be larger than they really are.§ By a ſmall addition to this inſtrument, provided it be of a moderate length, M. Bouguer thought it very poſſible to meaſure angles of three or four degrees; which is of particular conſequence in taking the diſtance of ſtars from the moon.†

Mr. Savery's paper containing a very particular deſcription of his conſtruction of this inſtrument was read at the Royal Society October 27, 1743, and M. Bouguer's account of his inſtrument which he called an *heliometer* is contained in the memoirs of the Royal Academy of ſciences for the year 1748, p. 15.

A very great improvement was made in this kind of micrometer by Mr. Dollond; for, inſtead of two complete object glaſſes, he uſed only one, cut into two equal parts, one of them ſliding by the other. Each half of this object glaſs will give a ſeparate and diſtinct image, and as the diſtance at which their centers are placed from one another may be exactly aſcertained, the ſame uſes may be made of them as of two intire object glaſſes, and the application of them is much more commodious.*

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* Ac. Par. 1748, H. p. 128. § Ib. p. 130. † Ib. p. 131.

M. Zeiher took a great deal of pains with the ſubject of *burning mirrors*, chiefly with the laudable view of making the conſtruction of them cheaper, ſo that almoſt every philoſopher might have them in his private apparatus. He firſt went to work on the plan of Kircher, before he had heard of what was done by M. Buffon, with a combination of plane mirrors. He then improved upon an inſtrument conſtructed by Sir Iſaac Newton, conſiſting of ſeveral concave mirrors, diſpoſed in the ſame manner; and, to ſave expence, he took only pieces of plane glaſs, and gave them the requiſite degree of curvature, by heating them ſo hot, on a diſh made of metal, that they would aſſume the ſame form.§

Many experiments to aſcertain the proportion of heat produced by burning mirrors, at different diſtances from the focus, and compared with the power of the direct rays, were made by M. Richman; but he could not find that there was any exact proportion in this caſe; and though he propoſes another ſcheme, which he thought was likely to anſwer better, it does not appear that he ever executed his deſign.†

I ſhall conclude this ſection, concerning *optical inſtruments*, with an account of one deſcribed by Mr Martin, which he calls a

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* Phil. Tranſ. Vol. 48, p. 178. § Nov. Com. Petrop. Vol. 7, p. 237.

† Ib. Vol. 3, p. 340.

Graphical Perſpective. It conſiſts of a tube, containing two lenſes, AB and CD, fig. 172, which are placed at twice their focal diſtance from one another; and in their common focus is another glaſs EF divided into equal parts, with the point of a diamond. Though, therefore, this inſtrument does not magnify any object, yet the angle under which an object is ſeen is eaſily known by means of it; and ſince this angle varies with the diſtance of objects, it is eaſily applied to the purpoſe of meaſuring inacceſſible heights and diſtances; and ſince the field of view is divided into equal ſquares, it is uſeful in drawing the perſpective appearance of objects. Moreover, as all foreign light is excluded by the tube in which theſe lenſes are incloſed; pictures ſeen through it have a fine relievo, on which account, as alſo becauſe objects appear inverted through it, the images of a Camera Obſcura are viewed to pecular advantage by the help of it. If a lens of a greater focal length be fixed at a proper diſtance from the center of the tube, this inſtrument will be a teleſcope, and will magnify the prints which are looked at through it; and if a ſmall lens be uſed, it will be a microſcope, and the ſame micrometer will ſerve for them both.*

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* Martin's Opticks, p. 288.

SECTION XIV.

Improvements in the MATHEMATICAL part of the science of opticks.

THE ſcience of Opticks has been far from having reaſon to complain of the want of any aſſiſtance that *Mathematics* have been able to afford it within this period. Many elaborate diſquiſitions have been publiſhed in the memoirs of ſeveral philoſophical ſocieties, beſides ſeveral intire treatiſes on optical ſubjects. Thoſe who have diſtinguiſhed themſelves the moſt in this way are M. Euler, M. Clairaut, and M. D' Alembert.

Not a ſingle circumſtance relating to the power, or the conſtruction of teleſcopes has eſcaped the attention of M. Euler, whoſe laborious calculations on theſe ſubjects may be ſeen in ſeveral volumes of the memoirs of the ſociety at Berlin; and all the diſſertations of M. Euler on the ſubject of opticks are now digeſted into one treatiſe printed at Peterſburgh, which was announced to the world in Nov. Com. Petrop. Vol. 12, p. 23. The aberration of the rays of light in ſpherical lenſes was conſidered by Ab. Got. Kæſtner, whoſe eſſays on this ſubject may be ſeen Com. Gotting, Vol. 1, p. 185, and Vol. 2, p. 183. The Count of

Redern, alſo, took great pains to explain the principles of teleſcopes, and has laid down rules for the application of his principles to practice, in the conſtruction of all kinds of teleſcopes and microſcopes.*

From the phenomena of light M. Mairan conceived the idea of another curve beſides the *cauſtic*, which he calls the *reflectoire*, becauſe it is the appearance of the plane bottom of a baſon covered with water, to an eye perpendicularly over it. In this poſition the bottom of the baſon will appear to riſe upwards from the center outwards; but the curvature will be leſs and leſs, and at laſt the ſurface of the water will be an aſſymptote to it. He alſo found ſeveral kinds of theſe curves, and he gives a geometrical deduction of their properties, ſhewing their analogy to cauſtics by refraction.†

M. Kurdwanowſki, a Poliſh gentleman, obſerved that any line, ſtraight or curved, expoſed to the action of a luminous point, received the light differently in its different parts, according to their diſtance from the light. Theſe different effects of the light upon each point of the line may be repreſented by the ordinates of ſome curve, which will vary preciſely with theſe effects. To theſe curves he gives the name of *courbes a la lumiere.*§

Dr. Smith has, in a ſeries of propoſitions, determined the apparent ſhapes, poſitions, magnitudes, and diſtances of large objects ſeen by rays that fall upon reflecting or refracting ſurfaces, either perpendicularly, or with any degrees of obliquity.‡ This is a ſubject that Newton no where touched upon, and only a caſe or two of it were imperfectly noticed by M. Huygens, and Dr.

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* Ac. Berl. 1761.

† Ac. Par. 1740, H. 121, M. 1.

§ Ib. 1732, H. p. 135.

‡ Opticks, Vol. 1, p. 135.

Barrow. Upon this ſubject, therefore, our author is original; but as his reaſoning cannot be abridged, I muſt refer my mathematical readers to his treatiſe, contenting myſelf with reciting the more curious of the optical phenomena which he has explained by the help of his principles.

From this theory, he ſays, it follows that a ſtraight object, ſeen through a glaſs, bounded by parallel plane ſurfaces, ſhould appear a little concave towards the eye, though ſcarce ſenſibly, unleſs the glaſs be very thick.*

It alſo agrees with this theory, that a large plane object, ſeen through a deep concave lens, always appears concave towards the eye, and the more concave, as the object is more remote, and the lens at ſome diſtance from the eye.§

A large plane object, ſeen inverted through a very thick convex lens or ſphere, appears convex towards the eye placed at ſome diſtance from the glaſs, and concave when it is ſeen upright. The parallel hairs of a micrometer are another inſtance of this kind, appearing always concave towards the eye, and convex towards each other, when opened ſo wide as to be ſeen through the edges of a thick eye glaſs.‡

Several curious phenomena of viſion with both eyes are likewiſe explained by him upon theſe principles. He particularly conſiders more fully the experiment of the compaſſes, mentioned above, adding another caſe to the manner of viewing them.

If, ſays he, the legs be opened to any angle, and held by the joint, in a plane nearly perpendicular to a plane paſſing through the axes of the eyes, with their points in this latter plane, and the axis of the right eye be directed ſteadily towards the point on the left hand, and that of the left eye towards the point on the right hand; then, of the legs that appeared double, the two innermoſt will ſoon unite in one, appearing like a third leg in the middle, between the other two, and will tend from the joint towards the interſection of the axis of the eyes; and this middle leg, if it be viewed attentively, while the real legs are gradually opened, will decreaſe in apparent magnitude, and approach towards the eyes; and, on the contrary, will increaſe and recede while the angle of the legs is contracted.*

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* Opticks, Remarks, p. 83. § Ib. p. 84. ‡ Ib.

The like phenomenon, he ſays, is obſervable in viewing two lighted candles, of an equal height and thickneſs, at the diſtance of two or three feet from the eyes; but the apparent candle in the middle does not approach quite ſo near the eyes as the correſponding leg of the compaſſes, in proportion to the given diſtance of the original objects from the eyes.

In figure 173, a *a* and b *b* are the diameters of the candles, *d* and *e* the centers of the pupils, *a e* a and *b d* b two cones of rays, croſſing one another in *f*, where the candles appear united in one, of a ſize proportional to the thickneſs of the cones at *f*, or a little beyond it. Now if the candles *a b*, be gradually drawn aſunder, their apparent union near *f*, while the eyes are fixed upon it, will decreaſe in apparent magnitude, and ſeem to come nearer. For the ſimilar and equal pictures of the two candles upon correſponding places of the two retina's, cauſe the ſame ſenſation as two ſuch pictures of a ſingle candle at *f* would do, and this ſenſation excites the uſual idea of a ſingle candle.

If the lines *a f e*, *b f d*, be drawn upon a plane board, a foot or two long, and a pin be ſtuck upright, at their interſection *f*, and the pupils of the eye be placed near the edge of the board, at any ſmall height above the points *d*, *e*, while the pin is viewed ſteadily, the two lines *f a*, *f b* will appear united, and upright, in the place, or by the ſide of the pin. For, in this caſe, they cannot appear in two different places, and therefore muſt appear in the common interſection of two planes, paſſing through the lines *a f*, *b f*, and the pupils of the eyes.*

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* Remarks, p. 86.

Upon theſe principles our author likewiſe explains a phenomenon, which is, at firſt, very much ſurpriſing. Viewing a foot ruler, held upright between his eyes, with its flat ſide againſt his forehead, and about a foot farther from a concave ſpeculum than the center of its concavity, it appeared convex to either eye alone, but concave to both eyes at once.†

He alſo explains a caſe which Dr. Barrow took notice of as ſomewhat extraordinary. If a perſon view his face in a concave ſpeculum, held pretty near to it, firſt with the right eye only, and then with the left, the appearance will be the ſame, only a little more to one hand, than the other, in each caſe. But upon looking immediately with both eyes, the two apparent faces will ſeem to be united, in a much larger face, more remote, and more concave than before.§ Alſo a piece of perſpective, placed a little nearer to a very large concave ſpeculum than its principal focus, and viewed by a perſon ſtanding cloſe behind the piece, and looking over the top of it, appears in the glaſs much larger, and more projected, and conſequently to much greater advantage than when it is ſeen by direct rays.‡

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* Smith's Opticks, Remarks, p. 86. † Remarks, p. 86. § Ib. p. 87. ‡ Ib.

SECTION XV.

MISCELLANEOUS OBSERVATIONS.

I HAVE made ſo many diſtinct ſections in this period of my hiſtory, that I have reſerved but few materials for this miſcellaneous one. The following particulars, however, could not well have been arranged under any of the former heads, and yet, I doubt not, but that my reader will think them well worth his notice.

Almoſt all optical experiments have been made with the light of the ſun, and few perſons ſeem to have thought of comparing this, in any reſpect, with the light of other burning bodies, from which it is poſſible that conſiderable diſcoveries might ariſe. Some obſervations, however, have been made on this ſubject by Mr. Melville, with which, my reader cannot but be pleaſed, as they may be a beginning to ſomething farther of the ſame kind.

Bodies of all the principal colours, namely red, yellow, green, and blue, are very little altered, he obſerves, when they are ſeen by the light of burning ſpirits; but if ſalts be continually mixed with them, during the burning, different changes enſue. When ſal ammoniac, pot aſh, or alum are infuſed, the colour of red bodies appears ſomewhat faded and dirty. Green and blue appear much the ſame as in candle light, both being faint, and hardly diſtinguiſhable. White and yellow are hardly at all affected.

When nitre, or ſea ſalt, is plentifully mixed with the burning ſpirits, and the whole is ſtirred about briſkly, the brighteſt red bodies, ſeen by the light then emitted, are reduced to a dirty tawny brown, which ſeems to have nothing of redneſs in it. Green is transformed into another ſort of brown, only diſtinguiſhable from the former by a certain inclination to a livid olive colour. When nitre is mixed with the ſpirits, one may ſtill ſee ſome remains of a greeniſh colour, unleſs it be poured in very plentifully. Dark blue is hardly to be known from black, except that it appears the deeper black of the two. Light blue is changed into a very light brown, of a peculiar kind. White aſſumes a livid yellowiſh caſt; and yellow alone appears unaltered, and extremely luminous.

Theſe experiments he made with different ſorts of richly coloured bodies, as ſilks, cloths, and paints. Poliſhed copper, which had contracted from the air a high flaming colour, was reduced, by the ſame light, to the appearance of yellow braſs. The faces and hands of the ſpectators appeared like thoſe of a dead corpſe; and other mixed colours, which had red or green in their compoſition, underwent the like changes.

Having placed a paſteboard, with a circular hole in it, between his eye and the flame of the ſpirits, in order to diminiſh and circumſcribe this object, he examined the conſtitution of thoſe different lights with a priſm, holding the refracting angle upwards; and found that, in the firſt caſe, viz. when ſal ammoniac, alum, or pot aſh fell into the ſpirits, rays of all kinds were emitted, but not in equal quantities, the yellow being much more copious than all the reſt put together, and the red being more faint than the green and blue.

In the light of ſpirits mixed with nitre, or ſea ſalt, he could obſerve ſome blue, though exceedingly weak and diluted. With the latter the green was equally faint, but with the former pretty copious. But when either of theſe ſalts were uſed, he could hardly perceive any trace of red, eſpecially when they were mixed in great plenty, and the ſpirits conſtantly agitated. At every little intermiſſion, indeed, the red rays would ſhew themſelves very manifeſtly below the hole, and red bodies ſeen by that light reſumed ſomewhat of their ordinary colour; and it was entertaining to obſerve how both would vaniſh again at once, when the ſalting and ſtirring were renewed.

The proportion in which the bright yellow exceeded the other colours in this light, was ſtill more extraordinary than in the former; in ſo much that the hole, ſeen through the priſm, appeared uniformly of that yellow, and as diſtinctly terminated as through a plane glaſs; except that there was adjoining to it, on the upper ſide, a very faint ſtreak of green and blue. White bodies illuminated with it, appeared alſo through the priſm perfectly well defined, both which, he ſays, are very ſurpriſing phenomena to thoſe who have been accuſtomed to the uſe of the priſm in heterogeneous lights, where it never fails to throw confuſion on the extremities of all viſible objects.

Upon examining bubbles of ſoap and water, in the ſame light, he could only obſerve luminous ſtreaks, ſeparated by dark ones, the green and blue being too weak to affect his eye.*

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* Edinb. Eſſays, Vol. 2, p. 32.

Our author has alſo the following query on this ſubject, at the concluſion of his eſſay. Are not the rays emitted by all kinds of luminous bodies, ſimilar to thoſe of the ſun, both as to colour and degrees of refrangibility; and do not luminous bodies differ from one another only according to the colours which they emit moſt plentifully; in like manner as opaque bodies are diſtinguiſhed by the colours of incident light, which they reflect in the greateſt abundance? But to make our induction ſufficiently ſtrong, ought not, ſays he, experiments to be made with the lights of a greater variety of bodies; and would it not further conduce to the illuſtration of this queſtion, to form, by Sir Iſaac's method, a beam of ſolar light, conſiſting of ſuch colours, and in ſuch proportions, as were ſeen in the lights of ſalts and burning ſpirits, and then to obſerve in it the appearance of coloured bodies. Farther, are not the intervals of the fits in rays of any one colour the ſame in the ſame medium, from whatever luminous body they are emitted; for if theſe intervals were different, would there not be changes in the colours of bodies not to be accounted for by the compoſitions of the lights, with which they are illuminated.*

To theſe obſervations of Mr. Melville's, I ſhall add the following collected by M. Muſchenbroeck. The light of camphor, and alſo of burning zinc is white. Zinc mixed with arſenic, yields a greeniſh flame. Zinc with orpiment a black flame, and braſs in the fire gives a green flame.

Though the *colours of different mixtures* have no immediate relation to the general doctrine of light, and will very properly come to be mentioned when I treat of the ſubſtances themſelves; yet as many of my readers will probably expect to find an account of ſome facts of this kind in this part of my work, I ſhall copy from M. Muſchenbroeck, what he has collected from Boerhaave and others, together with ſome obſervations of his own.

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* Edinb. Eſſays, Vol. 2, p. 39.

1. *Colours produced in liquors which originally have no colour.*

Spirit of wine with red roſes, upon which, whilſt it is yet white, any ſaline acid ſpirit is poured, in ſo ſmall a quantity that the acid can hardly be perceived, produces a florid red.

Many other red flowers hardly give any colour to ſpirit of wine in a ſhort time, yet give a red colour by the addition of any of the acids.

A ſolution of mercury and oil of tartar gives orange.

A ſolution of ſublimate and lime water produces yellow.

The tincture of red roſes with oil of tartar per deliquium, or with ſpirit of ſal ammoniac gives green. In like manner, the tincture of many red flowers is changed into green by an alkali.

The tincture of red roſes and ſpirit of urine gives blue.

The ſolution of copper and ſpirit of ſal ammoniac gives purple.

The ſolution of ſublimate and ſpirit of ſal ammoniac gives white.

The ſolution of ſaccharum ſaturni and the ſolution of vitriol, alſo the tincture of red roſes, or many other red flowers, and the ſolution of copperas in water, and likewiſe the ſolution of galls and copperas, produces black.

2. *Colours ariſing from coloured mixtures.*

1.	Yellow	Tincture of red roſes	produces	green.
	Red	Tincture of crocus		
2.	Green	Tincture of violets		carmine.
	Brown	Spirit of ſulphur		

3. Red

3.	Red Brown	Tincture of red roſes Spirit of hartſhorn	blue.
4.	Blue Blue	Tincture of violets Solution of copper	violet.
5.	Blue Blue	Tincture of violets Solution of Hungarian vitriol	purple.
6.	Blue White	Tincture of cyanus Spirit of ſal ammoniac	green.
7.	Blue Brown	Solution of Hungarian vitriol Lixivium	yellow.
8.	Blue Red	Solution of hungarian vitriol Tincture of red roſes	black.
9.	Blue Green	Tincture of cyanus Solution of copper	red.
10	Violet Tranſparent	Tincture of dipſacum, or iris, or other flowers that give a violet colour in water Allum diſſolved in water	blue.

3. *Colours changed and reſtored.*

The ſolution of verdigriſe becomes colourleſs by the affuſion of ſpirit of nitre, and by the affuſion of the oil of tartar it becomes green again.

The tincture of red roſes grows black by the ſolution of vitriol, but is red again by means of oil of tartar.

The tincture of roſes aſſumes a beautiful red colour by the affuſion of ſpirit of vitriol; the addition of ſpirit of ſal ammoniac makes it green, and the oil of vitriol makes it red again.

The ſolution of verdigriſe is green, but it becomes colourleſs by the affuſion of ſpirit of vitriol, the ſpirit of ſal ammoniac gives it a purple colour, and oil of vitriol makes it pellucid.

The ſolution of galls with vitriol gives black, the affuſion of oil of vitriol deſtroys the blackneſs, and it becomes pellucid as before.

This author thought it to be univerſally true, that yellow vegetables, diſſolved in ſpirit of wine, gave yellow tinctures, which are either not at all, or very little changed by acid, alkaline, or any other ſalts; but he thought there were ſome exceptions, and ſays, that when oil of vitriol is put to a yellow paint, called *orlean*, it becomes of a beautiful blue, which is deſtroyed by water, or any ſalt.

When experiments were made with acidulous or vitriolic water, and aſtringent ſubſtances, the following were the reſults. A ſmall quantity of the aſtringent ſubſtance, with iron, excites a black colour. A greater quantity of the aſtringent produces blue, more of it makes a violet, and more ſtill produces purple.

There are ſome tinctures, the colours of which depend upon the external air. Such is the red tincture made of *Canarian lichen*, commonly called *orſeille*, and water, or a diluted ſpirit of wine, with lime and an urinous ſalt. For if this tincture be put into a glaſs tube, hermetically ſealed, in a few days it becomes colourleſs; and when the tube is opened, the colour returns. The Abbé Nollet made a variety of experiments with this tincture.

Similar to the experiments above mentioned are thoſe which produce what are commonly called *ſympathetick inks*, of which M. Hellot has given the following ſpecimens.

If litharge be diſſolved in diſtilled vinegar, and letters be written with it, and dried in the ſhade, nothing will appear; but if it be waſhed with a ſolution of orpiment in lime water, the letters will be firſt yellow, and then black; and aqua fortis will make them diſappear again. If a ſolution of gold in aqua regia be diluted with five times as much water, letters written with it will be viſible; but if it be waſhed with a ſolution of tin in aqua regia, diluted with five times the quantity of water, the letters will become purple.

If letters be written with a ſolution of gold in aqua regia, diluted with water, and be expoſed to the air in drying, they will firſt become yellow, and then purple.

If letters be written with a ſolution of zinc, biſmuth, or cobalt, in aqua fortis, mixed with marine ſalt, and purified, they will be inviſible while cold, but blue when hot.*

The reſult of a variety of mixtures to produce colours were recited before the Royal Society by Dr. Goddard, January 16, 1661, and they may be ſeen in Birch's Hiſtory, Vol. 1, p. 11.

I ſhall conclude this laſt ſection of my work with ſome of Dr. Hartley's obſervations concerning colours, which may be worth the notice of philoſophers, independently of the particular uſe he propoſed to make of them, which was to ſupport his hypotheſis, that the extreme red rays excite vibrations in the retina, which are to thoſe that are excited by the extreme violet ones, as 1 to 2, in reſpect of frequency; and that, in going from the extreme red, to the extreme violet, the exceſs of vibrations excited by each colour, above thoſe of the extreme red, will be proportional to its diſtance from the extreme red.

If, ſays he, we admit theſe two ſuppoſitions, then the vibrations excited by the extreme red, by the limit of red and orange, of orange and yellow, yellow and green, green and blue, blue and indigo, indigo and violet, and by the extreme violet, as theſe colours are fixed by Sir Iſaac Newton, will be to one another in frequency, reſpectively, as the 8 numbers 100, $112\frac{1}{2}$, 120, $133\frac{1}{3}$, 150, $166\frac{2}{3}$, $177\frac{7}{9}$, and 200; the diſtances of theſe ſeveral limits, and of the extreme violet from the extreme red, being to one another reſpectively as the 7 numbers $12\frac{1}{2}$, 20, $33\frac{1}{3}$, 50, $66\frac{2}{3}$, $77\frac{7}{9}$, and 100.

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* Introductio, Vol. 2, p. 740, &c.

Now the firſt ſuppoſition may be rendered probable thus. The intervals of the fits of eaſy reflexion and tranſmiſſion of the red and violet, in the ſame medium, and the ſame angle of refraction, are nearly as 5 to 3. See Newton's Opticks, Book 2, Obſ. 13, 14, and Prop. 16. But the red is leſs refracted by the coats and humours of the eye than the violet, and conſequently will not have its intervals ſo much diminiſhed in proportion; whence they may be to thoſe of the violet as 6 to 3, or 2 to 1, at their arrival on the retina. But it is probable that the vibrations of the rays themſelves, and conſequently thoſe which they excite in the retina, are reciprocally as the intervals of their fits. The frequency, therefore, of the vibrations excited by the extreme red, may be to that of the vibrations excited by the extreme violet as 1 to 2.*

Among other deductions from the above mentioned hypotheſis, he obſerves, that, ſince the greens are reſpectively to the yellows on one hand as 9 to 8, and to the blues on the other as 9 to 10, i. e. in the proportion of a tone, alſo to the reds on one hand as 4 to 3, and to the violets on the other as 3 to 4, i. e. in the proportion of a 4th: ſince, farther, the yellows are as 6 to 5, i. e. 3ds minor, to the reds; as 4 to 5, i. e. 3ds. major, to the blues; and as 2 to 3, i. e. 5ths to the violets; ſince the blues are as 5 to 6, i. e. 3ds minor, to the violets; and as 3 to 2, i. e. 5ths to the reds; and the reds as 9 to 16, i. e. flat 7ths to the violets, the difference of vibrations here exhibited may make the five foregoing colours appear diſtinct from each other to the mind, for the ſame reaſons, whatever they may be, that take place in ſounds. For natural bodies reflect all theſe colours in great abundance, and in ſufficient purity for this purpoſe. We may begin from green, as the moſt common of all. When this, as reflected by graſs, ſuppoſe, has been ſufficiently familiarized to the eye of a child, it is reaſonable to think that it may be diſtinguiſhed from yellow and blue, and much more from red and violet, as reflected by flowers, alſo that theſe may be diſtinguiſhed from each other. And it ſeemed to him that our fixed point ought to be placed in green, from the commonneſs and purity of the green of the third order, i. e. of graſs and vegetables in general. For the ſame reaſons one may expect that the ſeveral ſhades of red, orange, green, blue and violet, ſhould be conſidered as ſeveral degrees of the ſame colour, viz. on account of the ſmall difference of vibrations. At leaſt, this correſponds to the uſual method of proceeding in other things. We diſtinguiſh great differences in our ſenſations by new names, but refer all ſuch as are nearly related to the ſame. And thus the two foregoing ſuppoſitions furniſh us with a natural reaſon for diſtinguiſhing the primary colours into five, viz. red, yellow, green, blue, and violet; which, agreeably to this, were all that Sir Iſaac Newton himſelf diſtinguiſhed the oblong ſolar image into for ſome time, as may appear by his *Optical lectures*.*

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* Hartley's Obſervations on Man, Vol. 1, p. 193.

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* Hartley's Obſervations on Man, Vol. 1, p. 193.

It is remarkable, he ſays, that the order of the five tones and two ſemitones of an octave, which correſponds to the order of the ſeven primary colours, is the ſecond in abſolute perfection; which he had from a MS. paper of Sir Iſaac Newton's on Muſic, not yet publiſhed; and the firſt in relative, i. e. of thoſe in which the ſemitones are at equal diſtances from the middle or extremes; which circumſtance is evidently neceſſary in the order of the colours. For if diſtinct colours ariſe from ratio's, and a half-note colour ariſe next after the red; if, ſays he, you begin at one end; a correſponding one ought to appear next after the violet, if you begin at the other. The ſameneſs of the ratio's that muſt ariſe makes this neceſſary, on the ſuppoſition that the diſtinction of colours is founded on ratio's.

Farther, if the diſtinction of colours ariſe from the ratio's of vibrations, the colours may be expected to be broader where the vibrations are more numerous; becauſe a greater addition muſt be made to a greater number, in order to make an equal ratio. And there is a certain breadth for each of the colours reſpectively, which ſuits each ſet of ratio's of vibrations that they can be ſuppoſed to bear to one another, according to any ſuppoſed law of increaſe of the vibrations, in paſſing from one end of the ſolar image to the other. Since, therefore, the breadth of the ſeven primary colours, as determined by Sir Iſaac Newton, ſuits the ſimpleſt ratio's poſſible, according to the ſimpleſt law of increaſe poſſible, we ſeem to have from thence an argument both for the doctrine of vibrations in general, and for the particular ratio's of vibrations here alledged. There are two things, he ſays, in this matter, which deſerve particular notice, firſt, that Sir Iſaac Newton's ſpectrum was about ten inches long, and conſequently the breadths of the ſeven primary colours, red, orange, yellow, green, blue, indigo, violet in inches, 1, 25; 0, 75; 1, 33: 1, 66; 1, 66; 1, 11; 2, 22; which magnitudes are ſo conſiderable, that a ſmall error in fixing the limit of a colour does not much affect their mutual ratio's. Secondly, that the limits of the colours were determined in a way that had no dependence on any hypotheſis, and the operation was repeated ſeveral times. However, it may perhaps, he ſays, be worth the time and pains of ſome curious experimenter to examine the breadths of the ſeven primary colours a freſh, and compare them with the hypotheſis here propoſed.*

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* Hartley's Obſervations on Man, Vol. 1, p. 196.

A GENERAL SUMMARY OF THE DOCTRINE CONCERNING LIGHT.

THE more we know of any branch of ſcience, the leſs is the compaſs into which we are able to bring its principles, provided the facts from which they are inferred be numerous. Becauſe, in an advanced ſtate of knowledge, we are able to reduce more of the *particular* into *general* obſervations; whereas, in the infancy of a ſcience, every obſervation is an independent fact; and, in delivering the principles of it, they muſt all be diſtinctly mentioned; ſo that though a *ſelection* may be made, a proper *abridgement* is impoſſible.

Notwithſtanding the vaſt additions that have been made to the ſcience of opticks within the laſt hundred years, a judicious ſummary of the whole will be much ſhorter now, than it would have been a century ago, and yet I hope it is much larger than there will be any neceſſity of making it a century hence; as it may be preſumed that, by that time, a connection will be traced between many facts, which now appear to be unconnected and independent of one another, and therefore require to be recited ſeparately.

To be as conciſe as poſſible in delivering the elementary principles of the doctrine concerning light, I ſhall purpoſely omit the application of them to any of the phenomena of nature, though that be the chief object in all philoſophical inquiries; it being my buſineſs at preſent, barely to recite the knowledge we have acquired of the *laws of nature,* as diſcovered by an attention to thoſe appearances.

The obſervations that were made in the firſt part of the laſt period of this hiſtory will authorize us to take it for granted, that light conſiſts of very minute particles of matter, emitted from luminous bodies. Some of theſe particles, falling upon other bodies, are reflected from them, in an angle equal to that of their incidence, while other particles enter the bodies; being either bent *towards* or *from* a perpendicular to the ſurface of the new medium, if the incidence be oblique to it. In general, rays of light, falling obliquely on any medium, are bent as if they were attracted by it, when it has a greater degree of denſity, or contains more of the inflammable principle, than the medium through which it was tranſmitted to it. More of the rays are reflected when they fall upon a body with a ſmall degree of obliquity to its ſurface, and more of them are tranſmitted, or enter the body, when their incidence is nearer to the perpendicular.

The velocity with which light is emitted and reflected is the ſame; and ſo great, that it paſſes from the ſun to the earth in the ſpace of about eight minutes and twelve ſeconds. The velocity of light is ſuppoſed to be increaſed or diminiſhed by refraction, in proportion to the degree in which the angle of refraction is leſs, or larger than the angle of incidence.

Rays of light, emitted or reflected from bodies, enter the pupil of the eye, and are ſo refracted by the humours of it, as to be united, accurately, or nearly ſo, at the ſurface of the retina, or choroides, and ſo make images of objects, by means of which they are viſible to us.

When a beam of light is bent out of its courſe by refraction, all the rays of which it conſiſts are not equally refracted, but ſome more and others leſs; and the colour which they are diſpoſed to exhibit, is connected invariably with the degree of their refrangibility. The red coloured rays are the leaſt, and the violet the moſt refrangible; and the reſt are more or leſs ſo, in proportion to their nearneſs to theſe, which are the extremes, in the following order; violet, indigo, blue, green, yellow, orange, red.

Theſe colours, when they are ſeparated as much as poſſible, are ſtill contiguous, and all the ſhades of each colour have, likewiſe, their ſeparate and invariable degrees of refrangibility. When they are ſeparated by refraction, the extremes are removed from one another to ſuch a diſtance, that they divide the whole ſpace between them exactly as a muſical chord is divided, in order to ſound the ſeveral notes and half notes of an octave. The mixture of all theſe differently coloured rays, in the proportion in which they cover the ſpace, ſo divided, makes a white, and the abſence of all light is blackneſs.

The degree in which theſe differently coloured rays are ſeparated from one another, is not in proportion to the mean refractive power of the medium, but depends upon the peculiar conſtitution of the ſubſtance by which they are refracted. The diſperſing power of glaſs, into the compoſition of which lead enters, is great in proportion to the mean refraction; and it is little in proportion to it in that glaſs in the compoſition of which there is much alkaline ſalt.

Not only have the different rays of light theſe different properties with reſpect to bodies, ſo as to be more or leſs refracted, or diſperſed by them; but the different ſides of the ſame rays have different properties; for they are differently affected according to the ſides with which they are preſented to Iſland cryſtal. With the ſame degree of incidence, they are refracted in different angles.

Rays of light are not reflected or refracted by impinging on the ſolid parts of bodies, but by virtue of a power which extends to ſome diſtance from the ſurface. They are refracted by a power of attraction, and reflected by a power of repulſion.

At the firſt ſurface of any body, rays of all kinds are promiſcuouſly reflected or tranſmitted; but if the next ſurface be very near to it, ſo that their powers of attraction and repulſion interfere, the rays are affected in ſuch a manner, that, in ſome particular places, thoſe of one colour only are reflected, and thoſe of another colour, chiefly, are tranſmitted; and thoſe places occur alternately for rays of each of the colours, in paſſing from the thinneſt to the thickeſt parts of the medium; ſo that ſeveral ſeries, or orders of colours, will be viſible on the ſurface of the ſame thin tranſparent body.

When rays of light paſs near to any body, ſo as to come within the ſphere of its attraction or repulſion, an *inflection*, that is a partial refraction or reflection, of all the rays takes place; all the kinds being bent either *towards*, or *from* the body; and theſe powers affecting ſome rays more than others, within the ſame diſtance, they are, by this means, alſo, ſeparated from one another;

another ; ſo that coloured ſtreaks appear both within the ſhadow, and on the outſide of it. The red is inflected at the greateſt diſtance from all bodies. There are ſeveral diſtances at which the different rays are differently affected by the powers that are lodged at the ſurfaces of the bodies, to which they make a near approach, ſo that different orders of colours are made by rays which come within different diſtances from the bodies. Three of theſe orders have been obſerved.

Part of the light which falls upon bodies is retained within them, and proceeds no farther. This is more eſpecially the caſe in reſpect to light falling with certain degrees of obliquity on the ſurfaces of bodies. Part of this light is retained ſo looſely by ſome kinds of bodies, that a very ſmall degree of heat makes them emit it again ; but the more heat is applied to them, the ſooner is the light they have imbibed expelled.

Not only do bodies become luminous when they are heated to ſuch a degree that their texture is intirely deſtroyed by it, and their near approach gives us the ſenſation of intenſe burning ; but light is alſo emitted by bodies which are not in the leaſt ſenſibly hot. This has been obſerved with reſpect to many ſubſtances tending to putrefaction, and alſo in phoſphorus.

HINTS OF SOME DESIDERATA IN THE DOCTRINE OF VISION, LIGHT, AND COLOURS.

IF we conſider the infantine ſtate of our knowledge, concerning viſion, light and colours, about a century ago, very great advancements will appear to have been made in this branch of ſcience; and yet a philoſopher of the preſent age has more *deſiderata*, can ſtart more difficulties, and propoſe more new ſubjects of inquiry than even Alhazen or Lord Bacon. The reaſon is that, whenever a new property of any ſubſtance is diſcovered, it appears to have connections with other properties, and other things, of which we could have no idea at all before; and which are, by this means, but imperfectly announced to us. Indeed, every *doubt* implies ſome degree of *knowledge*; and while nature is a field of ſuch amazing, perhaps boundleſs extent, it may be expected that the more knowledge we gain, the more doubts and difficulties we ſhall have; but ſtill, ſince every advance in knowledge is a real and valuable acquiſition to mankind, in conſequence of its enabling us to apply the powers of nature to render our ſituation in life more happy, we have reaſon to rejoice at every new difficulty that is ſtarted; becauſe it informs us that more knowledge, and more advantage are yet unattained, and ſhould ſerve to quicken our diligence in the purſuit of them. Every *deſideratum* is an imperfect *diſcovery*.

Having taken a retroſpective view of the principal diſcoveries that have been made relating to viſion, light, and colours, I ſhall now endeavour to furniſh my reader with the beſt lights that I can afford him, in order to look forward into the yet undiſcovered regions of this branch of ſcience, by reciting the principal articles, concerning which we are the moſt ignorant, and giving ſome hints with reſpect to ſuch things as ſeem to lie moſt contiguous to the diſcoveries that have already been made. As the greateſt reſpect is certainly due to every hint that has been furniſhed us by Sir Iſaac Newton, I ſhall be particularly careful to introduce into this part of my work all thoſe *queries* of his that more immediately relate to this ſubject, beginning with thoſe which contain his conjectures concerning the nature of light, and the manner in which it is emitted from luminous bodies.

Are not, ſays he, groſs bodies and light convertible into one another, and may not bodies receive much of their activity from the particles of light which enter their compoſition ? For all fixed bodies, being heated, emit light as long as they continue ſufficiently hot, and light is ſtopped in bodies as often as its rays ſtrike upon its parts. I know no body, ſays he, leſs apt to ſhine than water ; and yet water, by frequent diſtillations, is changed into fixed earth, as Mr. Boyle has tried ; and then this earth, being able to endure a ſufficient heat, ſhines by heat like other bodies. The changing of bodies into light, and of light into bodies, is very conformable, he ſays, to the courſe of nature, which ſeems to be delighted with tranſmutations.*

This opinion of the convertibility of bodies into light ſeems to be very probable. It remains, however, a doubt whether, as

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* Opticks, p. 349.

M. Muſchenbroeck obſerves, terreſtrial bodies be capable of being turned into light, or light be a ſubſtance *ſui generis*, united to them, and incorporated with them, like air in a fixed ſtate.*

M. Boſcovich ſeems to conſider the matter of light as a principle in the conſtitution of natural bodies, and ſuppoſes that fire is a fermentation between it and the ſulphureous principle, by which the former is thrown off in ſmall particles, in the ſame manner as ſpecific effluvia ariſe from other fermentations.† He alſo advances other reaſons for this hypotheſis, depending upon his theory concerning the fits of eaſy reflexion and tranſmiſſion, mentioned above.§

Sir Iſaac Newton, in his ſeventh query, ſeems to ſuppoſe that the ſtrength and vigour of the action between light and ſulphureous bodies is one reaſon why they take fire more readily, and burn more vehemently than other bodies. But later diſcoveries ſeem to make it probable that this readineſs of what Newton calls *ſulphureous bodies* to take fire, depends upon their containing ſome peculiar, diſtinct, and ſeparable principle in their compoſition, which makes them ſo adapted to the air, that when they have attained a certain degree of heat, they will even grow ſtill hotter by the free admiſſion of it, ſo as readily to propagate the ſame degree of heat among other bodies.

The following queries of Sir Iſaac Newton relating to this ſubject have a great degree of probability.

Do not all fixed bodies, when they are heated beyond a certain degree, emit light, and ſhine? And is not this emiſſion performed by the vibratory motion of their parts; and do not all bodies which abound with terreſtrial parts, and eſpecially with ſulphureous phurcous ones, emit light as often as thoſe parts are ſufficiently agitated, whether that agitation be made by heat, by friction, by percuſſion, by putrefaction, by vital motion, or any other cauſe ?*

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* Muſchenbroeck's Introductio, p. 689. † Theoria, p. 215, 217. § Ib. p. 231.

Is not fire a body heated ſo hot as to emit light copiouſly? For what elſe is red hot iron but fire, and what elſe is a burning coal, than red hot wood ?

Is not flame a vapour, fume, or exhalation heated red hot, that is, ſo hot as to ſhine; for bodies do not flame without emitting a copious fume, and this fume burns in the flame ?

Do not great bodies preſerve their heat the longeſt, their parts heating one another; and may not large, denſe, and fixed bodies, when heated beyond a certain degree, emit light ſo copiouſly, as, by the emiſſion and reaction of its light, and the reflexions and the refractions of its rays within its pores, to grow ſtill hotter, till it comes to a certain period of heat, ſuch as is that of the ſun ? And are not the ſun and fixed ſtars great earths, vehemently hot, the heat of which is preſerved by the cauſes above mentioned, and the parts of which are prevented from fuming away, not only by their fixity, but alſo by the vaſt weight and denſity of their atmoſpheres, which very ſtrongly compreſs them, and condenſe the vapours and exhalations which ariſe from them ?

Whatever be the nature of light, the aſtoniſhing velocity with which it is projected from luminous bodies makes it difficult to conceive in what manner, and by what kind of force it is produced. This M. Muſchenbroeck makes one of his queries. §

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* Newton's Opticks, p. 313.

§ Introductio, p. 690.

He likewiſe aſks what becomes of the light that is emitted from the ſun. Does it, ſays he, return to the ſun, or does it make its way through infinite ſpace? The latter ſeems to be the more probable ſuppoſition. Indeed, we can hardly think of the immenſe ſpace which the light of ſome of the fixed ſtars does actually traverſe, and (as may be inferred from Dr. Bradley's obſervations) without any ſenſible abatement of its velocity, and ſuppoſe that it can return. It is probable, therefore, that light once emitted from luminous bodies never returns to them. Beſides the calculation made by Mr. Michell, to prove the very inconſiderable diminution of the body of the ſun in the ſpace of ſeveral thouſand years, makes the ſuppoſition of the return of the light altogether unneceſſary.*

M. Muſchenbroeck queſtions whether light be projected from all bodies with equal velocity, though he admits that it may be inferred, from the obſervations of Dr. Bradley, that it is emitted from the ſun and the fixed ſtars with equal velocity.§ But ſuppoſing light to be refracted, after paſſing through any medium, by the attracting power of that medium, at its ſurface; ſince the ſame coloured rays, from whatever bodies they are emitted, are equally refracted, it is manifeſt that their original velocity, at their incidence upon the refracting medium, muſt have been the ſame.

From what has been written concerning the phyſical cauſe of the reflexion, refraction, and inflection of light, it has been made ſufficiently evident, that the motion of light in theſe caſes, is not produced in a manner ſimilar to the motion of common projectiles; and philoſophers are now almoſt univerſally agreed, with Sir Iſaac Newton, that theſe effects are produced by certain *powers of attraction and repulſion*, extending beyond the ſurfaces of bodies. But, not content with reſting the matter here (which, indeed, is little more than a confeſſion of our ignorance) Newton proceeds a little farther, and propoſes a conjecture concerning the phyſical cauſe of this attraction and repulſion; but his hypotheſis ſeems to labour under as many difficulties as the hypotheſis of the mechanical production of the motion of light without attraction or repulſion. I ſhall, however, ſubjoin two queries of his upon this ſubject, without entering into any particular diſcuſſion of his hypotheſis in this place.

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* It appears, by calculation, that the greateſt diminution which light emitted from the ſun can receive in its velocity, from attraction towards the ſun, in infinite time, is only about the 500,000th part of the whole.

§ Introductio, p. 690.

Does not the refraction of light ariſe from the different denſity of an etherial medium in different places, the light always receding from the denſer parts of the medium? And is not the denſity of it greater in free and open ſpaces void of air, and other groſs bodies, than within the pores of water, glaſs, cryſtal, gems, and other compact bodies? For when light paſſes through glaſs or cryſtal, and falling very obliquely upon the farther ſurface, is all reflected, the total reflexion ought to ariſe rather from the denſity and vigour of the medium without and beyond the glaſs, than from the rarity and weakneſs of it?

Does not this etherial medium, in paſſing out of water, glaſs, cryſtal, and other compact and denſe bodies, into empty ſpaces, grow denſer and denſer by degrees, and by that means refract the rays of light, not in a point, but by bending them gradually in curve lines? And does not the gradual condenſation of this medium extend to ſome diſtance from the bodies, and thereby cauſe the inflexions of the rays of light, which paſs by the edges of the denſe bodies, at ſome diſtance from the bodies ? *

The cauſe of the *extinction* of light in bodies, is a ſubject of very great difficulty. If light was ſubject to no laws but thoſe of reflexion and refraction, no place into which light was admitted would ever be dark, and the light of the whole univerſe would be continually increaſed. If the ſame cauſe produce all theſe effects, it might be expected that it would ſometimes produce effects of an intermediate kind; in conſequence of which, light might not always be reflected or refracted with equal velocity. M. Bouguer ſuppoſes that the power which abſorbs or extinguiſhes the light is confined to the ſurfaces of bodies, and that it operates chiefly when the rays fall upon it with a certain degree of obliquity, whereas Newton ſuppoſes that a ray is never ſtopped but when it impinges on ſome of the ſolid parts of bodies.

To whatever cauſe it be owing that light is ſtopped in bodies, the queſtion is, What becomes of it ? The light that is again emitted by phoſphoreal ſubſtances is trifling compared with that amazing quantity which is received and abſorbed by all terreſtrial bodies from the light of the ſun. Perhaps, if experiments were made on odours, and other effluvia; which are, in ſome reſpects, ſimilar to light, this ſubject might be in a way of being ſomewhat better underſtood.

Much, it muſt be acknowledged, has been done with reſpect to the colours of *thin plates*; but a complete illuſtration of this difficult ſubject ſeems to be ſtill wanting. The power which is lodged at the firſt ſurface of any body ſeems to affect all the rays equally, reflecting or tranſmitting them promiſcuouſly; and yet if two ſurfaces be ſo near to one another, that their powers can interfere, they do not affect all the rays promiſcuouſly. The effects of theſe powers, therefore, as they diſcover themſelves by this combination, yet remain to be inveſtigated. That this, as well as reflexion and refraction, in general, is effected by powers of attraction and repulſion, extending to ſome diſtance beyond the ſurfaces of bodies, is ſufficiently probable; but what theſe powers are with reſpect to the different kinds of rays is not ſufficiently known; and this is the thing that is neceſſary to the complete elucidation of this ſubject. It might be worth while to give more attention to the obſervations of the Abbé Mazeas, and to compare them with thoſe of Sir Iſaac Newton, I muſt acknowledge, however, that the more I conſider the experiments of the Abbé Mazeas, the leſs I ſee in them that ſeems to differ from the obſervations of Newton. The experiments of the Duc De Chaubres on the very obſcure ſubject of the colours of thick plates have by no means been ſufficiently explained by himſelf; and it ſeems very poſſible to diverſify them much more than he has done, and to conſiderable advantage.

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* Introductio, p. 324.

The doctrine of the *inflection* of light ſeems to be very capable of much farther illuſtration, though the general nature of it be ſufficiently known. The origin of the three coloured ſtreaks, on each ſide of the termination of the ſhadows of bodies, has not been ſatisfactorily traced. Newton conjectured that light bends in its motion, like an eel, in paſſing by the edges of bodies; but will not the ſuppoſition of there being ſeveral definite diſtances, at which there is a reflection or tranſmiſſion of light, account for the ſame appearances rather more naturally? That the ſame particle of light ſhould turn ſeveral times, backwards and forwards, in this caſe, ſeems to be a kind of hypotheſis that one would not chuſe to have recourſe to without neceſſity. As the inflexion of light is capable of being much more diverſified than ſimple reflexion or refraction, it may poſſibly be a clue to lead us to a more perfect knowledge of theſe two other properties.

It ſeems to be hardly poſſible to throw more light on the ſubject of the ordinary *rainbows* that has been done already; but the attempts to explain other atmoſpherical appearances, as halo's, parhelia, and the magnificent phenomena which attend them, conſiſting of luminous circles, ſome of which have their centers in the ſun, and others in the zenith, have been by no means ſatisfactory. The ſame may be ſaid of the additional rows of colours within the common rainbow, obſerved by Dr. Langwith, and thoſe deſcribed by M. Bouguer, as ſeen by himſelf and his companions on the Andes.

The obſervations that have been made on the refractive power of *Iſland cryſtal* demonſtrate the properties of the rays of light to be ſo various, and extraordinary, as cannot but excite our utmoſt attention to the ſubject. It is poſſible, alſo, that the examination of other natural ſubſtances may be a means of diſcovering more properties of light. The obſervations of Mr. Martin may depend upon ſomething of which we are not apprized, or they may reſult from the interference of the cryſtallizations with one another.

The nature of *viſion*, in general, ſeems to be very well underſtood; and there are few phenomena belonging to it that have not been ſatisfactorily explained; but it may be poſſible to add ſomething more deciſive for or againſt the retina being the place where the pencils of rays terminate; or, in other words, being the *ſeat of viſion*, than has yet been advanced by the advocates for either of the opinions: Alſo the doctrine of *ſingle viſion with two eyes* is not without ſome remaining doubts and difficulties.

Sir Iſaac Newton advances ſome probable conjectures concerning the manner in which viſion is performed after the images of objects have been made within the eye, and his hypotheſis has been carried much farther, and illuſtrated at large by Dr. Hartley. What Newton ſays upon the ſubject is contained in the following queries.

Do not the rays of light, in falling upon the bottom of the eye, excite vibrations in the retina, which vibrations, being propagated along the ſolid fibres of the optick nerves into the brain, are the cauſe of viſion?

Do not different rays excite vibrations of different bigneſſes, as different vibrations of the air excite different ſounds, and particularly, do not the moſt refrangible rays excite the ſhorteſt vibrations, and the leaſt refrangible ones the longeſt?

May not the harmony and diſcord of colours ariſe from the proportions of theſe vibrations? For ſome colours, if they be viewed together, are agreeable to one another, as thoſe of gold and indigo, whereas other colours diſagree.

When a man, in the dark, preſſes either corner of his eye with his finger, and turns his eye away from his finger, he will ſee a circle of colours, like thoſe in the feather of a peacock's tail. If the eye and the finger remain quiet, theſe colours vaniſh in a ſecond of time; but if the finger be moved with a quavering motion, they appear again. Do not theſe colours ariſe from ſuch motions excited in the bottom of the eye by the preſſure and motion of the finger, as at other times are excited there by light? When a coal of fire, moved nimbly in the circumference of a circle, gives the appearance of an intire circle of fire, is it not becauſe the motions excited in the bottom of the eye are of a laſting nature, and continue till the coal of fire, in going round, returns to its former place; and muſt not this motion, in order to have this continuance, be of a vibratory nature?

It is poſſible that the magnifying power of *teleſcopes* has been carried as far as the ſtate of the atmoſphere will admit of their being uſeful; but there is ſtill room to make them more commodious, and likewiſe cheaper, eſpecially thoſe of Mr. Dollond's conſtruction. Perhaps, too, it may be poſſible to grind the mirrors of reflecting teleſcopes to figures which, with the ſame aperture of the inſtrument, will admit of a greater magnifying power.

We ſeem to have every thing we can wiſh with reſpect to *microſcopes*, for the purpoſe of viewing tranſparent objects; but it may be poſſible to render the view of opaque objects both more commodious, and more perfect.

Beſides this *general view* of the principal *deſiderata* in the ſcience of viſion, light, and colours, the reader will find hints of others, interſperſed in ſeveral parts of the preceding hiſtory.

ADDITIONS.

SOME of the following articles were furniſhed by books which I was not poſſeſſed of when the pages to which they refer were printed off; others are ſuch as I had either overlooked, or as I had been undetermined about inſerting; but the moſt valuable materials are the communications of my friends, who had ſeen ſome of the printed ſheets, and eſpecially thoſe of Mr. Michell.

§ 1. *A caſe of Indiſtinct Viſion.*

THERE is a remarkable indiſtinctneſs of viſion, when the pencils, by which the image of an object is formed, are very ſmall. This is probably owing to ſome unknown peculiarity in the ſtructure of the eye, as the effect is the very reverſe of what might naturally be expected in the caſe. The *fact*, however, I am informed, is well known, but not having met with any account of it, in the books that have gone through my hands, I deſired my friend Mr. Michell, who mentioned it to me, to give me the beſt deſcription of it that he could recollect; or, if he had an opportunity, to take the trouble of making ſuch experiments, as might enable me to give a pretty ſatisfactory account of it.

From him I learn that the beſt way of making the experiment, ſeems to be by diminiſhing the aperture of a teleſcope; ſince by this means the pencil may be made exceedingly ſmall, without danger of coming within the reach of any errors, from the mutual attraction between the ſides of the hole employed in contracting the pencil, and the rays of light. By ſuch experiments as it was then in his power to make (uſing for objects a card pierced with circular and triangular holes, placed near the flame of a candle, betwixt the candle and his eye, in ſuch a manner, that the flame apparently filled the holes) he found that, when the pencil was only the hundredth part of an inch in diameter, the image was pretty ſenſibly indiſtinct; but when it was only a thouſandth part of an inch in diameter, it became ſo indiſtinct, that he could not certainly diſtinguiſh a ſmall triangular hole from a circular one. With a pencil of the two thouſandth part of an inch in diameter, without the card, he could ſtill barely diſtinguiſh ſome faint traces of the form of the whole flame of the candle, being a large object.

Looking at the ſun (which makes a much better object for this purpoſe than a candle, on account of its much greater brightneſs) with a pencil of about the two thouſandth part of an inch in diameter, it appeared about as indiſtinct as the image of the candle in the ſame circumſtances. He could, indeed, juſt diſtinguiſh whereabouts the image of the ſun was; but it was exceedingly indiſtinct, and ill defined, even more ſo than he could well deſcribe; and yet the brightneſs of the ſun was ſtill greater than that of the moon ſeen by the naked eye, in the proportion of nearly *two* to *one*, (ſuppoſing the pupil of the eye looking at the moon to be the eighth of an inch in diameter) and at leaſt an hundred times greater than when ſeen through an helioſcope, where, yet, there is ſeen an image of the ſun very beautifully and perfectly well defined.

He alſo tried the experiment with a triangular pencil, which contained about double the area of the former, which was circular. With this the ſun appeared very little, if at all better, though a good deal brighter; but he did not perceive any thing of the radiation of the points of the hole which he ſaw before; but the angles were rather blunt.

Though this indiſtinctneſs, ariſing from the ſmallneſs of the pencil, did not begin to be viſible in theſe experiments, till the pencil was reduced to about the one hundredth part of an inch, Mr. Michell ſtrongly ſuſpects that it begins to take place when the pencil is much leſs than the thirtieth of an inch. For he thinks there is very little, if any thing, gained by increaſing the magnifying power of teleſcopes, though there may ſtill be light ſufficient, when, in order to do this, the pencil is farther diminiſhed than what has been juſt mentioned, but the teleſcope he uſed was not a very diſtinct one, with its whole pencil.

§ 2. *Of the force by which light is emitted from luminous bodies, and of that which, being lodged at the ſurfaces of bodies, is the cauſe of what is generally called their impenetrability.*

THE following computations on theſe very curious ſubjects were communicated to me by Mr. Michell, and they add conſiderably to the probability of his and M. Boſcovich's hypotheſis, concerning the *penetrability of matter*.

The ſemidiameter of the ſun, ſeen from the earth at their mean diſtance, ſubtending an angle of 16′, 3″, will be contained in that diſtance 214,19 times; and therefore, ſince the ſquares of the periodic times, in which bodies would revolve about a central body, are proportional to the cubes of their reſpective diſtances, it will be found, by computation, that a body would make one revolution about the ſun, at the diſtance of a ſemidiameter from his centre, (that is, at his ſurface) in $2^{h},47',,47''$ or $10067''$. Let us now ſuppoſe the diameter of the ſun to be equal to 110 diameters of the earth, and the diameter of the earth to be equal to 7.915 ſtatute miles or 41.791.200 feet: then will the diameter of the ſun be equal to 4.597.032.000 feet, and his circumference to about 14.441.915.000 feet, and the ſpace a body, revolving in a circle at his ſurface, would move through in one ſecond of time, to 1.434.580 feet. And, as this is a mean proportional between his diameter and the ſpace, through which a body would fall in one ſecond of time, by a force of gravity equal to that, by which the revolving body is retained in it's orbit, that ſpace will be found to be 448 feet nearly. This divided by the ſpace a body falls through at the ſurface of the earth, in the ſame time, viz. about 16 feet 1 inch, gives the proportion of the force of gravity at the ſurface of the ſun, to that at the ſurface of the earth; which proportion is therefore very nearly 28 to 1. N. B. This proportion differs a little from that given by Sir Iſaac Newton in his Principia, both becauſe the apparent diameter of the ſun has, ſince his time, by better inſtruments, been found ſomewhat leſs than he made it, and it's diſtance conſiderably greater.

Suppoſing then the force of gravity at the ſurface of the ſun to exceed that at the ſurface of the earth in the proportion of 28 to 1, he endeavoured to make ſome kind of compariſon between this force, and that, by which light is emitted, reflected, refracted, &c.

Now in order to find the velocity, which will be produced by a force acting through any given diſtance, according to any law whatſoever: If we ſuppoſe the given diſtance to be repreſented by a right line, and to this line be erected every where perpendiculars proportional to the centripetal force at each place of the line reſpectively, the velocity ultimately produced will be as the ſquare root of the area deſcribed by theſe lines; and hence, *e converſo*, any ultimately acquired velocity will require a force to produce it, that would be repreſented by an area proportional to the ſquare of that velocity. See Newton's Pr. B. 1. Prop. 39. By the help of this Prop. we ſhall find, that the velocity acquired by a body falling from an infinite height towards a centre, by a law of gravity decreaſing inverſely as the ſquares of the diſtances from that centre, will, at any given diſtance from thence, be the ſame, which it would have acquired by falling through the remaining diſtance to the centre, with an uniform force of gravity equal to that, which obtained at the ſaid given diſtance.

The velocity therefore, which a body would acquire, by falling from an infinite height to the ſurface of the ſun, would be the ſame as that, which it would acquire by falling from his ſurface to his centre, by an uniform force of gravity equal to that at his ſurface, that is, 28 times the force of gravity at the ſurface of the earth: But this velocity according to what has been ſaid above, will be to the velocity acquired by falling 16 feet at the ſurface of the earth (that is about 32 feet *per* ſecond) as the ſquare root of 28 (the force of gravity at the ſurface of the ſun) multiplied into 2.298.516.000 (the number of feet in his ſemidiameter)

diameter) to the ſquare root of 1 (the force of gravity at the ſurface of the earth) multiplied into 16; that is as 253,690 to 4, or as 63.422, 5 to 1, which multiplied into 32 gives 2.029.520 the number of feet per ſecond nearly, for the velocity ſought. This is the velocity, that a comet revolving in a parabolic orbit would have at the ſurface of the ſun, and is to the velocity of a body revolving in a circle at the ſame place, as the ſquare root of 2 to 1.

The velocity of light is greater than this, in the proportion of about 497 to 1, as will appear from multiplying the ſemidiameter of the ſun above given by 214,19 for the diſtance from thence to the earth, and dividing it by 488 the number of ſeconds, that light takes up in travelling through that ſpace, which therefore will give the diſtance travelled through by it in one ſecond of time, equal to 1.008.850.700 feet, or about 191.000 ſtatute miles. Since then, as was ſaid above, the areas repreſenting the forces, that would produce theſe velocities, are as their ſquares, the area repreſenting the force, which produces the velocity of light, muſt be greater than the area repreſenting the force, which would produce a velocity equal to that acquired by falling from an infinite height to the ſurface of the ſun, in the proportion of the ſquare of 497 or 247.009 to 1.

From the vaſt diſproportion between theſe forces it appears what an extremely little diminution the velocity of light can ſuffer by the attraction of the ſun; the velocity at it's firſt iſſuing from thence, being to the velocity diminiſhed as much as it poſſibly can be by this cauſe, in the proportion of the ſquare root of 247.009 to the ſquare root of 247.008; ſo that the diminution of the velocity, on this account, cannot amount to more than one 492.032d part of the whole; and of this diminution, there will remain only one 45.878th [$214,19^2$] part to take place, after the ſun's light has arrived at the diſtance of the earth.

In order to compute what proportion the force, by which the particles of light are propelled, bears to the force of gravity, we ought to know through what diſtance this force acts. That this diſtance is extremely ſmall is very manifeſt: for computation ſake he aſſumed it equal to the hundredth part of an inch, though he apprehends that this is far larger than it is in reality; but it is no eaſy matter to determine the true quantity of it; and he was therefore willing to ſet it ſufficiently without what he thought could poſſibly be it's utmoſt limits. Now ſuppoſing this to be the caſe, and that it acts with an uniform force throughout this diſtance; then will this force be to the common force of gravity at the ſurface of the earth, as the ſemidiameter of the ſun [2.298.516.000 feet] to the $\frac{1}{100}$th of an inch; as 247.009 to 1; and as 28 to 1 conjointly, that is very nearly as 19.000.000.000.000.000.000 to 1.

In as much greater or ſmaller diſtance than that of the hundredth part of an inch, the force producing the velocity of the particles of light is exerted, ſo much leſs or greater proportion muſt this force bear to that of gravity at the ſurface of the earth. And whatever force this velocity requires to produce it, the ſame muſt it require to ſtop it. But the force, by which the rays of light are refracted in paſſing out of a rarer medium into a denſer, or at leaſt that force, by which they are reflected from the back ſurface of a denſer medium contiguous to a rarer, muſt neceſſarily, he thought, be ſtill greater; and it can hardly be ſuppoſed,

poſed, that, in the latter caſe at leaſt, the whole of this force is not exerted within as narrow a compaſs, as the 70 or 80.000th part of an inch; but it would be going much too far to enter into a more minute inquiry relative to this intricate and difficult ſubject at preſent.

Upon this moſt aſtoniſhing force, by which light is thrown off off from bodies, or what is equivalent to it, the reſiſtance it meets with at their ſurfaces, whilſt it is endeavouring to enter them, he makes an obſervation, which at the ſame time, that it ſerves to ſhew how the effect of a force not improbably the ſame with this, may very well have been miſtaken for abſolute impenetrability, will alſo ſerve to convey to the imagination ſome idea of the vaſtneſs of it, which a long row of figures, or the words nineteen trillion times the force of common gravity can only do in a very confuſed and inadequate manner. A globe therefore of 59.438 feet, or ſomething more than eleven ſtatute miles and a quarter in diameter, compoſed of any uniform materials, and reſting with its whole weight upon a ſurface of only one ſquare inch, would but juſt be able to overcome a reſiſtance equal to that above mentioned; for nineteen trillion times the hundredth part of a cubic inch of matter (which is all that could come at one time within the ſphere of action of a force extending only to the hundredth part of an inch) would form a globe of that ſize. And the ſize of this globe will be the ſame, whatever be the law of this force, and to whatever diſtance it be ſuppoſed to extend, ſince the leſs diſtance it extends to, the greater it muſt be, and in the ſame proportion, and vice verſâ.

§ 3. *Obſervations relating to* the ſeat of Viſion.

THE dimenſions and preciſe form of the ſpot in the eye in which there is no viſion (ſee p. 192) were more accurately calculated by Daniel Bernoulli, in the following manner. He placed a piece of money O, fig. 60*, upon the floor, and then ſhutting one of his eyes, and making a pendulum to ſwing, ſo that the extremity of it might he nearly in the line AO, he obſerved at what place C, it began to be inviſible, and where it again emerged into view, at A. Raiſing the pendulum higher and lower, he found other points, as H,N,P,G,B, at which it began to be inviſible, and others, as M,L,E,A, at which it began to be viſible again; and, drawing a curve through them, he found that it was elliptical, and, with reſpect to his own eye, the dimenſions of it were as follows, OC was 23, AC 10, BD 3, DH 13, and EG 14; ſo that, the center being at F, the greater axis was to the leſs as 8 to 7.*

From theſe *data* the plane on which the figure was drawn being obliquely ſituated with reſpect to the eye, he found that the place in the eye that correſponded to it was a circle, the diameter of which was a ſeventh part of the diameter of the eye, the center of it being 27 parts of the diameter from the point oppoſite to the pupil, a little above the middle. He concludes with obſerving that, in order that this ſpace, in which there is no viſion, may be as ſmall as poſſible, it was neceſſary that the nerve ſhould enter the eye perpendicularly, and that both this

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* Com. Petrop. Vol. 1. p. 315.

and,

end, and alſo its entering the eye at a diſtance from its axis, are gained by the particular manner in which the two optic nerves unite and become ſeparate again, by croſſing one another.

In favour of one of the obſervations of Mr. Michell, concerning the uſe of the choroides in viſion, I would obſerve that Aquapendonte mentions the caſe of a perſon at Piſa, who could ſee very well in the night, but very little, or none at all, in the day time.* This is alſo ſaid to be the caſe with thoſe white people among the blacks of Africa, and the inhabitants of the Iſthmus of America, who, from this circumſtance, are called *moon eyed.* I have met with no account of the diſſection of the eyes of any of theſe people, but ſhould think it probable that their choroides will be found not to be of a dark colour, as it is in others of the human ſpecies, but white, or light coloured, as in thoſe animals which have moſt occaſion for their eyes in the night.

Since the ſection concerning the proper ſeat of viſion was printed off, ſeveral conſiderations in favour of the *retina* have occurred to me, in reading upon the ſubject, and others have been pointed out to me by Mr. Hey, an ingenious ſurgeon and anatomiſt in Leeds.

Dr. Porterfield obſerves, that the reaſon why there is no viſion at the entrance of the optic nerve into the eye, may be *that it wants* that *ſoftneſs* and *delicacy*, which it has, when it is expanded upon the choroides; and that, in thoſe animals in which that nerve is inſerted in the axis of the eye, it is obſerved to be equally delicate, and therefore probably equally ſenſible, in that place as in any other part of the retina.* I am informed that, in general, the nerves, when conſtringed by their coats, have but little ſenſibility, in compariſon of what they are endued with when they are diveſted of them, and unfolded, in a ſoft and pulpy ſubſtance.

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* De Viſione, p. 46.

Haller obſerves that the choroides cannot be the univerſal inſtrument of viſion, becauſe that ſometimes in men and birds, but eſpecially in fiſhes, it is covered internally with a *black mucus*, through which the rays cannot penetrate.† This writer ſpeaks of a *fibrous membrane* in the retina diſtinct from its *pulpy ſubſtance*. On theſe fibres, he conjectures, that the images of objects are painted.

M. De la Hire's argument in favour of the retina, from the analogy of the ſenſes, is much ſtrengthened by conſidering that the retina is a large nervous apparatus, immediately expoſed to the impreſſion of light; whereas the choroides receives but a ſlender ſupply of nerves, in common with the ſclerotica, the conjunctiva, and the eyelids, and that its nerves are much leſs expoſed to the light than the naked fibres of the optic nerve. Indeed, from anatomical conſiderations, one might imagine that any other part of the body was as ſenſible of the impreſſion of light, as the choroides.

That the optic nerve is of principal uſe in viſion is farther probable from ſeveral phenomena attending ſome of the diſeaſes in which the ſight is affected. When an amauroſis has affected one eye only, the optic nerve of that eye has been found manifeſtly altered from its ſound ſtate. I myſelf was preſent when Mr. Hey examined the brain of a young girl, who had been blind of one eye, and ſaw that the optic nerve belonging to it was conſiderably ſmaller than the other; and he informed me that, upon cutting into it, he found it to be much harder, and cinericious. Morgagni, indeed, mentions two caſes, in one of which he found the optic nerves ſmaller than uſual, and of a cinericious colour, when, upon inquiry, he was informed that the perſon had not been blind, though there might have been ſome defect in the ſight of one of the eyes. In the other caſe, only one of the optic nerves was affected in that manner, and the eye itſelf was in other reſpects very perfect. Here, alſo, he was expreſly told that the perſon was not blind of that eye; but it appears that he himſelf had not been acquainted with the perſons whom he diſſected; and there have been many caſes of perſons being blind of one eye without knowing it themſelves for a conſiderable time.

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* Vol. 2, p. 254.

† Phyſiologia, Vol. 5, p. 474.

Moreover, as the optic nerve is ſolely ſpent in forming the retina, ſo no function of the eye, not immediately ſubſervient to viſion, is affected by an amauroſis. On the contrary, thoſe nerves which go to the choroides are found to retain, in this diſeaſe, their natural influence. The iris will contract in a recent gutta ſerena of one eye, if the other remains ſound, and is ſuddenly expoſed to a ſtrong light. The ſclerotis, conjunctiva, and eyelids, which receive their nerves from the ſame branches as the choroides, retain their ſenſibility in this diſorder.

The manner in which perſons recover from an amauroſis, favours the ſuppoſition of the ſeat of viſion being in the retina; ſince thoſe parts which are the moſt diſtant from the inſertion of the nerve recover their ſenſibility the ſooneſt, being, in thoſe places, the moſt pulpy and ſofteſt; whereas there is no reaſon to think that there is any difference in this reſpect in the different parts of the choroides. Mr. Hey has been repeatedly informed, by perſons labouring under an imperfect amauroſis, or gutta ſerena, that they could not, when looking at any object with one eye, ſee it ſo diſtinctly, when it was placed directly oppoſite to the pupil, as when it was ſituated ſomewhat obliquely. And thoſe perſons whom he had known to recover from a perfect amauroſis firſt diſcovered the objects whoſe images fell upon that part of the retina which is at the greateſt diſtance from the optic nerve.

I ſhall conclude theſe remarks with obſerving that, if the retina be as tranſparent, as it is generally repreſented to be, ſo that the termination of the pencils muſt neceſſarily be either upon the choroides, or ſome other opaque ſubſtance interpoſed between it and the retina, the action and reaction occaſioned by the rays of light being at the common ſurface of this body and the retina, both theſe mediums (ſuppoſing them to be equally ſenſible to the impreſſion of light) may be equally affected; but the retina, being naturally much more ſenſible to this kind of impreſſion, may be the only inſtrument by which the ſenſation is conveyed to the brain, though the choroides, or the black ſubſtance with which it is ſometimes lined, may alſo be abſolutely neceſſary for the purpoſe of viſion. Indeed, when the reflexion of light is made at the common boundary of any two mediums, it is with no propriety that this effect is aſcribed to one of them rather than the other, and the ſtrongeſt reflexions are often made back into the denſeſt mediums, when they have been contiguous to the rareſt, or even to a vacuum. This is not far from the hypotheſis of M. De la Hire, and will completely account for the intire defect of viſion at the inſertion of the optic nerve.

§ 4. Obſervations

§ 4. Obſervations relating to *Optical Inſtruments.*

IT was a glaſs, or rather a precious ſtone, that Socrates called for, to melt the wax. See the *Nubes* of Ariſtophanes. His ſcoliaſt ſays it was a thick and round glaſs, Pliny, Lib. 36, 37, ſpeaks of globes of glaſs and of cryſtal, which being expoſed to the ſun, would burn cloth, or the fleſh of diſeaſed perſons, which they wanted to cauterize; and Lactantius, in his treatiſe *De ira Dei* ſays, that globes of glaſs, filled even with cold water, would kindle fire, if they were held in the ſun. Conſidering that the antients were in poſſeſſion of theſe facts, it is ſurpriſing that they made no more uſe of them; but M. De la Hire conjectures that their exceedingly lame theories of light and viſion were the cauſe of it; as they could ſee no ſort of connection between them and theſe effects. * But then it may be wondered, that theſe facts ſhould not have led them to call in queſtion, and reject an hypotheſis, upon which they were altogether unexplicable.

Dr. Hooke makes ſome remarks on the diſſertation of M. De la Hire, and concludes that, upon the whole, he is of opinion that the antients were wholly ignorant of burning glaſſes, except ſpheres; and that lenſes were by no means known to them.†

Mr. Diggs is ſaid to have hit upon the conſtruction of a teleſcope, as, Dr. Hooke ſays, is teſtified by his ſon; who printed ſome of his father's works after his death; but, as Dr. Hooke ſays, that B. Porta made the ſame diſcovery, it is very probable that there is ſome miſtake in the caſe; and that what Mr. Diggs did make was not properly a teleſcope. §

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* Ac. Par. 1708, p. 140.

† Hooke's Experiments, by Derham, p. 348.

§ Ib. p. 258.

J. G. Leutman has deſcribed an ingenious method, by which one image is made to appear metamorphoſed into another, by looking at it through a tube, furniſhed with different reflecting planes; but for the particulars, I muſt refer my reader to the memoirs of the Imperial Academy at Peterſburgh, Vol. 4, p. 202, or to the account which the author there refers to, as publiſhed by him, in high Dutch, and printed at Wittemburgh, in 1719.

I am apprized by Mr. Michell, that the objection to Caſſegrain's teleſcope, mentioned p. 347, is not the true one; ſince this form admits as much light, in proportion to its magnifying power, with the ſame aperture, as the Gregorian. From what he has heard Mr. Short ſay upon the ſubject, he judges that his principal objection to Caſſegrain's conſtruction was its repreſenting hills (in the moon, for inſtance) as hollows, and *vice verſa.* He did not, indeed, think them, in other reſpects, quite ſo diſtinct; but Mr. Michell ſuppoſes that the reaſon was a want of equal perfection in the conſtruction of them; for it is almoſt impoſſible, in the common way of working, to make them as perfect as thoſe of the Gregorian form; nor can it, perhaps, be done at all, but certainly, not without a great deal of management and addreſs.

In the Gregorian form, he ſays, the great ſpeculum ought to be exactly parabolic, and the little ſpeculum elliptic, having one of its foci in the focus of the great ſpeculum, and the other in the place to which the rays, after being reflected from it, would again be collected to their ſecond focus, if not intercepted by the firſt eye glaſs. In the Caſſegrain form, the great ſpeculum ought, as before, to be exactly parabolic, and the little ſpeculum

lum hyperbolic; having its two conjugate foci in the ſame places with the foci of the former elliptical one; the elliptic little ſpeculum, in the Gregorian and the hyperbolic one in the Caſſegrain teleſcope, differing exceedingly little from a parabolic figure, which would lie in the middle way between them.

If, in either teleſcope, either of the ſpeculums ſhould vary a little from theſe forms, the errors ariſing from ſuch a variation may be corrected by a proper alteration in the form of the other ſpeculum, which will be effected by altering it the contrary way in the Gregorian form, and the ſame way in the Caſſegrain; and this alteration, if it is to be made in the little ſpeculum, in order to correct the errors of the greater one, muſt be as much greater than it muſt have been if it had been made in the greater one, to correct a like error in the leſs, as the focal diſtance of the greater ſpeculum is greater than that of the leſs. Now, in grinding all ſorts of ſpeculums, lenſes, &c. it unavoidably happens, from the nature of the materials, and the common manner of working them, that all hollow ſurfaces vary from the ſpherical towards a parabolic form, or may even go beyond it, as Mr. Michell knew from experience; and all convex ſurfaces vary from the ſpherical form towards that of a portion of an oblate ſpheroid, next to one of its poles. Theſe effects, he thinks, he can venture to ſay, take place in a greater or leſs degree, without any exception, the edges of the ſpeculums, lenſes, &c. wearing away a little faſter than their middles.

Hence, the variations from the ſpherical form, which neceſſarily ariſe from the manner of working them, in both the ſpeculums of the Gregorian teleſcope, lie the right way to correct the errors of theſe ſpeculums, ſuch as they are uſually ſuppoſed to be in theory; and if they go far enough, and not too far, will, perhaps, perfectly correct them. But in the Caſſegrain form, in the little ſpeculum they lie the wrong way, and tend to increaſe inſtead of diminiſhing or correcting them. For, whereas the little ſpeculum ought, as was ſaid before, to be of an hyperbolic form, in which the edges are of leſs curvature than the vertex, and lie without the ſphere; it will, on the contrary, from this cauſe, be of a form in which the edges are of greater curvature than the vertex, and lie within the ſphere. And it will be extremely difficult to correct thoſe errors of the little ſpeculum by altering the form of the great one, which, in order to do this, muſt be of a conſiderably leſs curvature at the edges than a parabola; and if it ſhould require to be reduced as far as to the ſpherical form, or even ſtill farther, for this purpoſe, it will then be as impracticable as to grind the little ſpeculum originally of an hyperbolic form.

On account of the tendency of concave ſpeculums to recede from the ſpherical towards the parabolic or hyperbolic form, it is, Mr. Michell ſays, practically poſſible, to make reflecting teleſcopes more diſtinct, with an aperture of a certain ſize (according to the particular manner of grinding and poliſhing the ſpeculums) than if that aperture was ſmaller; for a certain degree of wearing away at the edges, which would only make the larger aperture parabolical, will make the ſmaller one hyperbolical. This, he ſays, is contrary to what is generally ſuppoſed by thoſe who imagine the ſpeculums are ground into a ſpherical form, in which caſe the ſmaller their apertures were, the more diſtinct they would be. The late Mr. Short, he ſays, knew this very well from experience, and availed himſelf of it accordingly.

§ 5. *Miſcellaneous Obſervations.*

THE Solar Microſcope;* (ſee p. 744) has been introduced into the ſmall, and *portable*, as well as the large *Camera Obſcura*; and if the image be received upon a piece of half ground glaſs, ſhaded from the light of the ſun, it will be ſufficiently viſible. The moſt particular deſcription that I have ſeen of this contrivance, is that of M. Brander, in a pamphlet intitled, *Kurtze Beſchribung, &c. eines Sonnen Microſcops*, printed at Auſburg, 1769. An account of it may alſo be ſeen in Martin's Gentleman's Philoſophy, Vol. 2, p. 249. Who was the author of this improvement I do not know.

That the pupil contracts by a ſtrong light was no diſcovery of Father Paul (ſee p. 40.) It was that of Achillinus, was known to the Arabians, as Rhazes and Avicenna. Galen alſo mentions it.§

Mr. Robins obſerves, that, though he has no doubt of Dr. Brook Taylor having invented his excellent method of perſpective (ſee p. 92) yet that the ſame method was publiſhed by Guido Ubaldi, in his *Perſpective* printed at Peſaro, in 1600. In this treatiſe, Mr. Robins ſays, that this method is delivered very clearly, and confirmed by moſt excellent demonſtrations. In the laſt book Ubaldi applies his method to the delineating the ſcenes of a theatre, and in this, with reſpect to the practice, he is followed by Signor Sabatellini, in his *Practica di fabricar ſcene*, of which there was a new edition at Ravenna in 1638; and to this was added a ſecond book, containing a deſcription of the machines uſed for producing the ſudden changes in the decorations of the ſtage.*

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* This article ought to have been inſerted in the laſt ſection, but was overlooked.

§ Haller's Phyſiologia, Vol. 5, p. 374.

It was not James Gregory, as in p. 135, but David, the nephew of James, who wrote the Elements of Dioptrics and Catoptrics.

From the conſideration of the ſucceſſive propagation of light, (ſee p. 405)M. L. Euler deduces a new argument in favour of the Copernican ſyſtem. For he ſhews that though no ſenſible difference is made in the obſerved places of the heavenly bodies, in conſequence of the diurnal motion of the earth, a very conſiderable one would be the conſequence of ſo very rapid a motion of the fixed ſtars as the Ptolemaic hypotheſis ſuppoſes. He alſo very elaborately calculates the allowance which is to be made for the obſerved places of the ſtars and planets, on the ſuppoſition that light is propagated from the ſun to the earth in eight minutes. Notwithſtanding M. Euler ſuppoſes that light conſiſts in the vibration of an etherial medium, he makes his calculations upon the Newtonian hypotheſis, as well as his own, ſhewing in what reſpects they would make his concluſions different; but this difference, he ſays, is too ſmall to determine the queſtion.§

Mr. Michell obſerves, (ſee p. 403) that if the difference of colour depended upon the different velocities of the differently coloured rays, a ſtar muſt change its place on account of the aberration a 77th more for the deepeſt blue rays than the red ones, according to Mr. Melville, and a 45th part more accordink to M. de Courtivron; and conſequently a ſtar placed in the pole of the ecliptic, whoſe mean aberration is about 20″, ought to appear in the form of the ſpectrum, formed by a priſm, of $\frac{20''}{70}$ or $17'''$ in length, according to the former hypotheſis; or of $\frac{20''}{45}$ or $27'''$ nearly according to the latter. This is a quantity which would be ſufficiently viſible in teleſcopes of pretty high magnifying powers, but no ſuch appearance has yet been obſerved. This obſervation Mr. Michell ſays is not his own, but he could not recollect where he met with it.

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* Robins's Tracts, Vol. 1. Appendix, p. 322.

§ Com. Petrop. Vol. 2, p. 174, 193.

M. Boſcovich propoſes what appeared to him a very great difficulty in accounting for the appearance of the ſecondary rainbow (ſee p. 588) which is made by two reflexions of the rays of light, intervening between their entrance into a drop of water and their exit out of it. Since the chord which the rays of light traverſe, before and after their reflexion, are equal, he ſays that the number of fits of eaſy reflexion and tranſmiſſion, in theſe equal ſpaces, will be exactly equal; and ſince it appears, from fact, that they muſt have been in a fit of tranſmiſſion at their entrance into the drop, and in a fit of reflexion when they had traverſed the firſt chord, they ought, conſequently, to be in a fit of tranſmiſſion at the end of the ſecond chord, in which caſe, being all tranſmitted in that place, none at all would be reflected to form the ſecondary rainbow. The ſolution which M. Boſcovich propoſes is, that, by the agitation of the air, a ſmall alteration is made in the form of the drop, making the ſpaces before and after reflexion a little different, but ſo little, as not to make any ſenſible change in the diameter of the bow.* But this reaſoning goes upon the ſuppoſition that all the rays of light, are not promiſcuouſly reflected or refracted at the ſurfaces of all mediums, when the interval between any two of them is conſiderable; whereas nothing is more evident than that, in fact, they are ſo; it being only where the interval between two mediums is extremely ſmall, that, at leaſt, a ſenſible ſeparation of the different colours is made, thoſe of one kind, chiefly, being reflected, and thoſe of another tranſmitted.

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* Benvenuti De Lumine, p. 135.

M. Boſcovich and M. Benvenuti have, endeavoured to explain the colours of both thin and thick plates (ſee p. 498) in ſeveral very elaborate diſquiſitions, which were publiſhed in the name of the latter of theſe gentlemen. But as they take no notice of any facts beſides thoſe which were obſerved by Sir Iſaac Newton, and ſome of the obſervations which have been made ſince his time, ſhew that he had no knowledge of ſome important circumſtances relating to theſe appearances, I ſhall not trouble my reader with any of their intricate computations on the ſubject. Concerning M. Boſcovich's general theory of light, and the cauſe of its eaſy reflexion and tranſmiſſion, I imagine that I have already given ſufficient ſatisfaction; and his reaſoning concerning the rainbow ſhews that he was miſtaken with reſpect to one of the moſt important particulars relating to theſe appearances.

Mr. Hey informs me, that a patient of his, who had recovered from a perfect amauroſis, told him that the firſt time ſhe diſcovered the fire, it ſeemed to look *blue*; but he had no opportunity of knowing whether this be an uſual phenomenon or not. This obſervation, however, agrees with thoſe of M. De la Hire and others (ſee p. 634) that the laſt and weakeſt impreſſion made by the images of objects intenſely luminous is blue.

Much has been ſaid on the ſubject of that experiment of Sir Iſaac Newton's, which Mr. Dollond found not to anſwer, and it is thought very extraordinary, that a man of Sir Iſaac's accurate attention ſhould overlook a circumſtance, the effect of which now appears to be ſo conſiderable. But it has happily occured to my friend Mr. Michell, ſince the chapter on that ſubject was printed off, that, as Sir Iſaac Newton obſerves he uſed to put *Saccharum Saturni* into his water, to increaſe its refractive power, the *lead*, even in this form, might increaſe the diſſipative refraction, as it does in the compoſition of glaſs ; and if ſo, that this would account for Newton's not finding the diſſipative power of water leſs than that of his glaſs-priſms, which he, otherwiſe, ought to have done, if he had tried the experiment as he ſaid he did.

Accordingly he included a priſm of glaſs in water, as highly impregnated with *Saccharum Saturni*, as it would bear, the proportion of Saccharum to water, being about as 5 to 11. When the image ſeen through the water, ſo impregnated, and a glaſs priſm, was in it's natural place, it ſtill was coloured, though very little. He thought not more than a fourth part as much, as when ſeen through plain water, and the priſm in its natural place ; ſo that he had no doubt but that, if, his priſm had had a little leſs of the diſperſive power, its errors would have been perfectly corrected; and, as it diſperſed the rays very much, he was ſatisfied that, if it had been made of crown glaſs, the colouring would have lain the contrary way. The refracting angle of his priſm was 58° ; that of the plain water (which brought the image into it's natural place, and in which the glaſs priſm was included) 46°, the contrary way ; and that of the water, impregnated with the *ſaccharum*, 36°, 30′.

Afterwards he put two priſms together, one of common water, and the other of water impregnated with *ſaccharum ſaturni*, the refracting angles lying the contrary way from each other; and of ſuch ſizes, as to ſhew the object in its true place. The refracting angle of the impregnated water was 90°, and it was included between two water priſms, each of which had a refracting angle turned the contrary way, of 50°, 45′, ſo that the whole together made a priſm with a refracting angle of 11°, 30′. This was the angle he had computed to be juſt ſufficient to correct the mean refraction of the rays, in the other priſm, when included within it, with the refracting angle turned the contrary way; and it did ſo as nearly as could poſſibly be expected, when allowance was made for the unavoidable errors, that muſt happen in the rough way in which he had the means of trying it; for it did not differ from the truth by more than about a quarter of a degree. This, however, he corrected till he brought the object into the ſame place, ſeen either by the naked eye, or through the priſm.

Under theſe circumſtances the object was manifeſtly coloured by the ſaccharum ſaturni and water; but not ſo much, as he expected, and whether enough to correct the errors, that would ariſe from a glaſs priſm or no, he could not tell, till it ſhould be tried in a more correct manner, and with glaſs, that has none, or at leaſt very little lead in it. It would, however, he ſays, certainly tend to do it, if it was not ſufficient to do it completely; and a little yellowiſh caſt in the liquor, would help, by abſorbing the blue rays, to diminiſh the remaining effect of the glaſs-priſm. The ſines of incidence and reflexion for his glaſs priſm were as 94, 57 to 60 (93 to 60 being about the uſual proportion for white flint glaſs, and 91 to 60 the proportion for crown) which made him think there was a great deal of lead in it. The ſines of incidence and reflexion for the ſaccharum ſaturni and water were as 82 to 60 very nearly.

I ſhall conclude theſe *miſcellaneous obſervations* with mentioning two circumſtances relating to the light of candles; one of which every perſon muſt have obſerved, and the other was ſhewn to me by Dr. Franklin. The loweſt part of the flame of a candle is always blue; and when the flame is ſufficiently elongated, ſo as to be juſt ready to ſmoke, the tip is always red. Doth not this ſtrengthen the conjecture, that red light conſiſts of larger particles than blue? For next to the red particles, are thoſe of the groſs vapour which is not ignited.

Dr. Franklin ſhewed me that the flames of two candles joined give a much ſtronger light than both of them ſeparate; as is made very evident by a perſon holding the two candles near his face, firſt ſeparate, and then joined in one. For immediately upon the junction, his face will be obſerved to be much more ſtrongly illuminated than it was before. It is conjectured that the union of the two flames produces a greater degree of *heat*, and that this cauſe a farther attenuation of the vapour, and a more copious emiſſion of the particles of which light conſiſts.

AN

AN EXPLANATION OF TECHNICAL TERMS MADE USE OF TO EXPLAIN THE DOCTRINE OF VISION, LIGHT, AND COLOURS.

ANAMORPHOSIS, a diſtorted image of an object, which is ſaid to be *reformed*, when it is made to appear like the object, by means of a mirror of any particular form.

Angle of incidence. The angle which a ray of light makes with a perpendicular to that point of the ſurface of any medium on which it falls; though it is ſometimes underſtood of the angle which it makes with the ſurface itſelf, and eſpecially, by M. Bouguer in his Traité d'Optique. Thus in general CDE, Fig. 1, is called the angle of incidence, but ſometimes the angle, CDA.

Angle of refraction now generally means the angle which a ray of light, refracted by any medium, makes with a perpendicular to that point of the ſurface on which it was incident; but has ſometimes been underſtood of the angle which it makes with the ſurface of the refracting medium itſelf. Thus IDH, Fig. 1, is properly the angle of refraction, though BDH has been ſo termed.

Bolognian Phoſphorus, a foſſil ſubſtance, found in the neighbourhood of Bologna in Italy, which, after calcination, is remarkable for its property of imbibing and emitting the light to which it has been expoſed.

Camera Obſcura, a darkened room, or place, into which a beam of light may be admitted, through a ſmall aperture.

Cauſtic Curves, curves to which rays of light, reflected or refracted by any medium bounded by curved ſurfaces, are tangents.

Corona, ſee *Halo*.

Diſperſion, the ſeparation of the differently coloured rays from one another, which was found by Mr. Dollond not to depend upon their mean refraction; that is the refraction of thoſe rays which are at an equal diſtance from the moſt and the leaſt refrangible.

Focus, a point in which any number of rays, after being reflected or refracted, meet.

Halo, or *Corona*, a luminous circle, generally coloured, appearing ſometimes round the ſun, moon, or the largeſt ſtars and planets.

Ignis Fatuus, a luminous appearance, which is ſometimes viſible in the fields in a dark night, and vulgarly called *Will with a wiſp*.

Image, A figure in the form of any object, made by the rays of light iſſuing from the ſeveral points of it, and meeting in ſo many other points, either at the bottom of the eye, on any other ground, or in any tranſparent medium, where there is no ſurface to reflect them. Thus we are ſaid to ſee all objects by means of their images, formed in the eye.

Inflected rays, thoſe rays of light which, on their near approach to the edges of bodies, in paſſing by them, are bent out of their courſe, being turned either from the body or towards it. This property of the rays of light is generally termed *diffraction* by foreigners, and Dr. Hooke ſometimes called it *deflection*.

Iſland Cryſtal, a foſſil ſubſtance, firſt found in Iceland, remarkable for its property of a double refraction of the rays of light.

Lens, a piece of glaſs, or any other tranſparent ſubſtance, the ſurfaces of which are ſo formed, that the rays of light, by paſſing through it, are made to change their direction, either tending to meet in a point beyond the lens, or made to become parallel, after converging or diverging, or laſtly proceeding as if they had iſſued from a point before they fell upon the lens. Some lenſes are *convex*, or thicker in the middle, ſome *concave*, or thinner in the middle; ſome *plano convex*, or *plano concave*, that is with one ſide flat and the other convex or concave, and ſome are called *meniſcuſes*, or convex on one ſide, and concave on the other.

Medium, any ſubſtance, through which light is tranſmitted.

Micrometer, an inſtrument, by the help of which the apparent magnitudes of objects, viewed through teleſcopes or microſcopes, are meaſured with great exactneſs.

Microſcope, an optical inſtrument, conſiſting of lenſes, or mirrors, by means of which ſmall objects appear larger than they do to the naked eye. *Single microſcopes* conſiſt of ſingle lenſes, or mirrors; or if more lenſes or mirrors be made uſe of, they only ſerve to throw light upon the object, and do not contribute to enlarge the image of it. *Double microſcopes* are thoſe in which the image of an object is magnified by means of more lenſes or mirrors than one.

Mirror, a ſurface of any ſubſtance from which the rays of light are reflected, whether it be *plane*, *convex*, or *concave*. If, after reflexion, the rays of light meet in a point, it is called a *burning mirror,* becauſe a ſtrong heat is excited by the rays of light in that place.

Optic Axis, the axis of the eye, or a line going through the middle of the pupil, and the center of the eye.

Optic Angle, the angle which the optic axes of both eyes make with one another, as they tend to meet at ſome diſtance before the eyes.

Parhelion, or *mock ſun,* a part of the heavens ſtrongly illuminated by the image of the ſun, looking like another ſun, but often coloured, and drawn out to a conſiderable length, in the form of a tail.

Paraſelene, an appearance ſimilar to the parhelion, but made by the moon.

Pencil of rays, a number of rays iſſuing from a point of an object, and diverging in the form of a cone.

Perſpective, the art of drawing the appearance of objects by their images, on a plane, convex, or concave ſurface.

Plane of perpendicular refraction, in Iſland cryſtal, that which Mr. Huygens calls the *principal ſection* of the cryſtal. It is peculiar to this plane, and its parallels, that all rays incident upon it keep within the ſame plane, after they divide within the cryſtal.

Priſm, a piece of glaſs, or any other tranſparent ſubſtance, contained under three plane ſides; ſo that the rays of light tranſmitted through them may be ſo refracted, as to be divided into their proper colours, and diverge from one another.

Ray of light, a term which is now generally applied to the ſmalleſt particle into which any quantity of light is diviſible; but often has been, and is ſometimes ſtill uſed for what is more properly called a *beam* of light, or a ſenſible quantity, admitted by an aperture into any place.

Reflected Rays, thoſe rays of light, which, after falling upon any body, do not go beyond the ſurface of it, but are thrown back again.

Refracted rays, thoſe rays of light which, after falling upon any medium, enter its ſurface, being bent either *towards* or *from* a perpendicular to the point on which they fell.

Spectrum, the coloured image of the ſun, made by light tranſmitted through a priſm, in a darkened room.

Teleſcope, an optical inſtrument, conſiſting of lenſes or mirrors, by the help of which remote objects appear larger, and nearer than they do to the naked eye.

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A Catalogue of the BOOKS, of which Dr. PRIESTLEY is already poſſeſſed, or to which he has acceſs, for compiling The History of Experimental Philosophy.

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BOOKS *not yet procured.*

Acta Upſalienſia.
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Lambert's Photometrie

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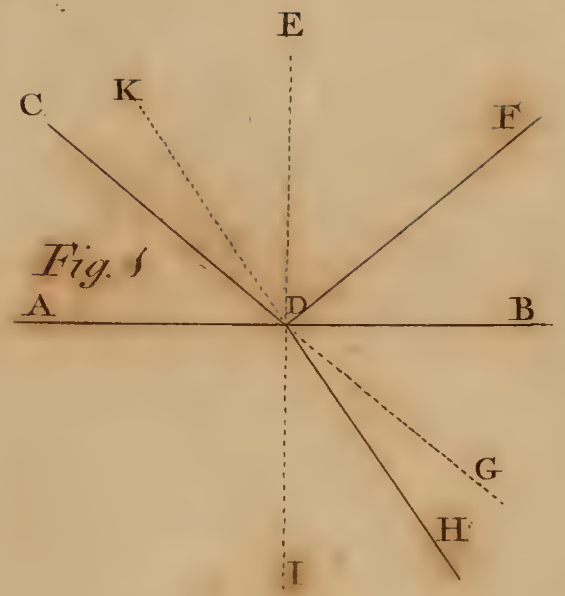
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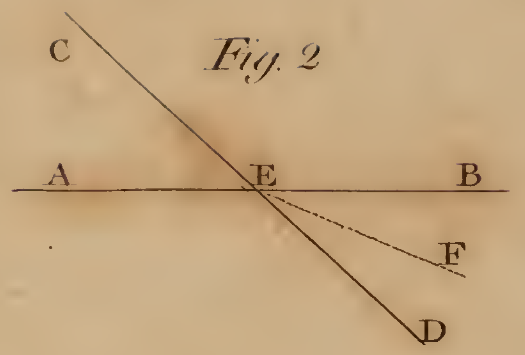
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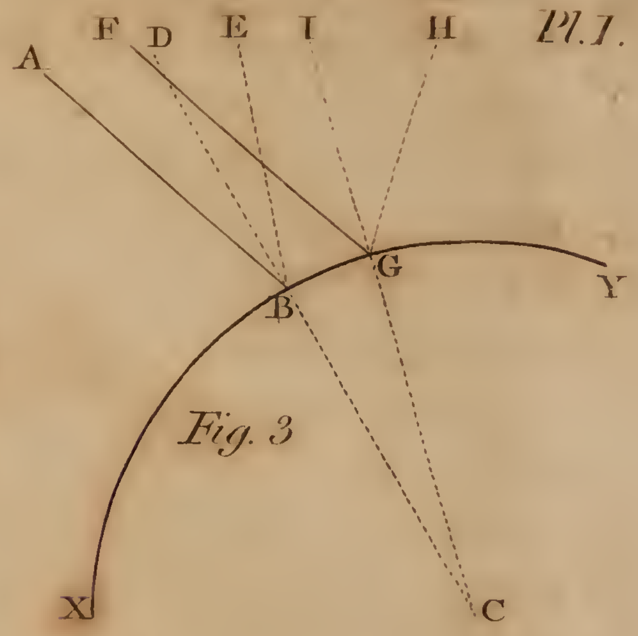
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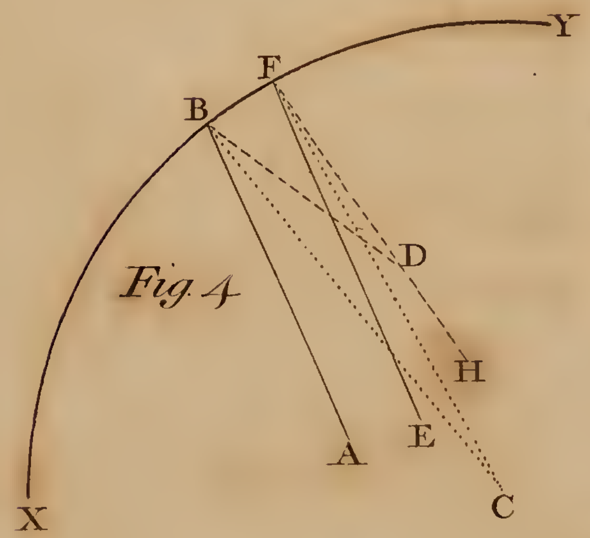
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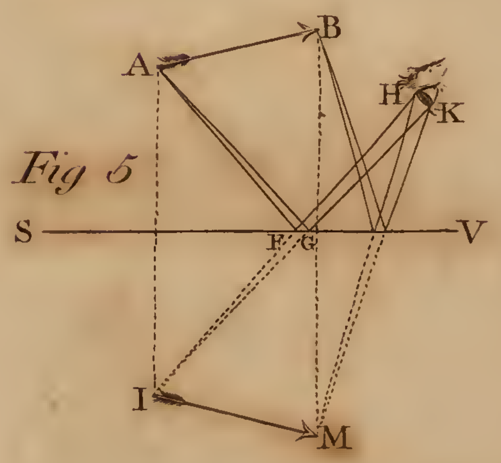
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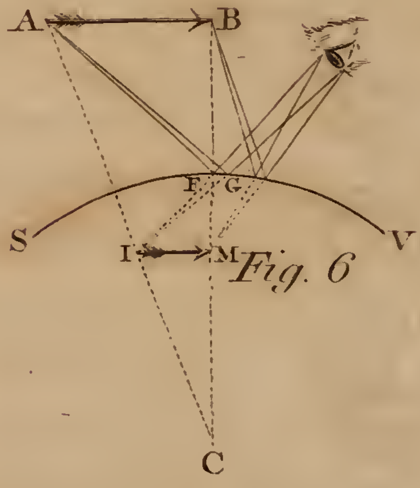
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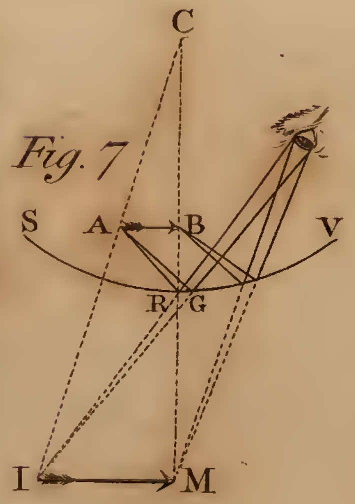
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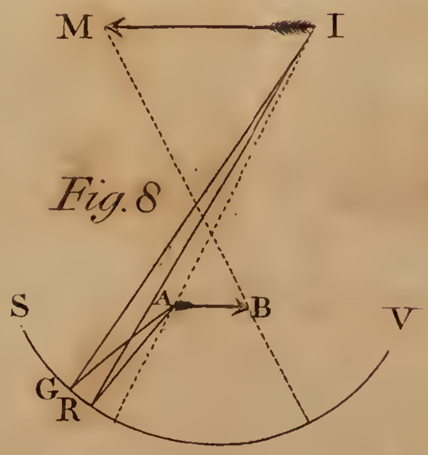
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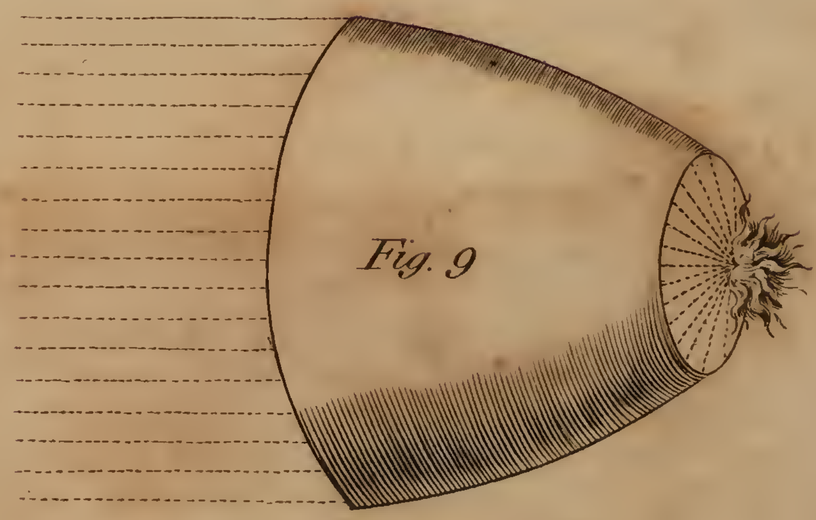
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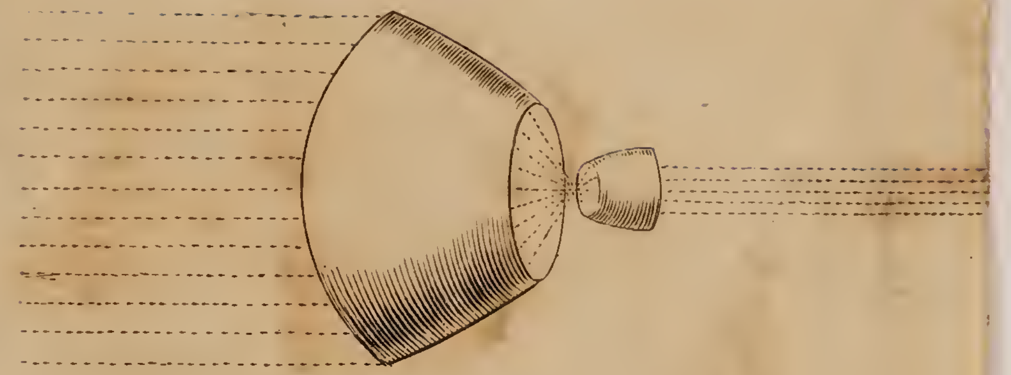

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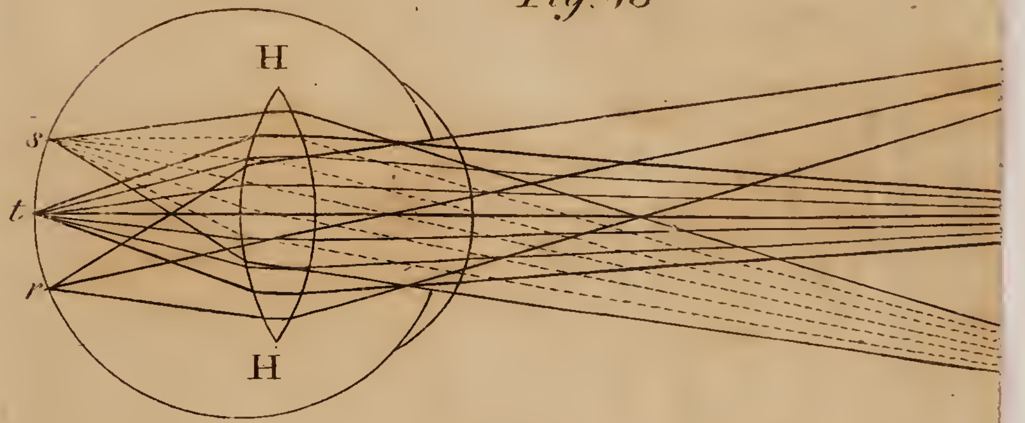

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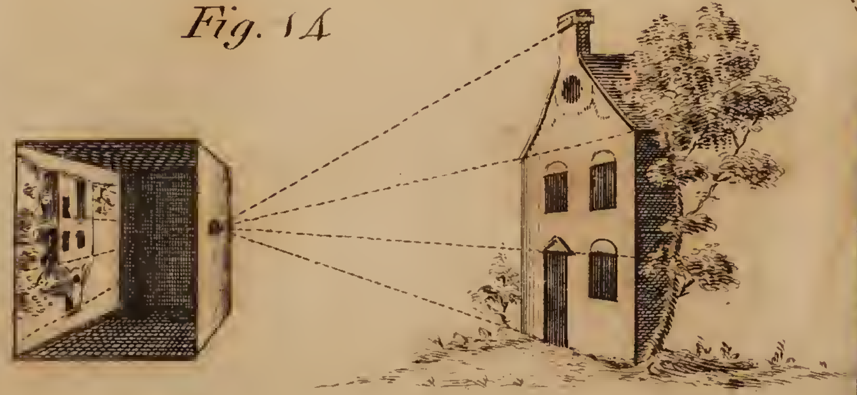

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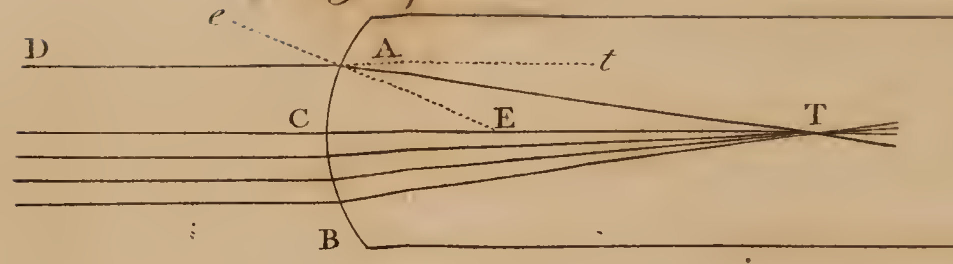

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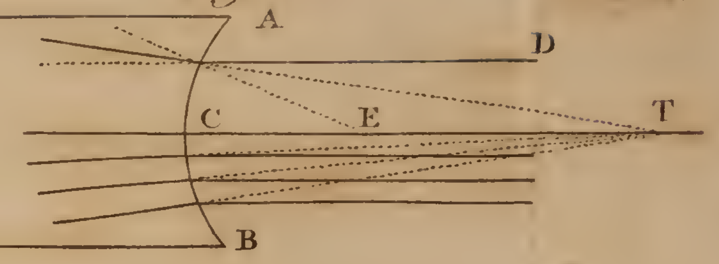

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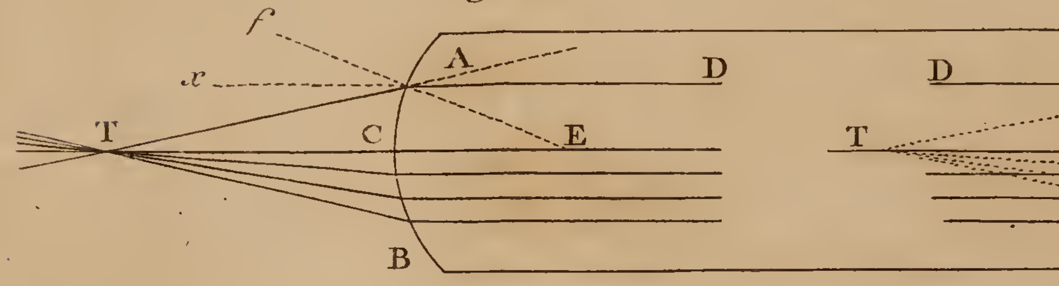

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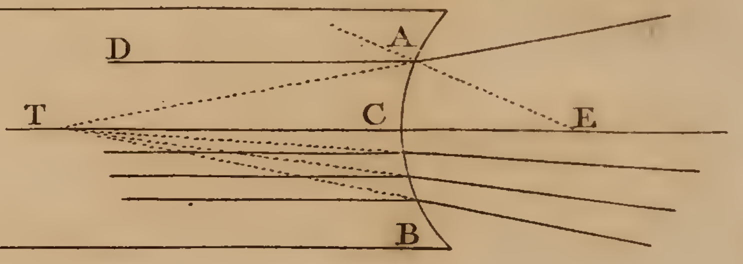

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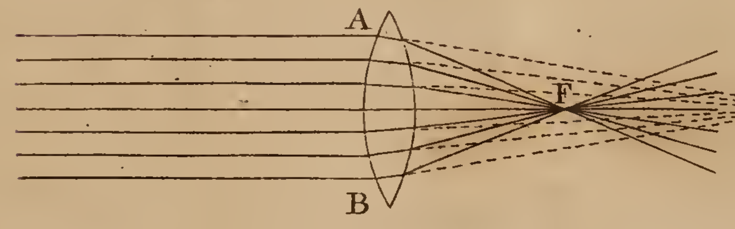

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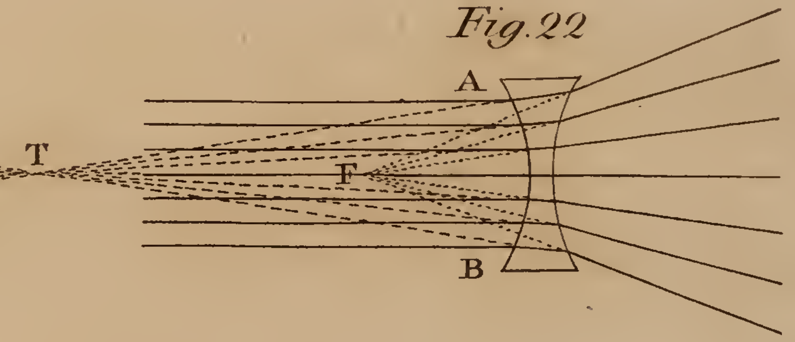
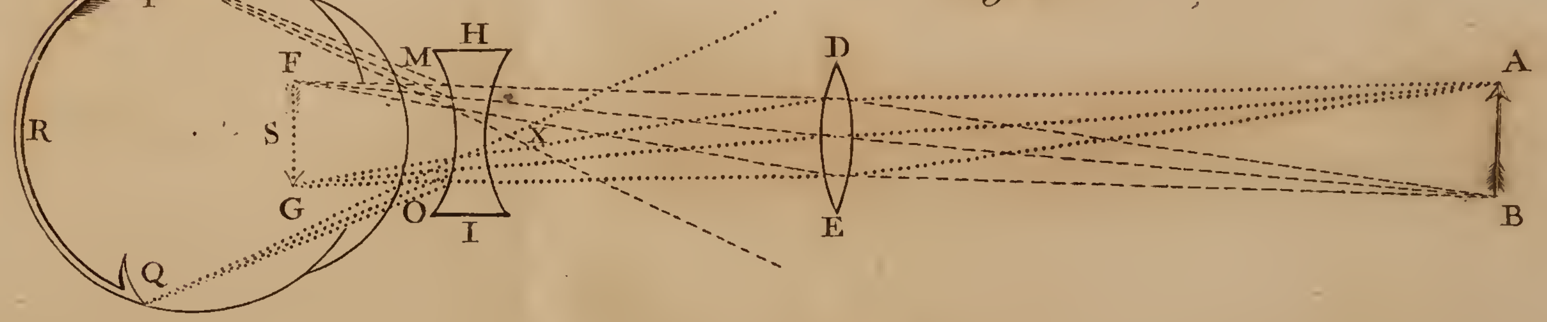


Fig. 23



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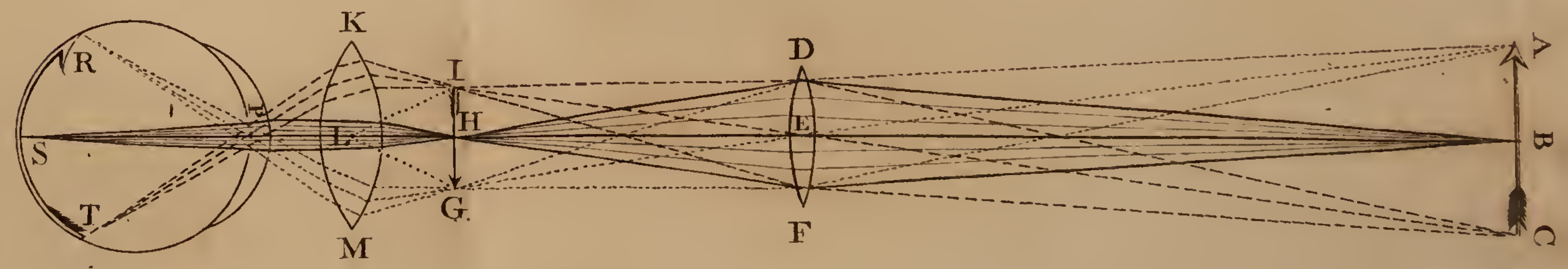


Pl. IIII.

Fig. 25

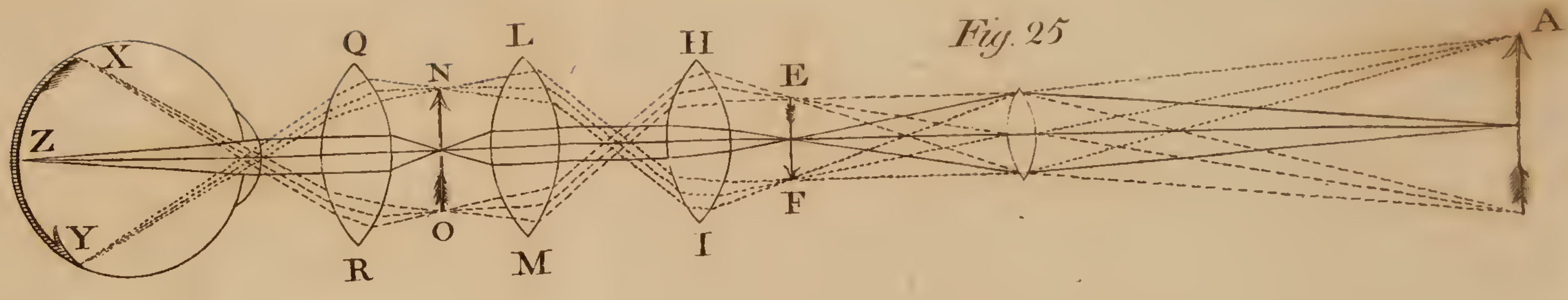
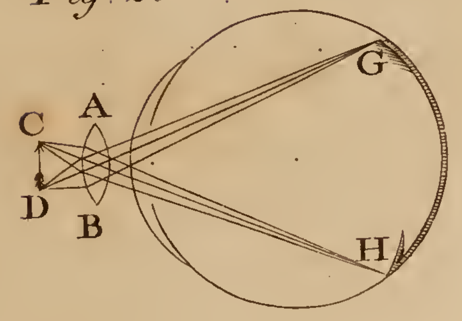


Fig. 26



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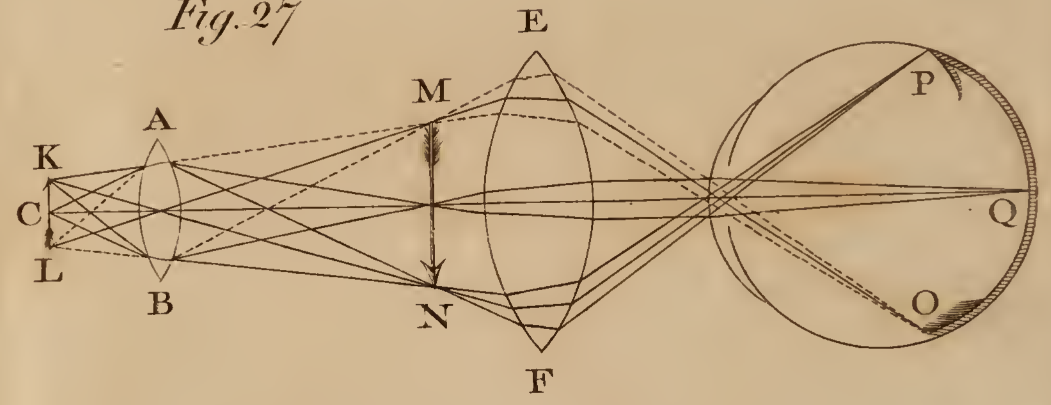

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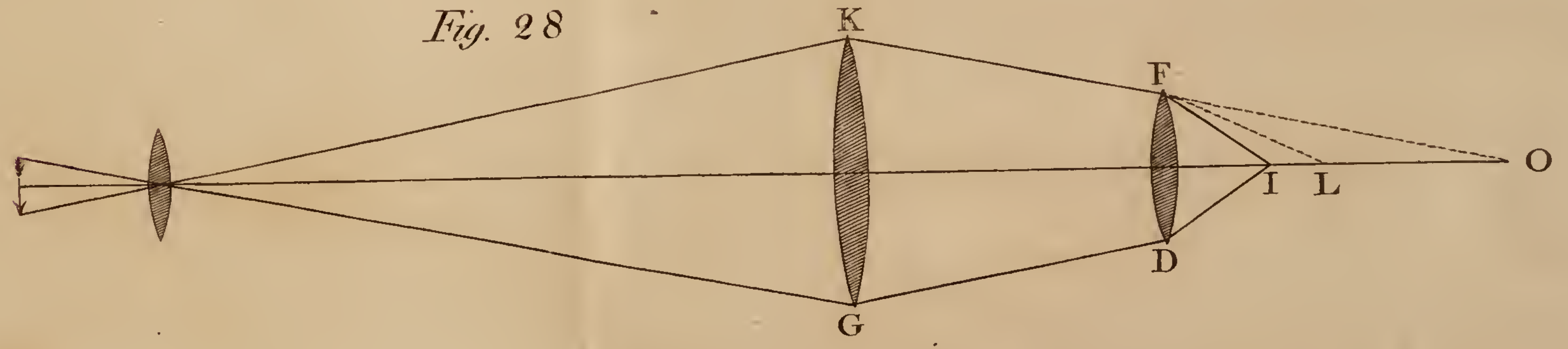

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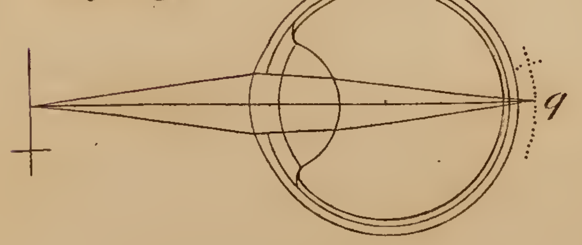

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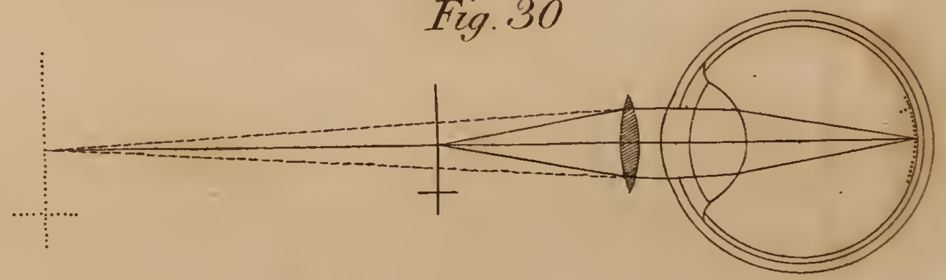

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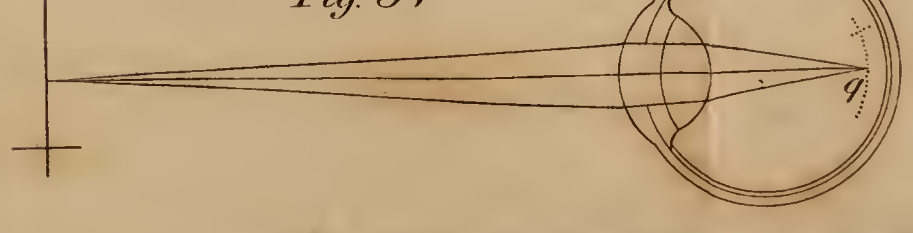

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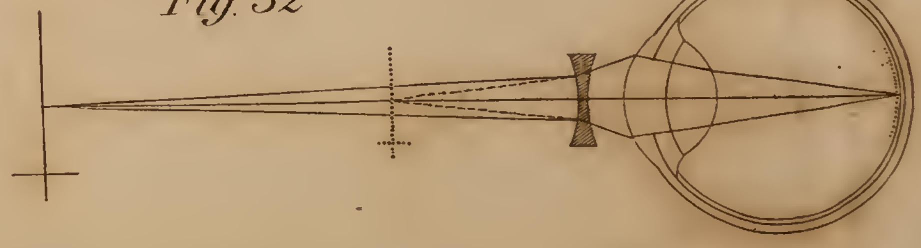


Pl. V.

Fig. 33

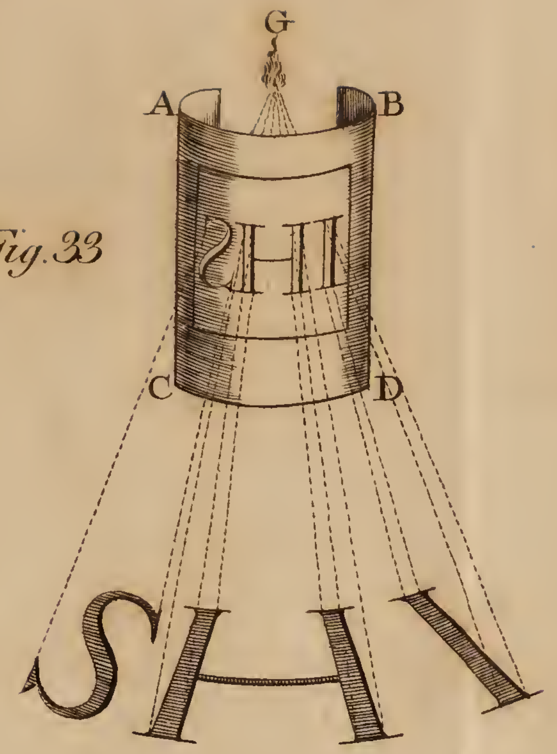
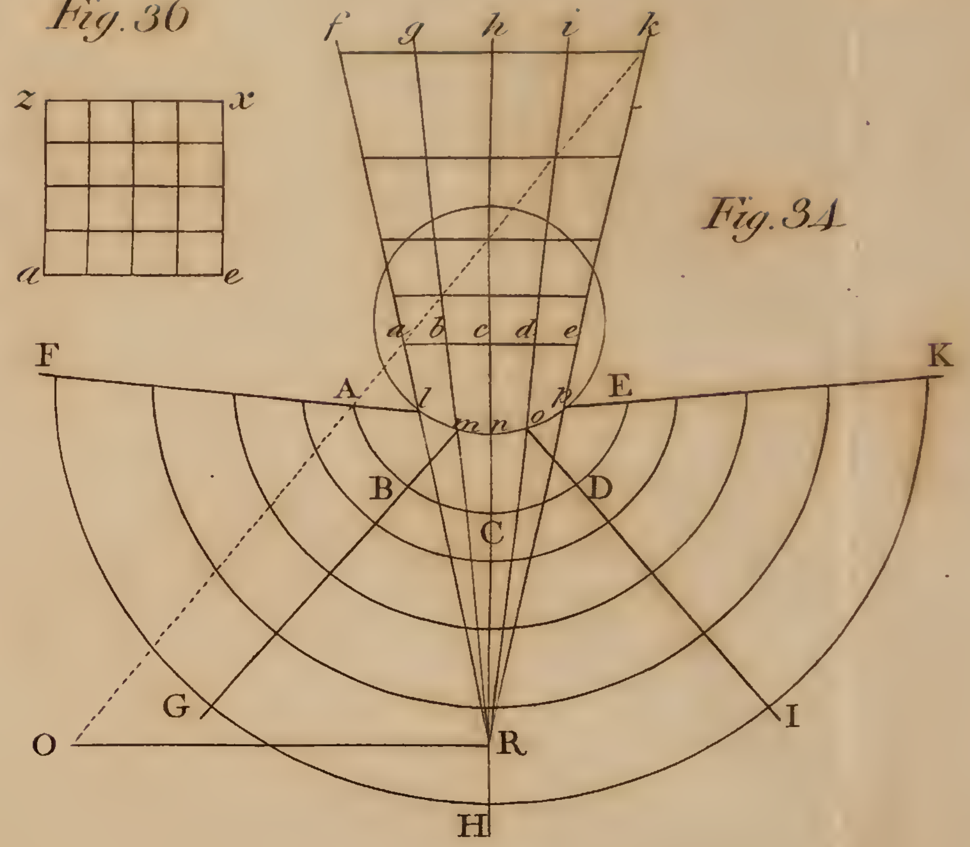


Fig. 36

Fig. 34



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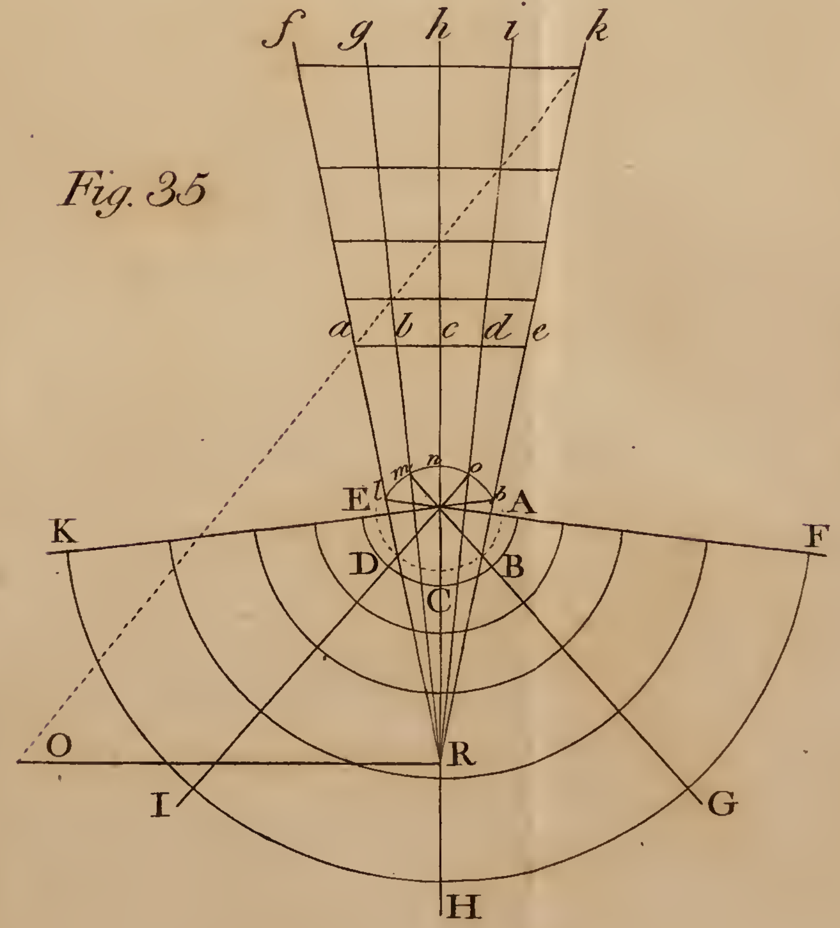

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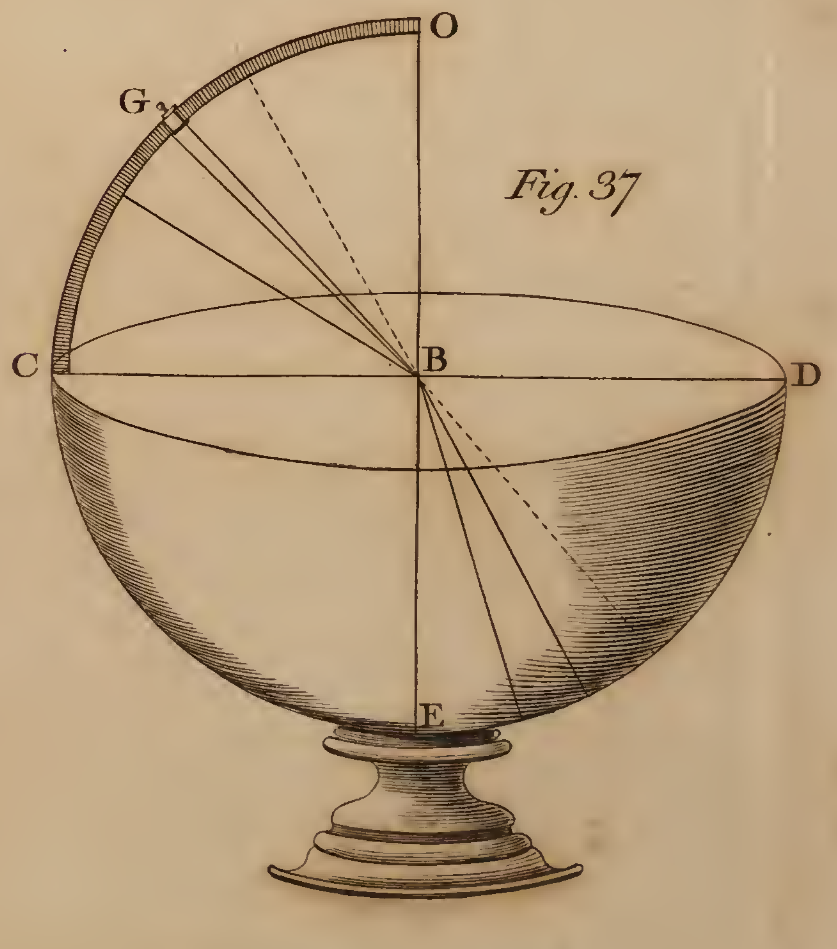


Pl. VI.

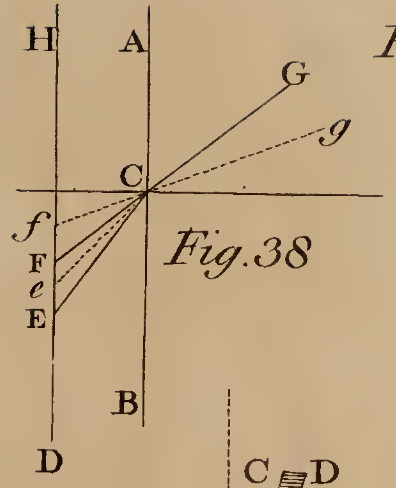
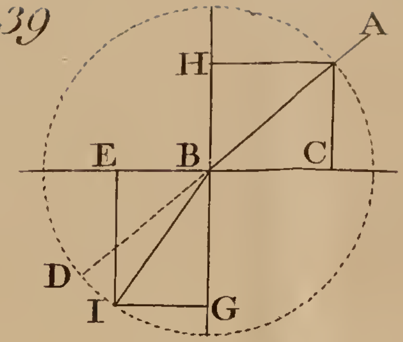


Fig. 38

Fig. 39


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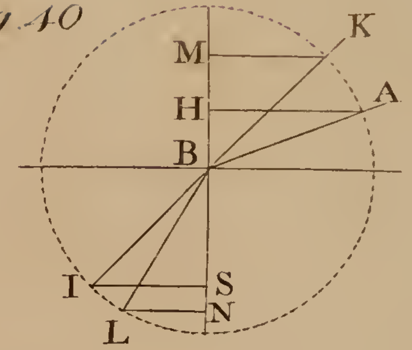

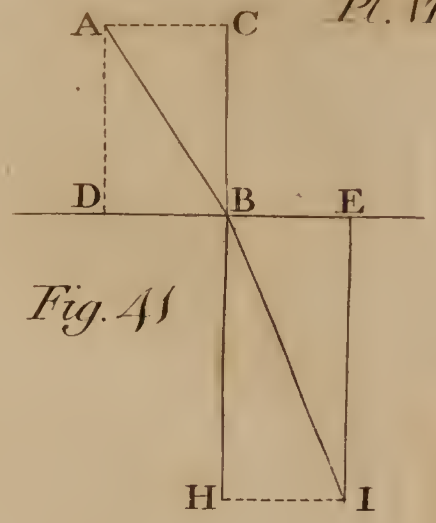
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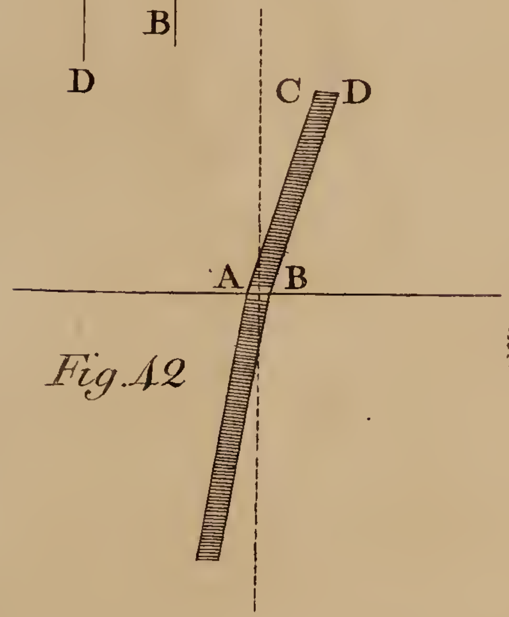
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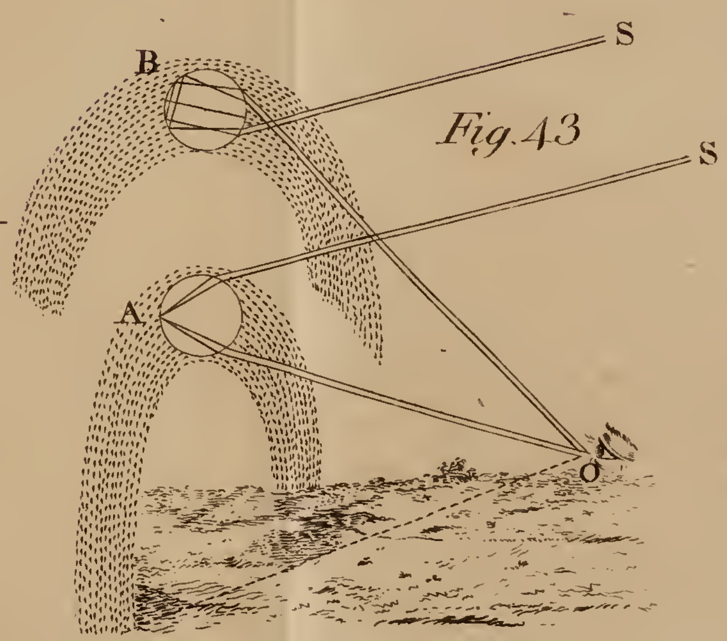
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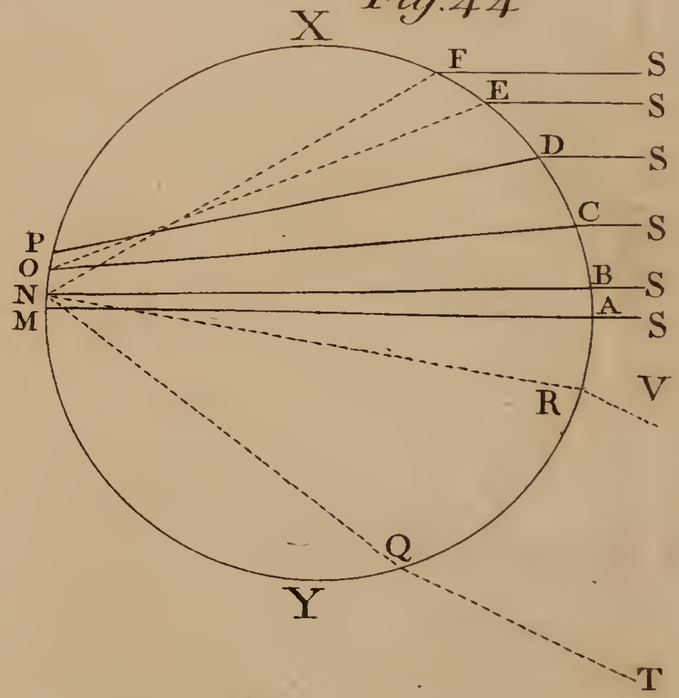
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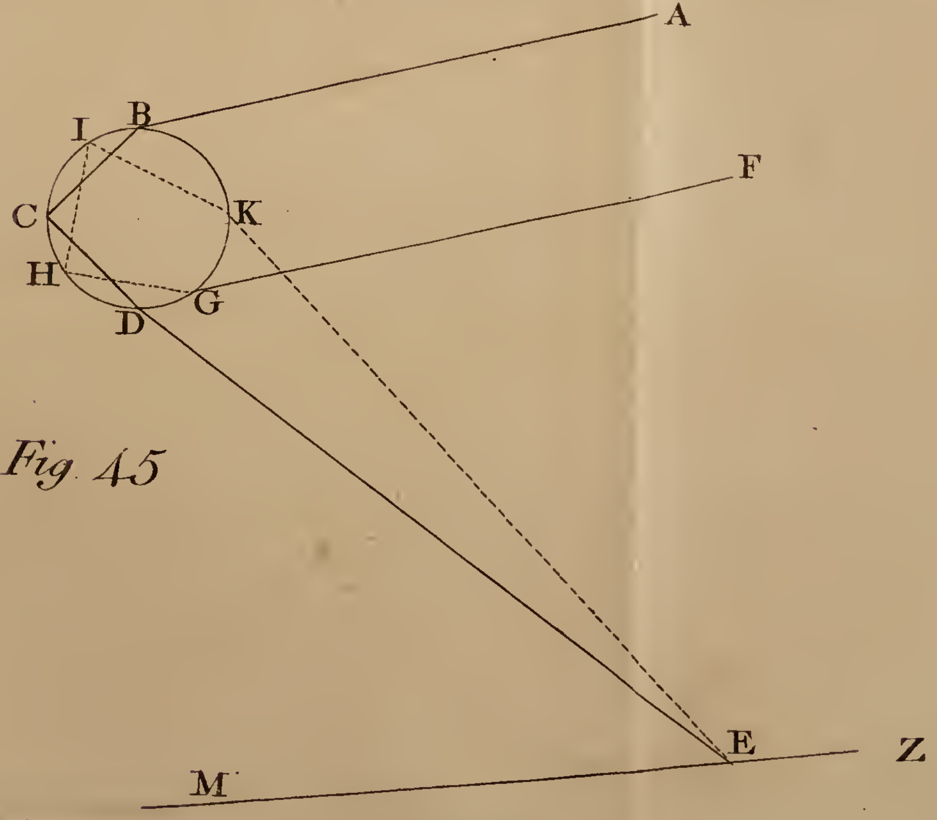
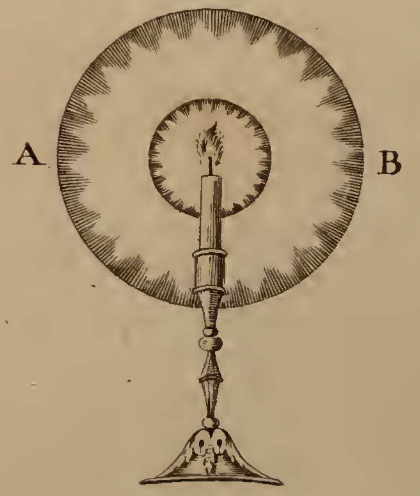
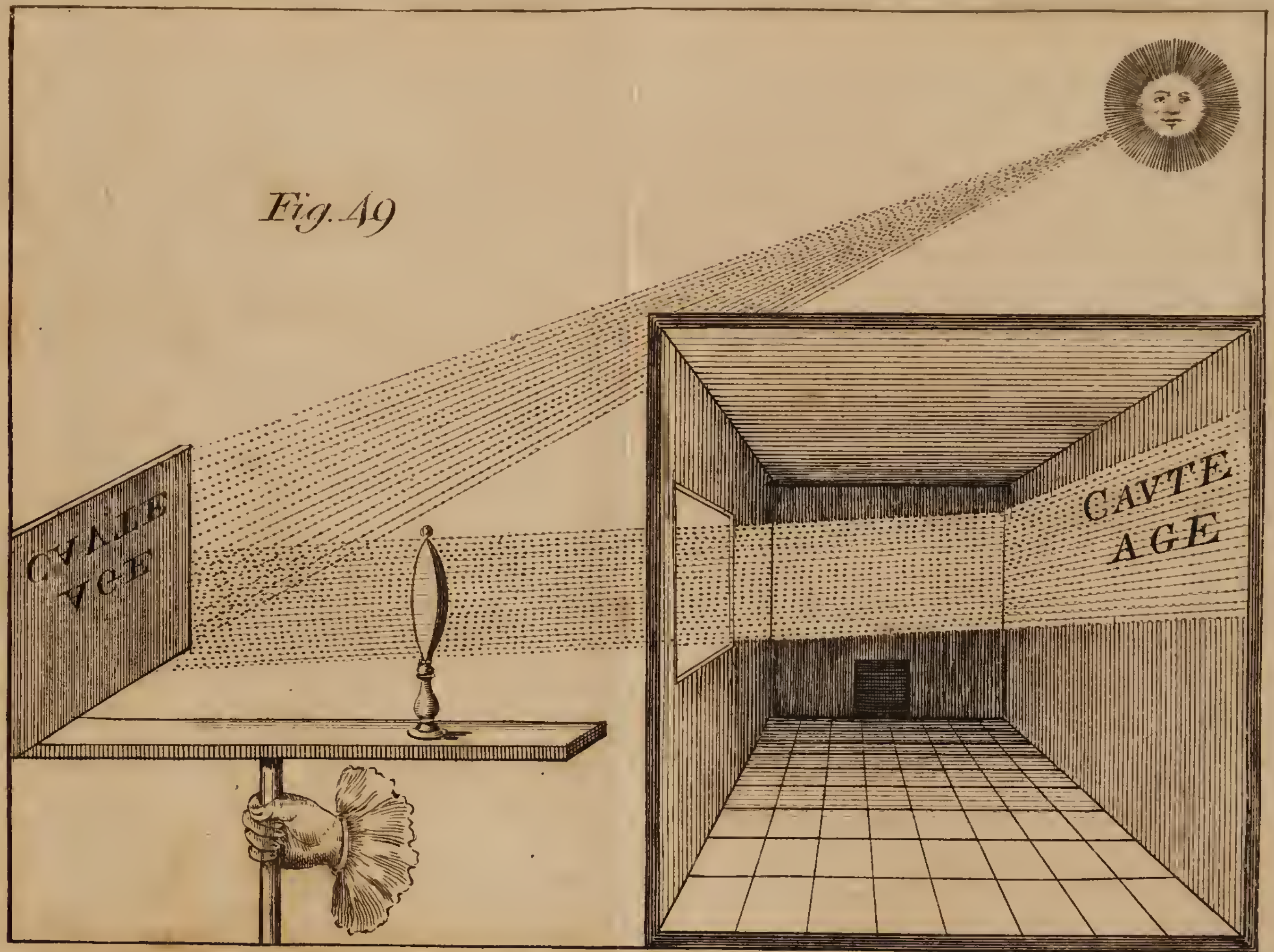


Fig. 45

Fig. 46




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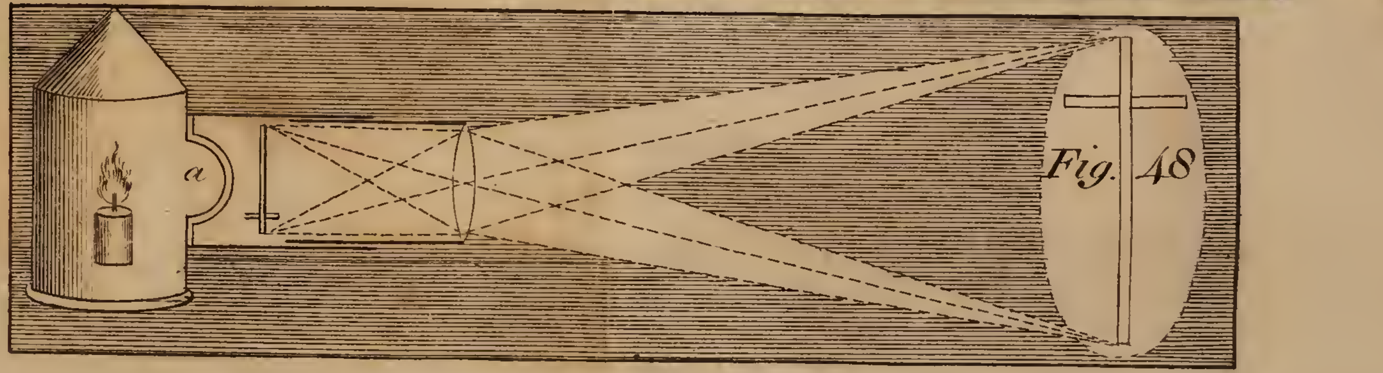
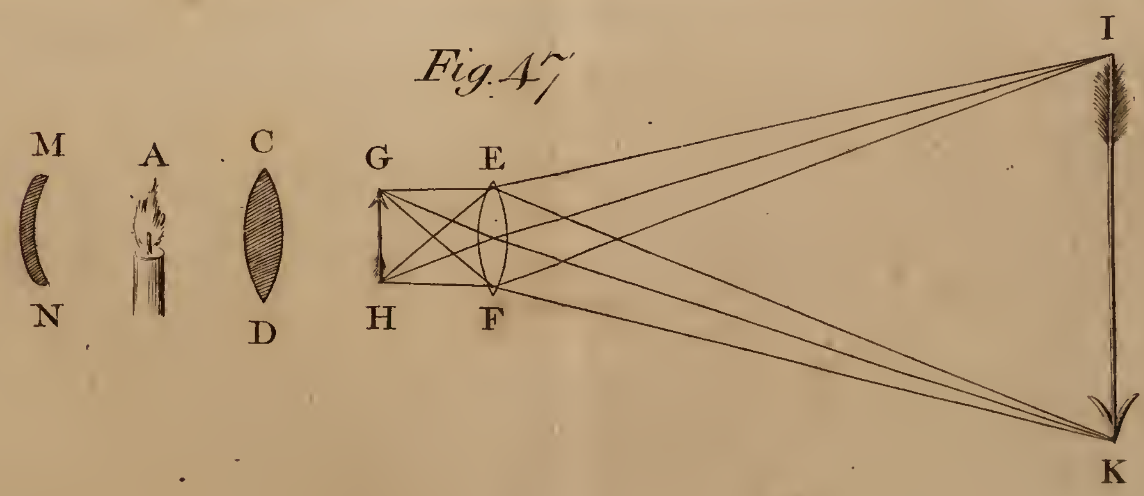
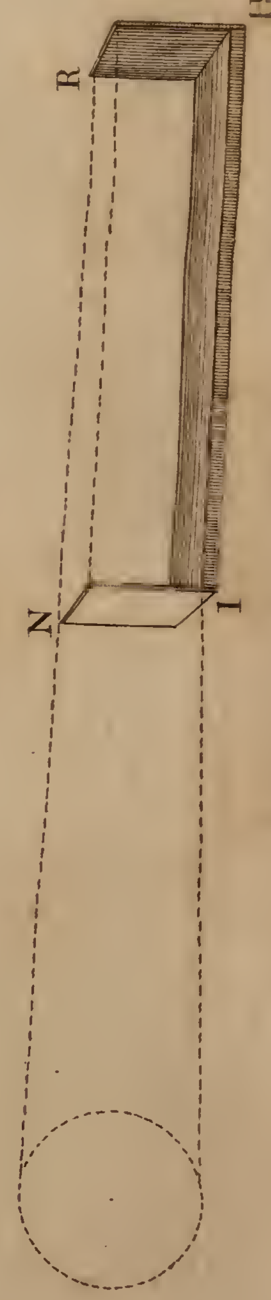


Fig. 48

Fig. 47





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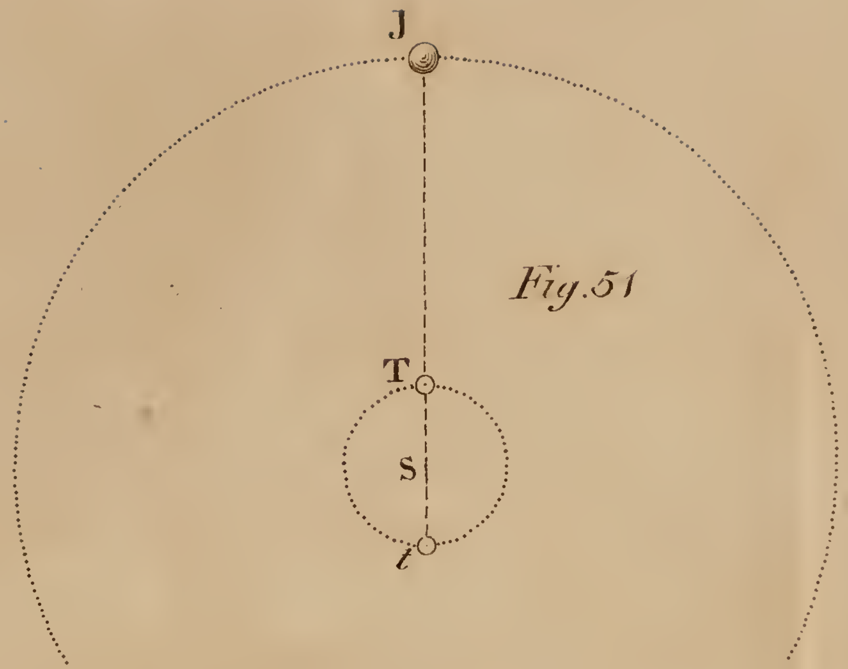
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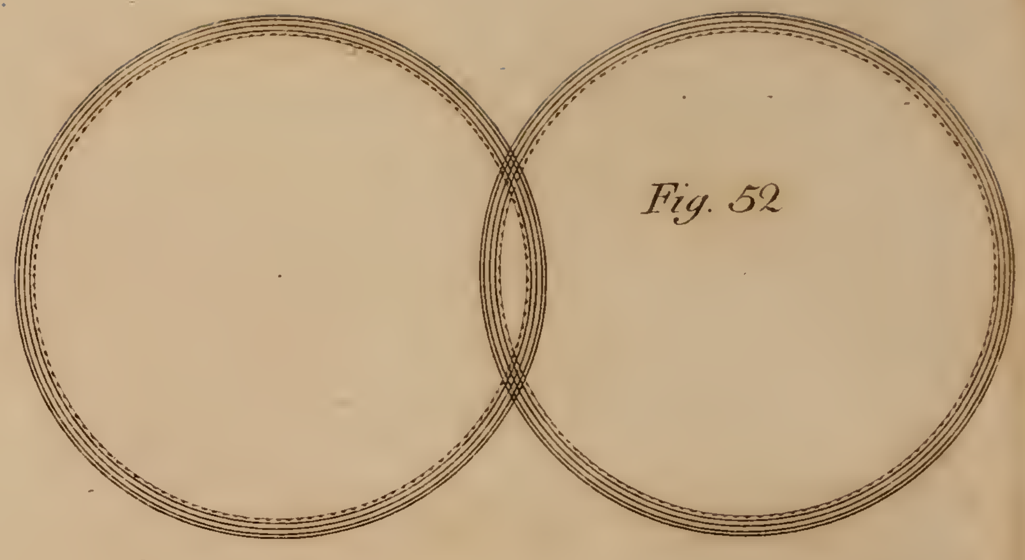
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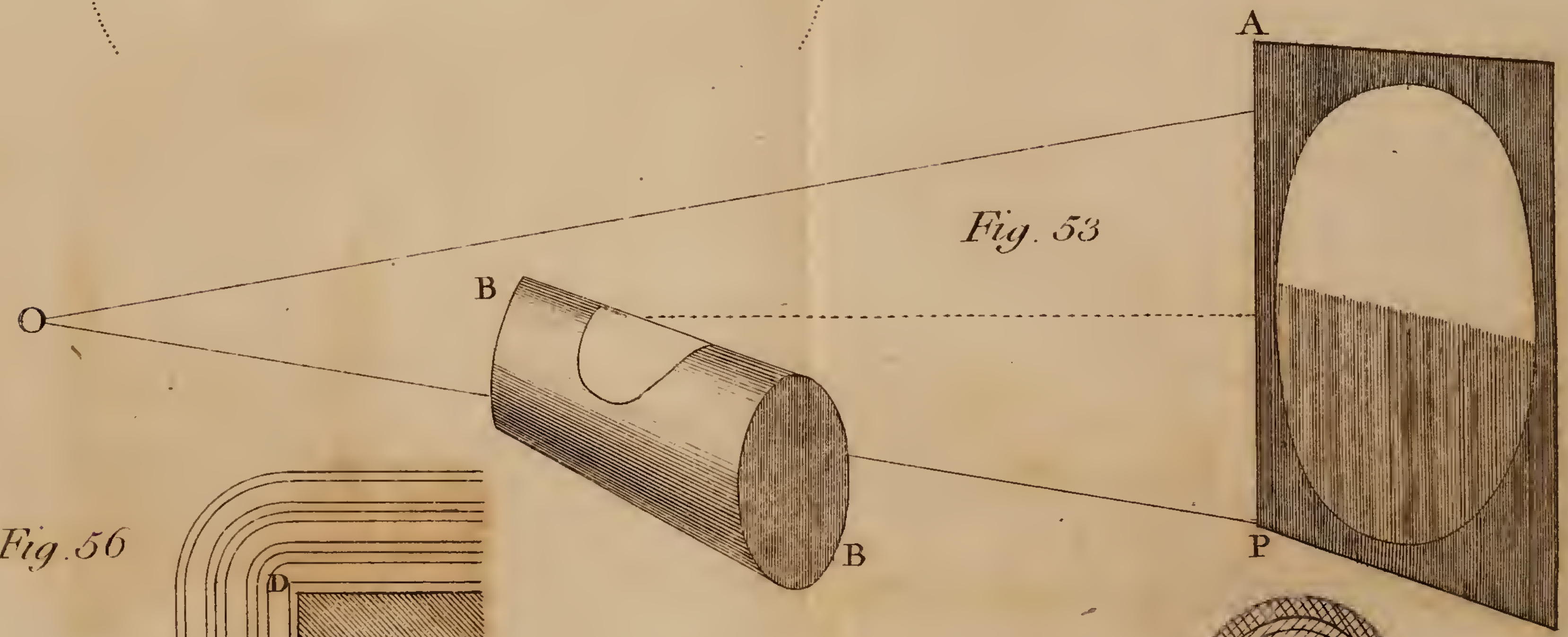
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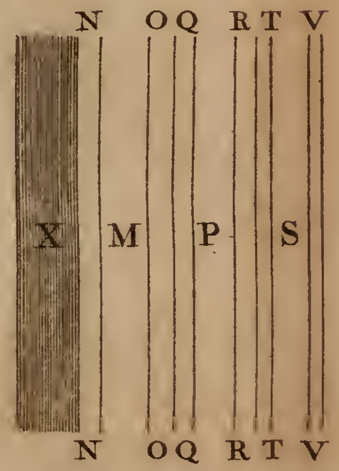
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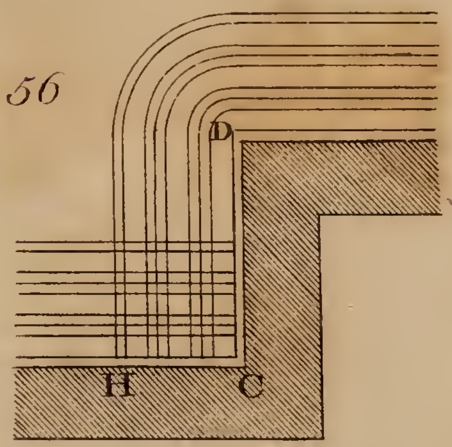
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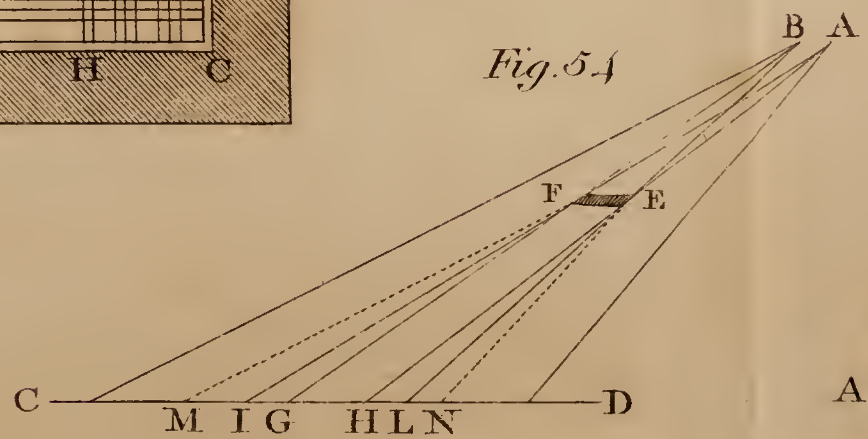
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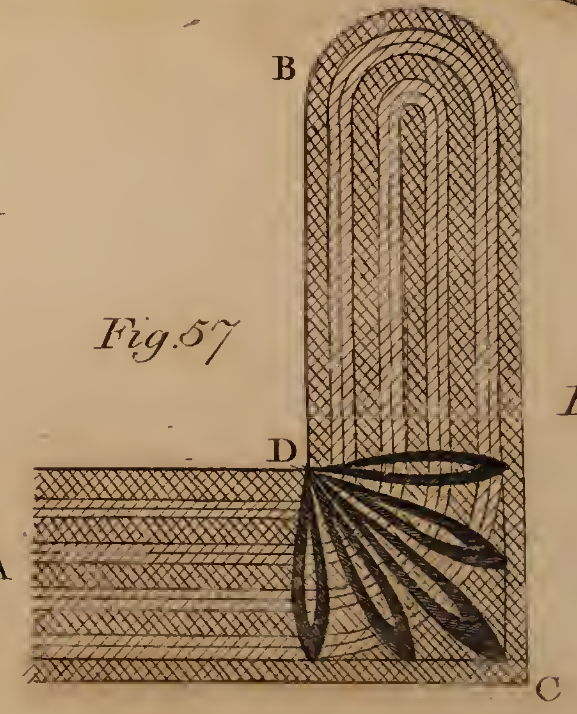
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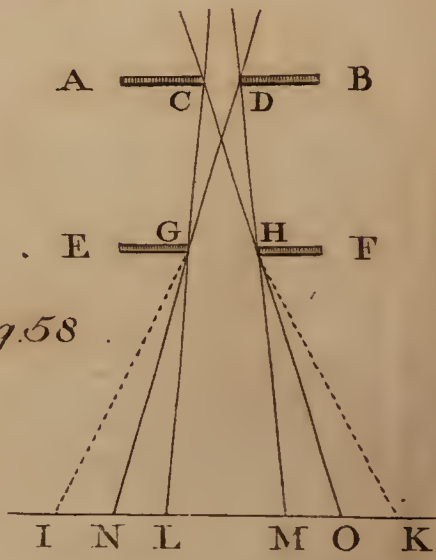


Fig. 58

Pl. IX.

Fig. 59

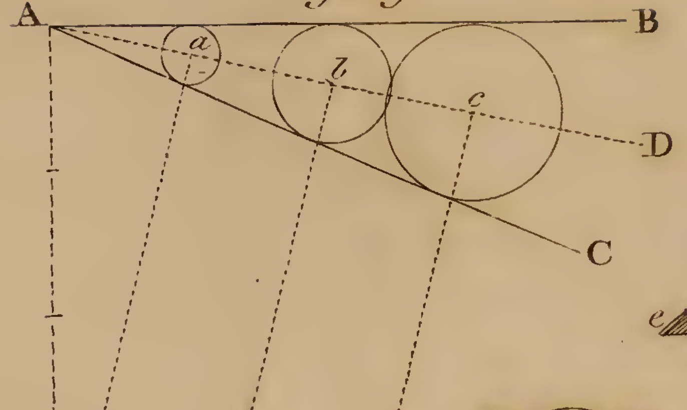


Fig. 60

Fig. 62

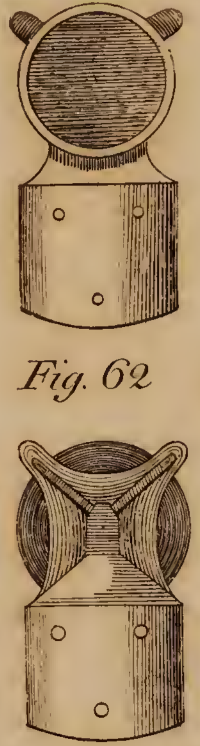
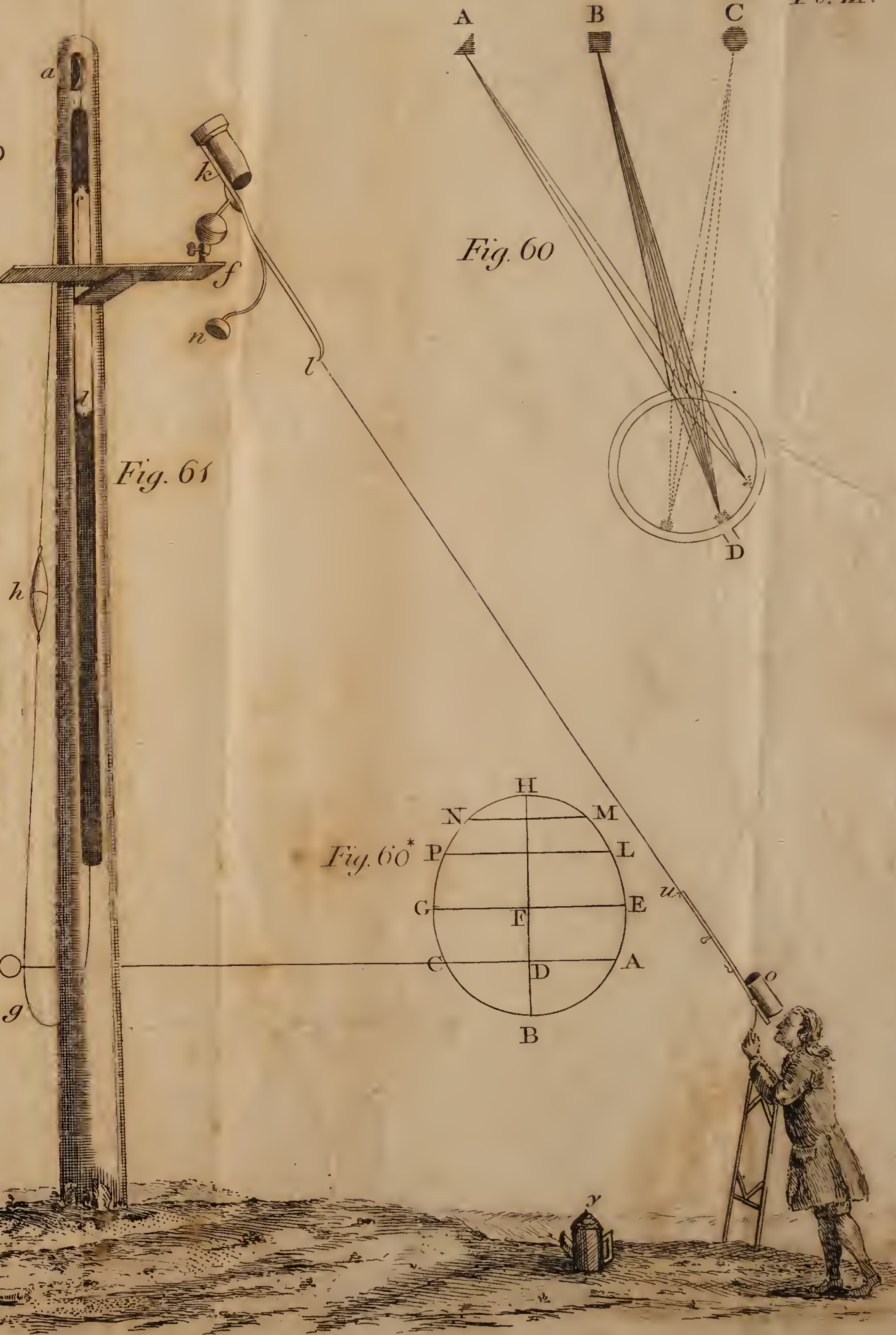


Fig. 61

Fig. 60*



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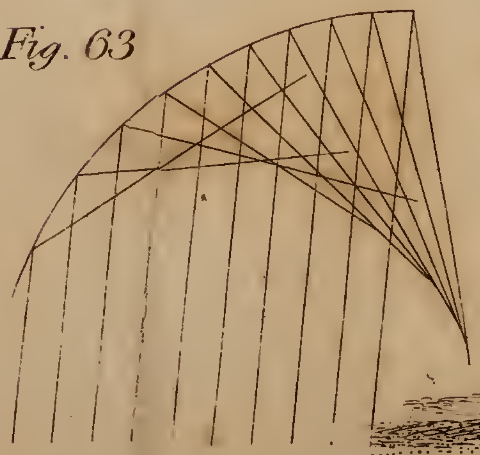
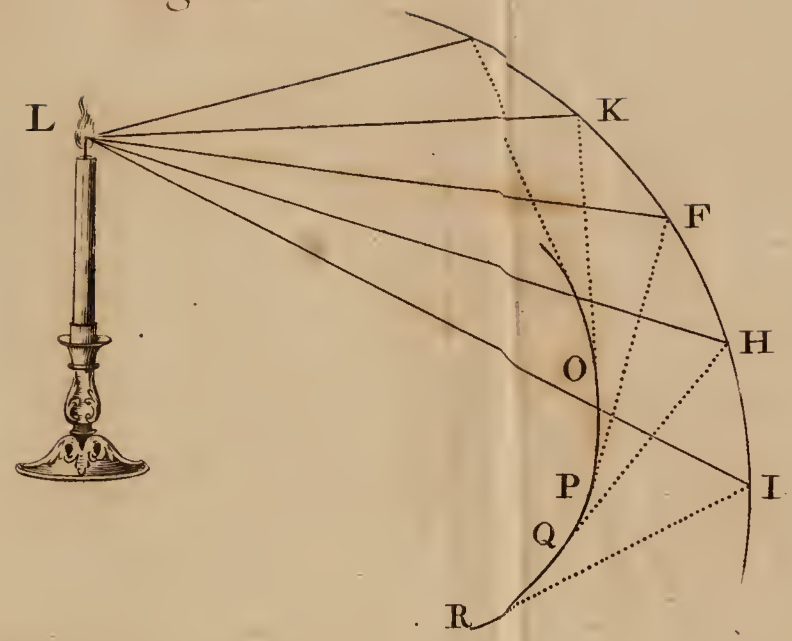


Pl. X.

Fig. 64



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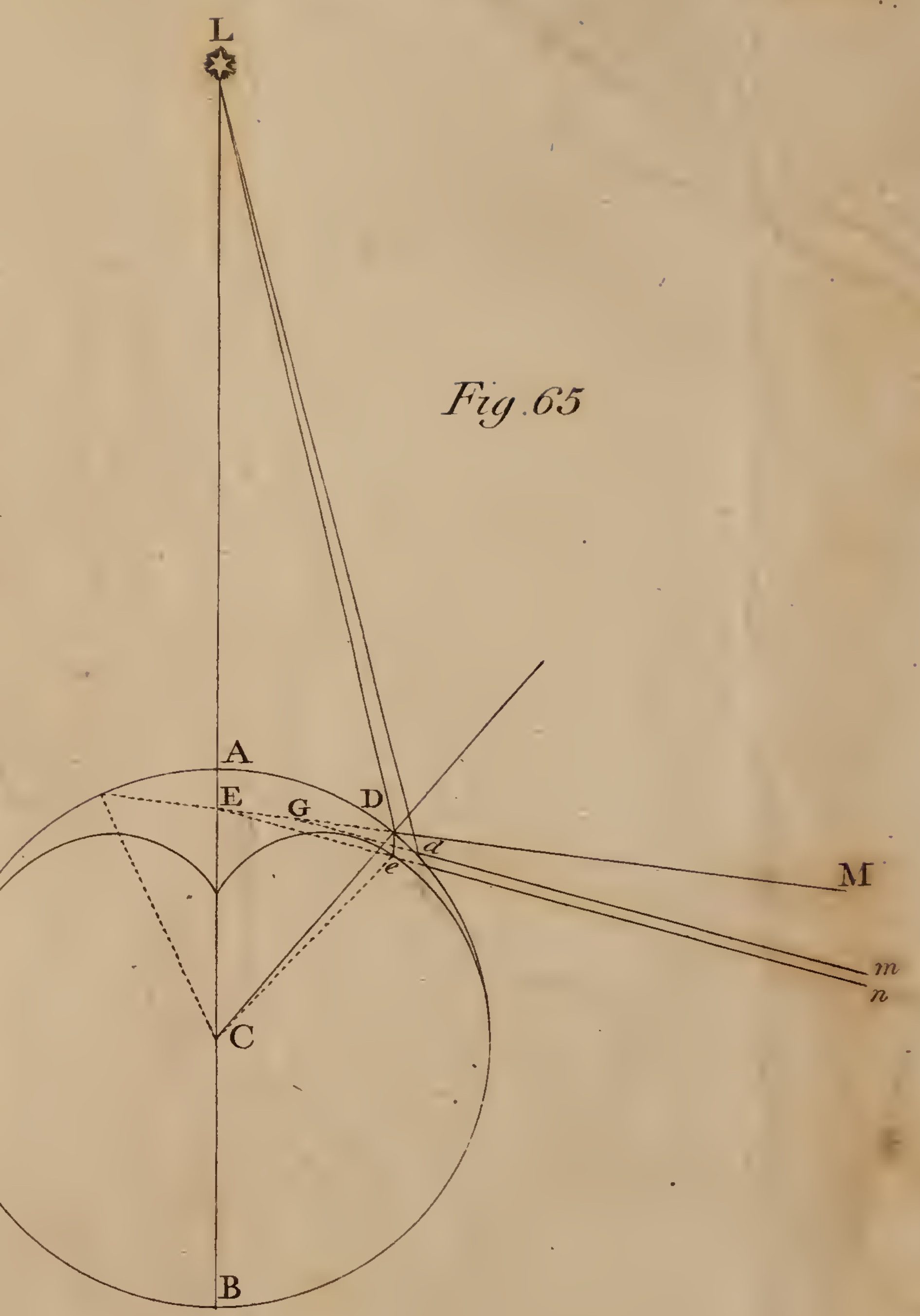

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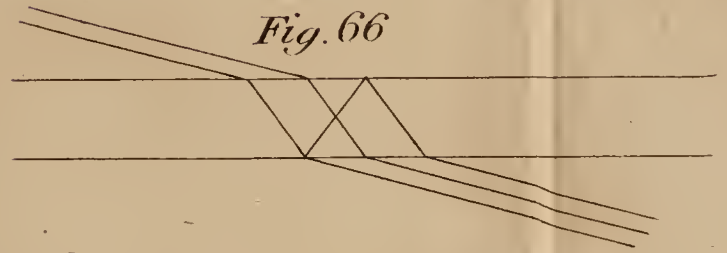

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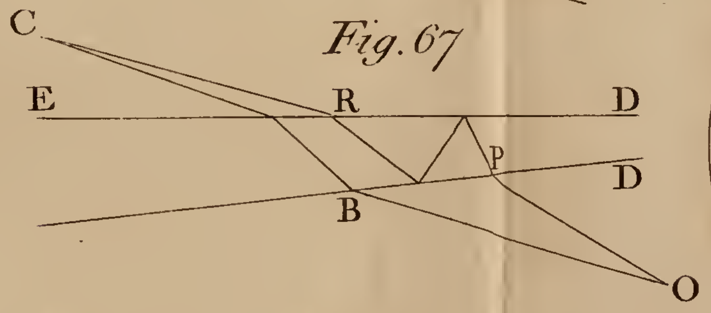

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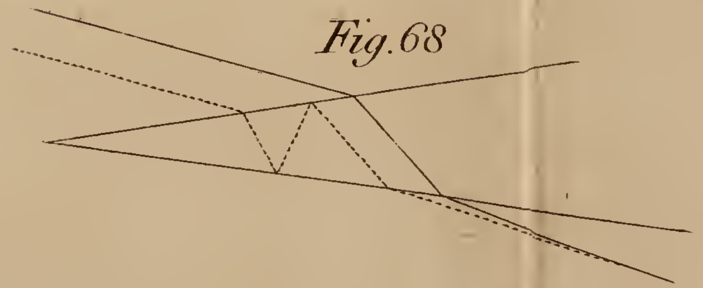

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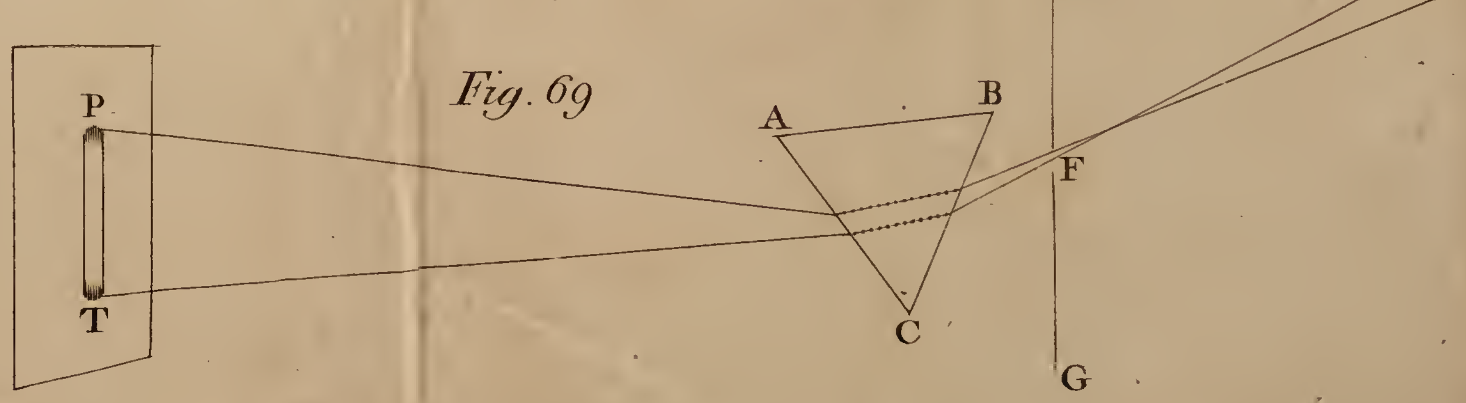
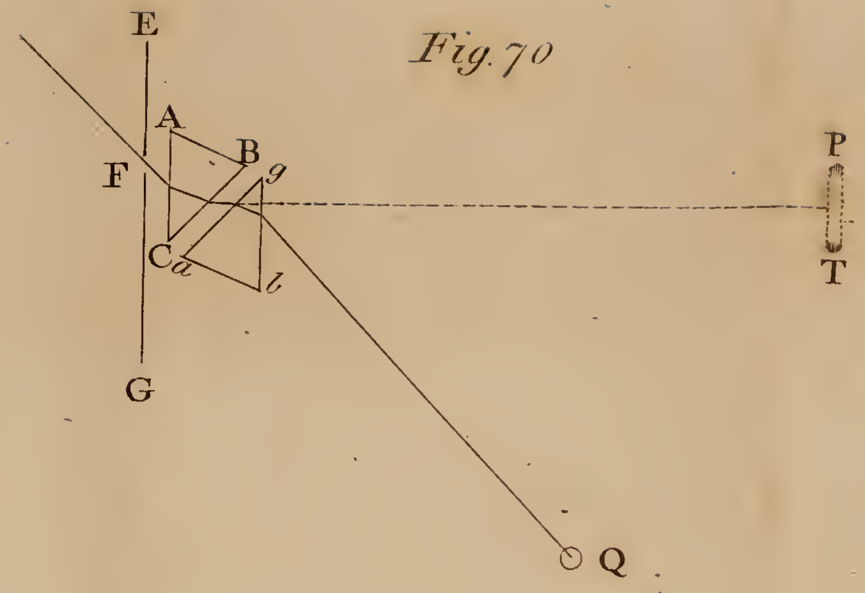


Pl. XI.

Fig. 70



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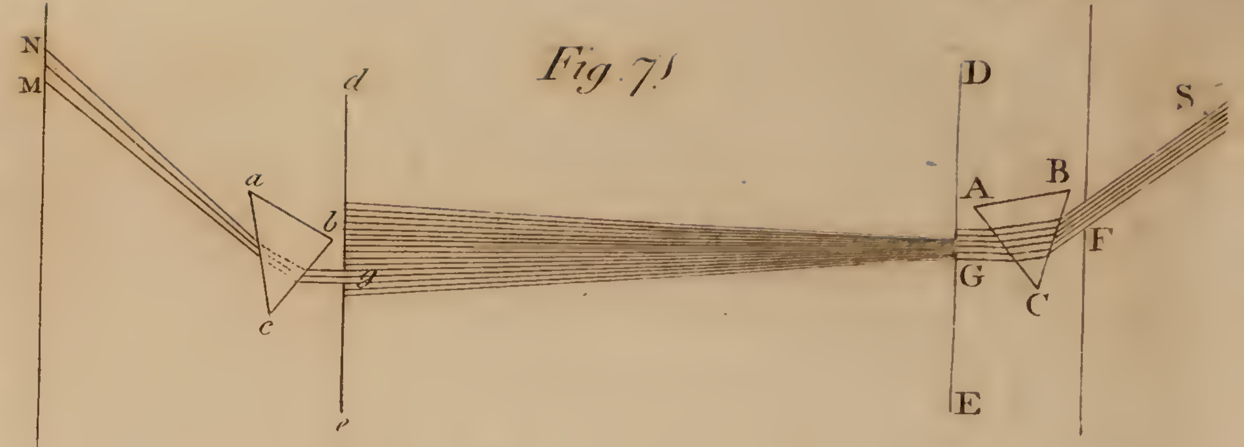

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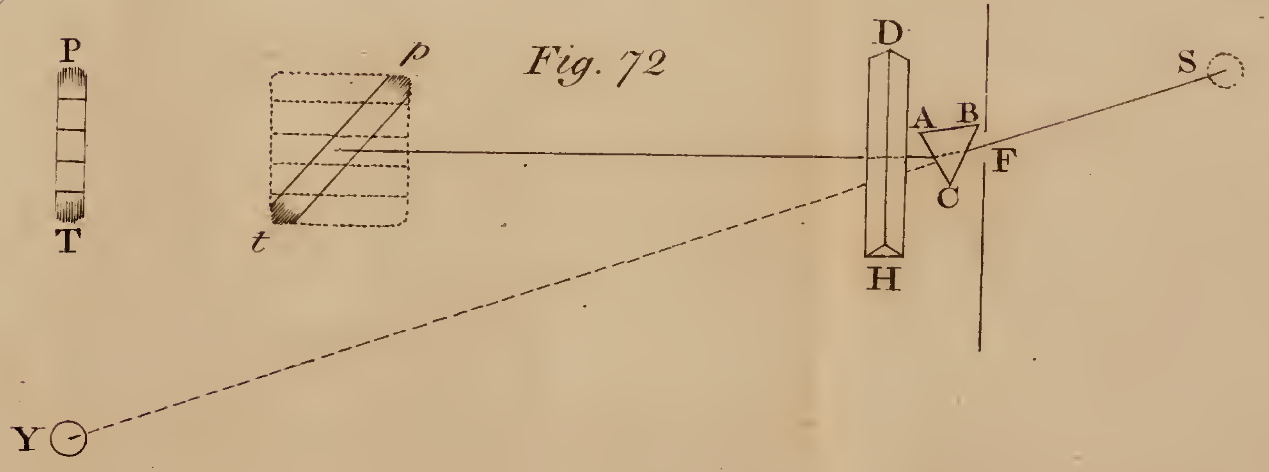

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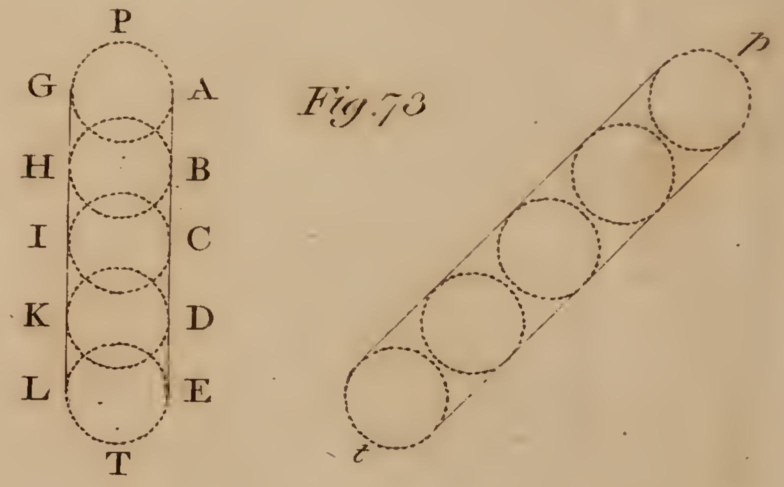

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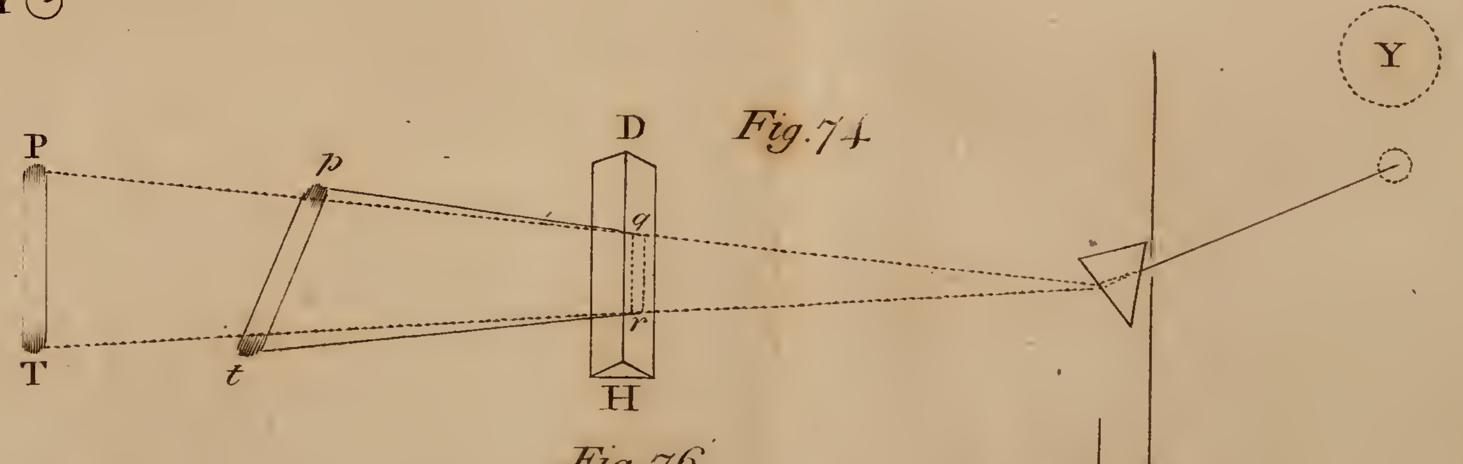

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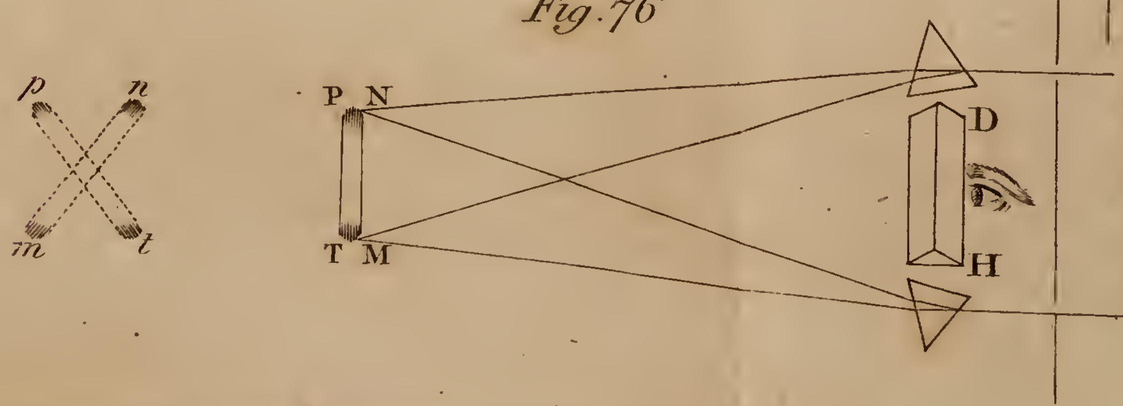

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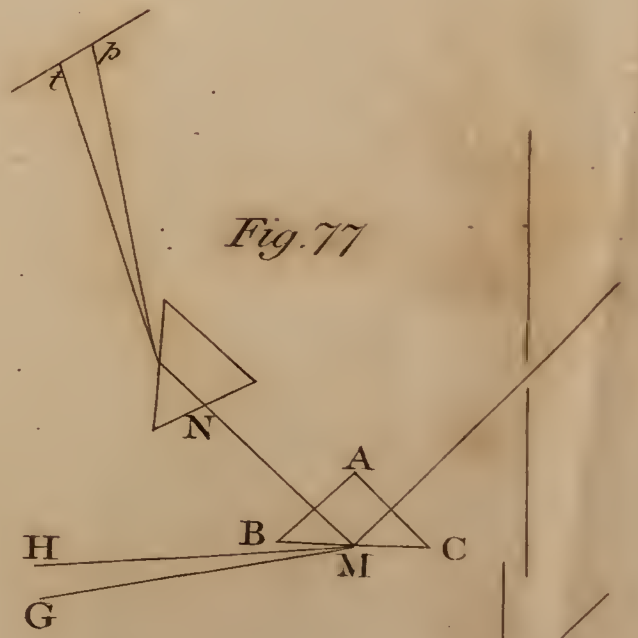

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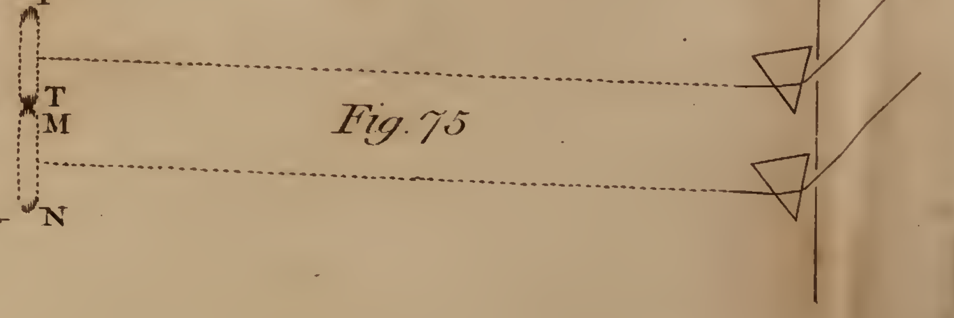
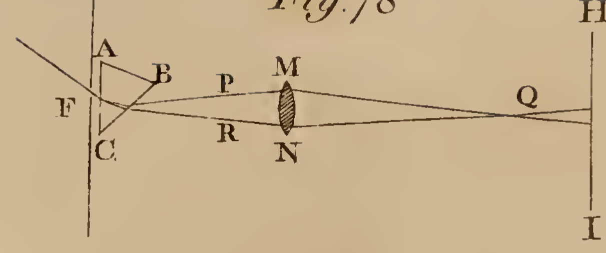


Pl. XII.

Fig. 78



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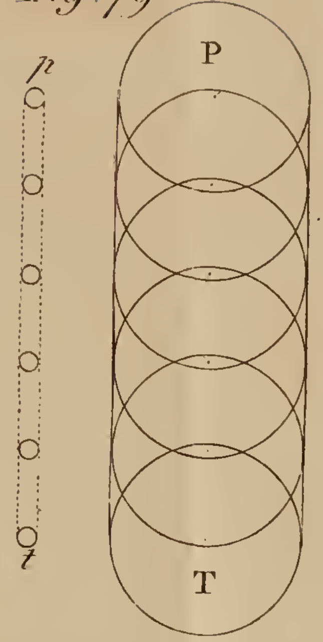

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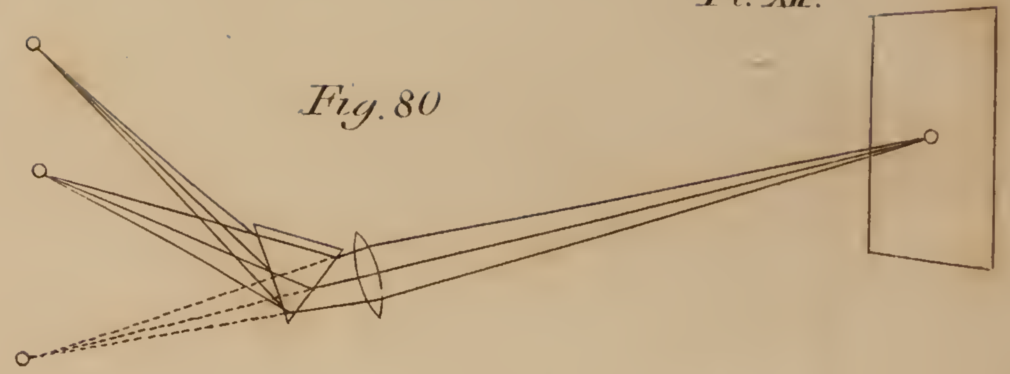


Fig. 81

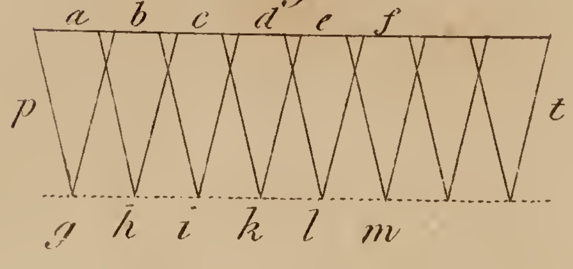
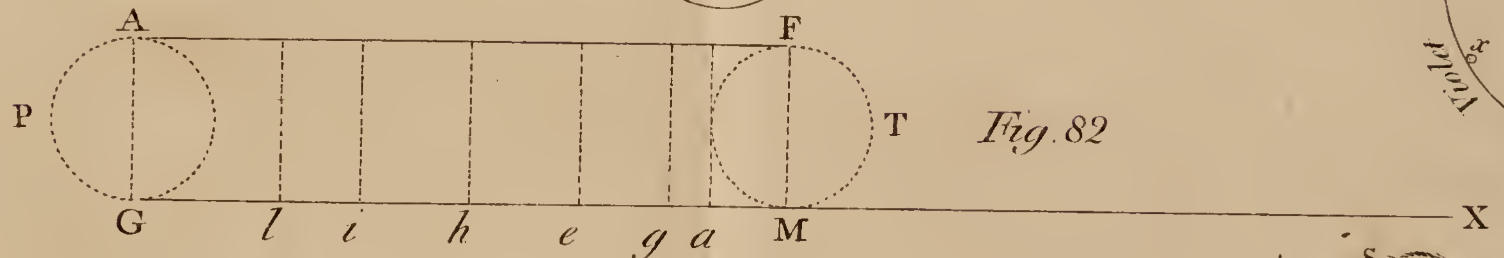


Fig. 82



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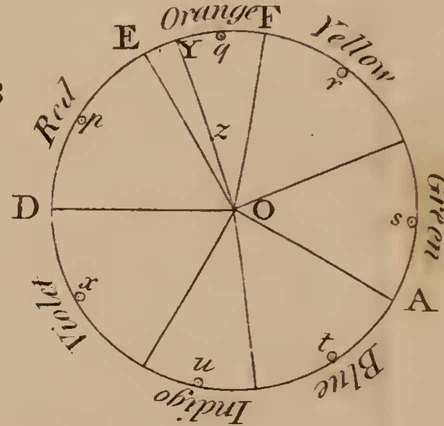

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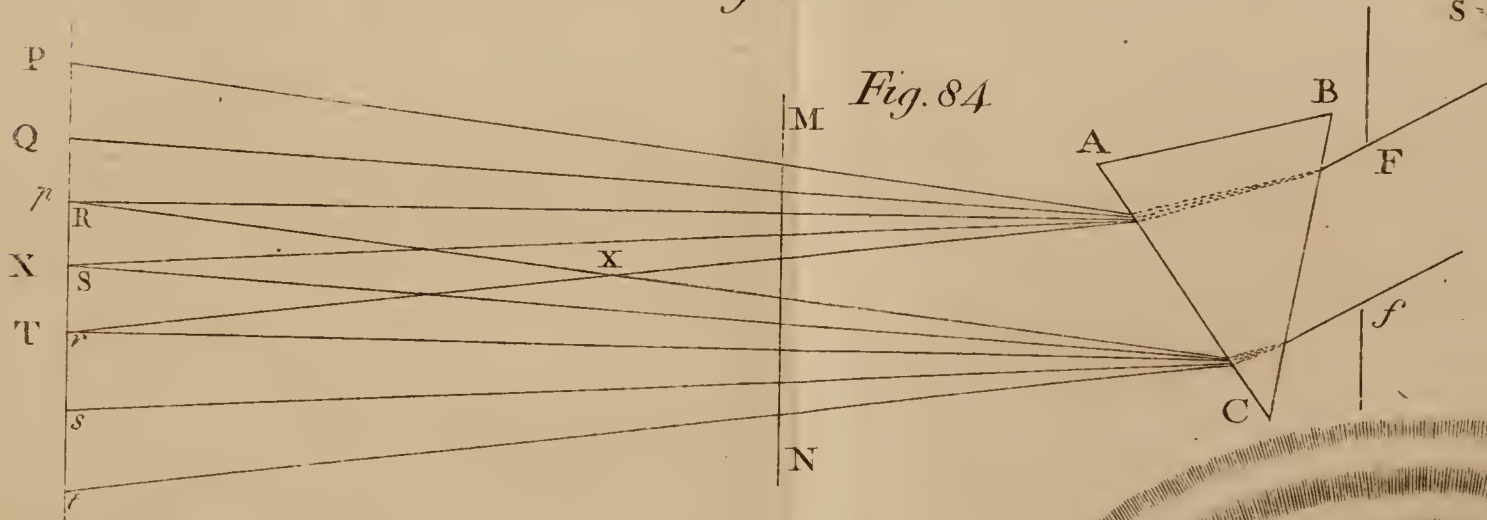

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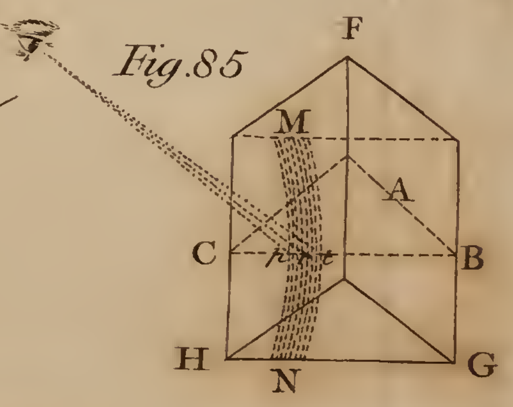

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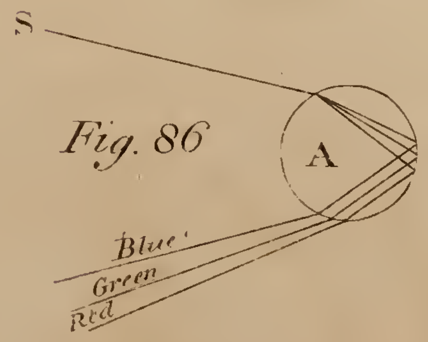

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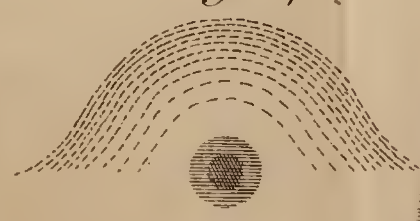

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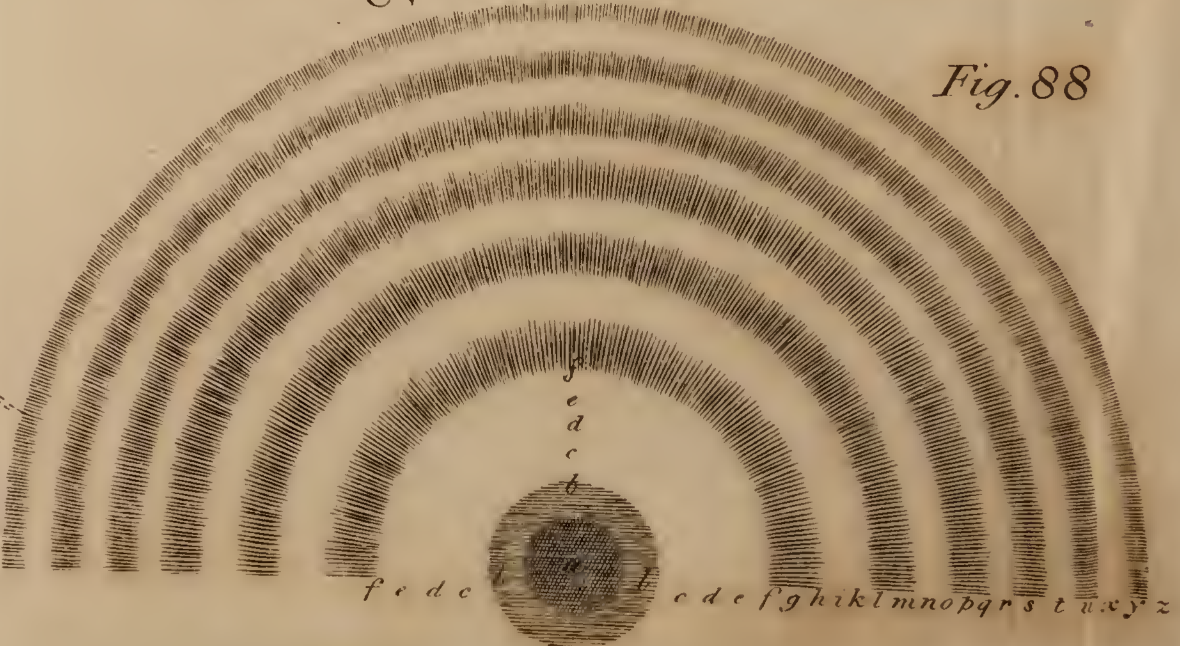

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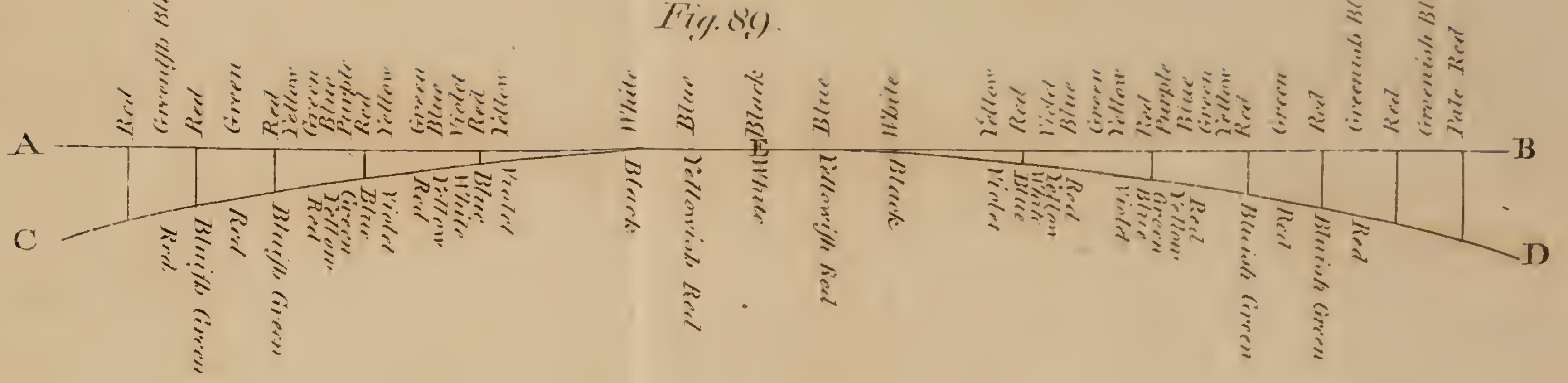
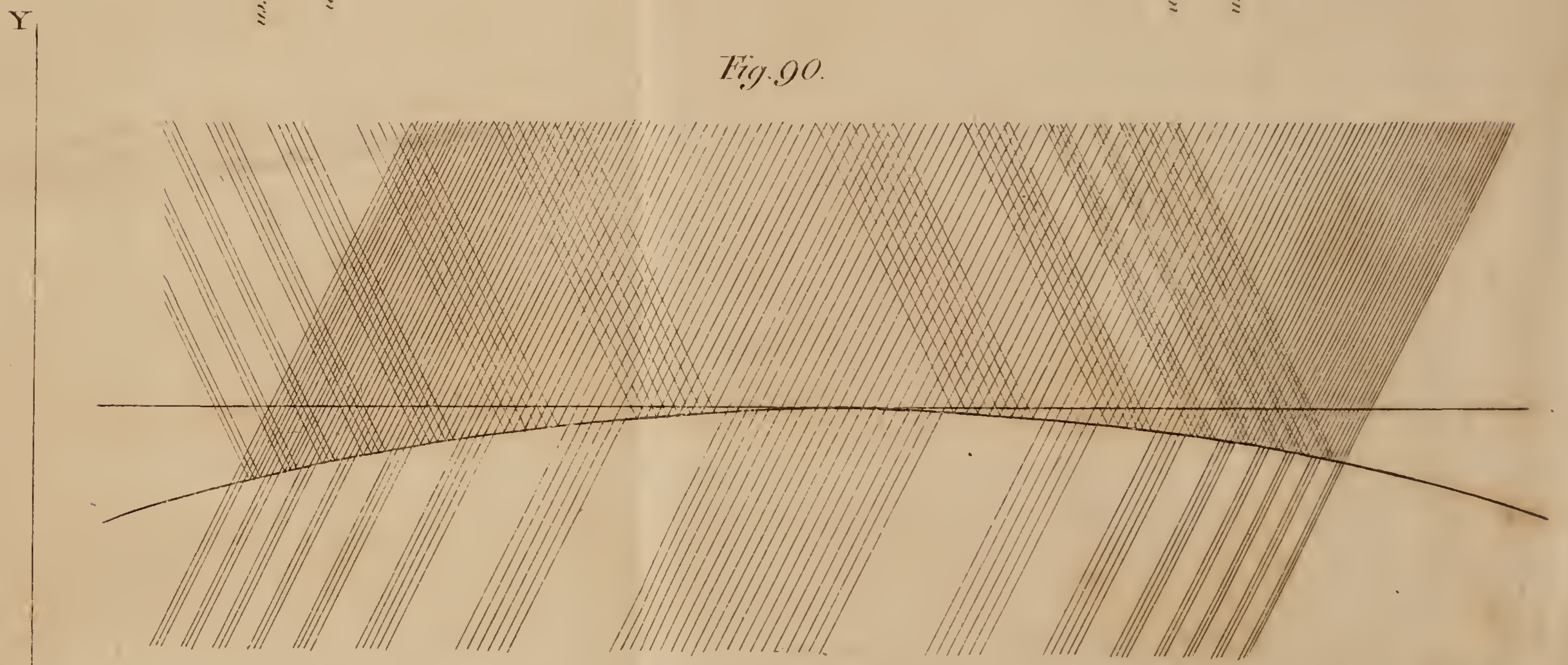


Fig. 90.



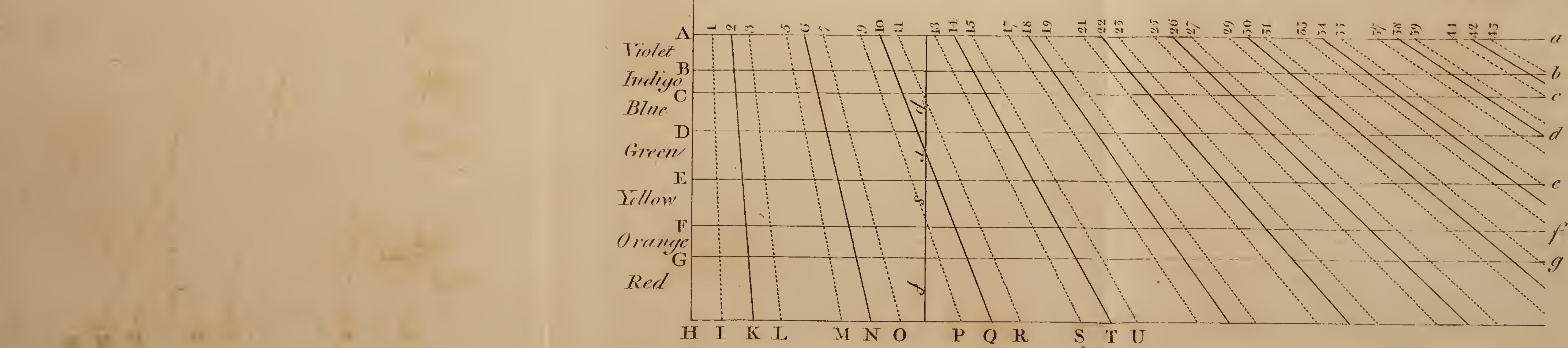
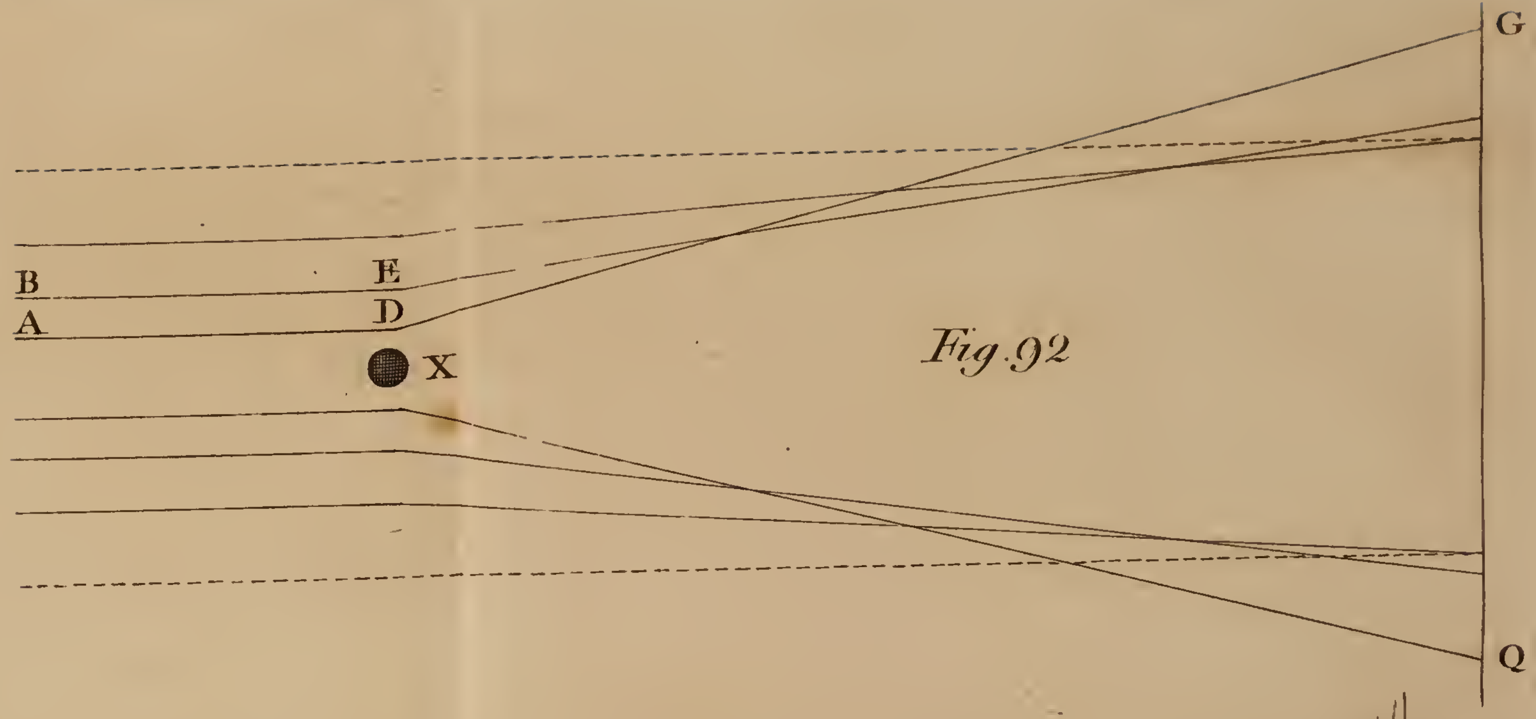


Fig. 91

Pl. XIIII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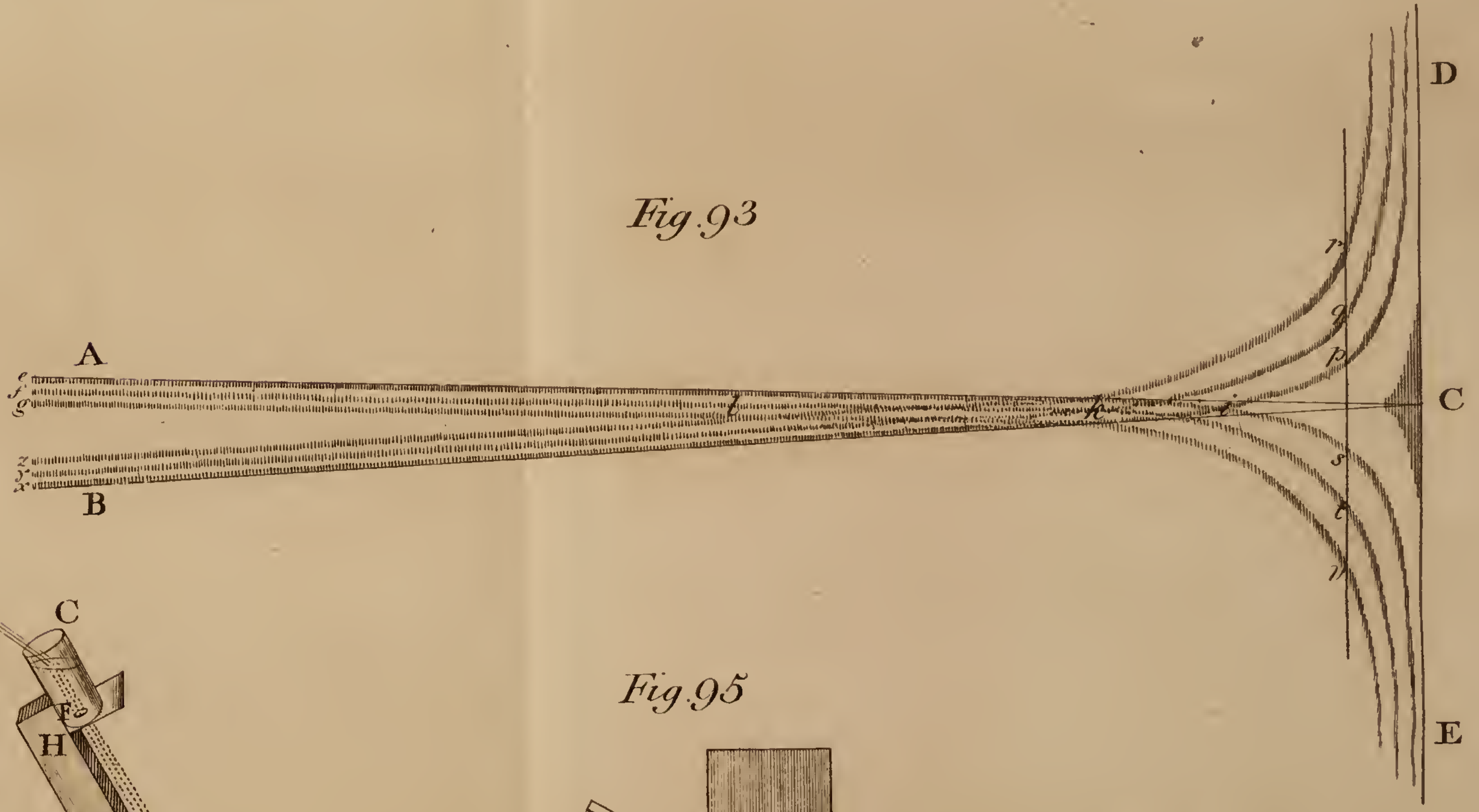
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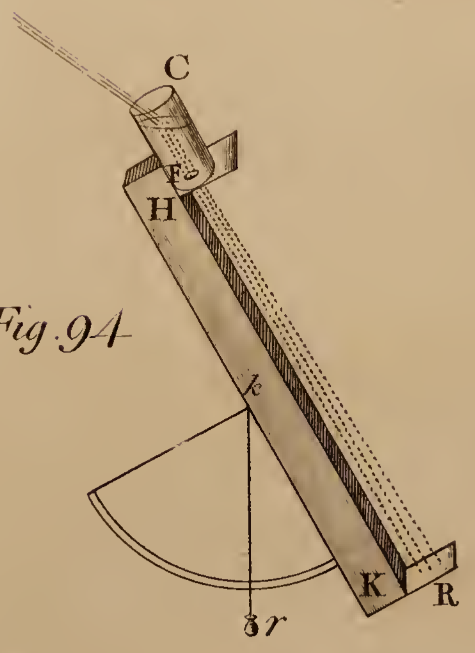
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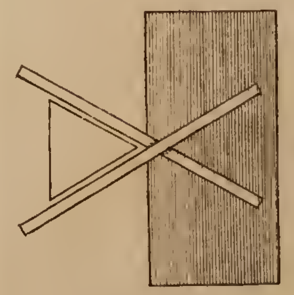


Fig. 95

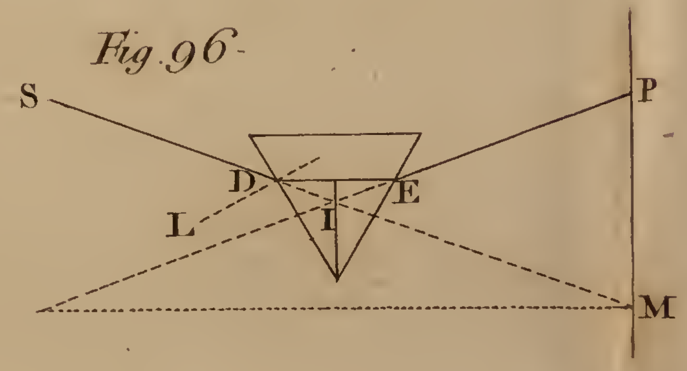
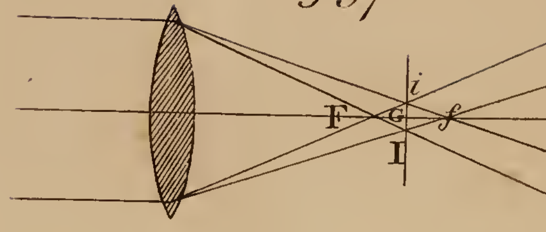
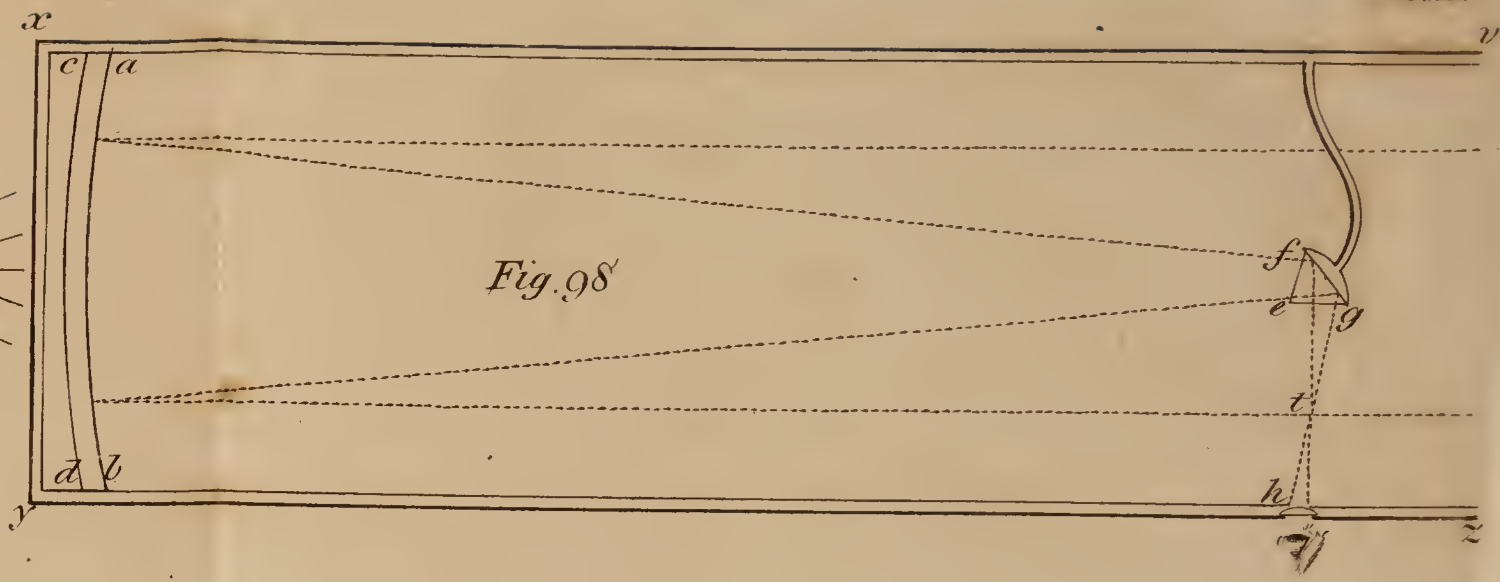


Fig. 96

Fig. 97





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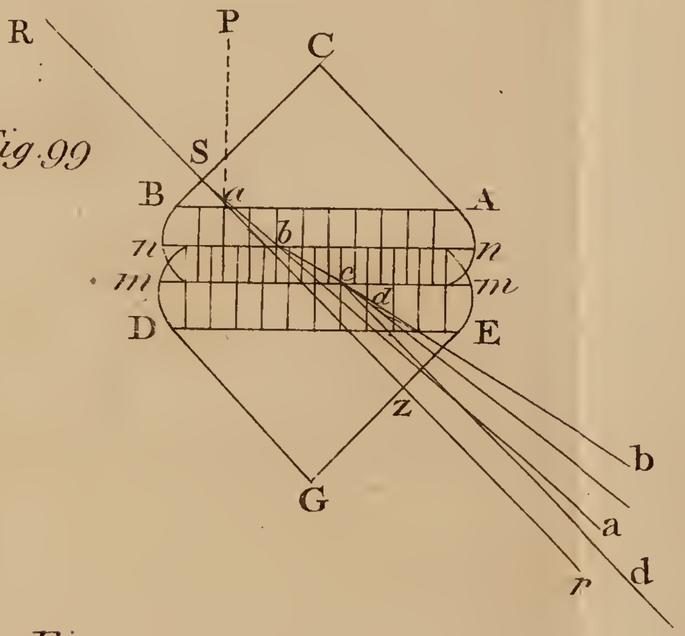

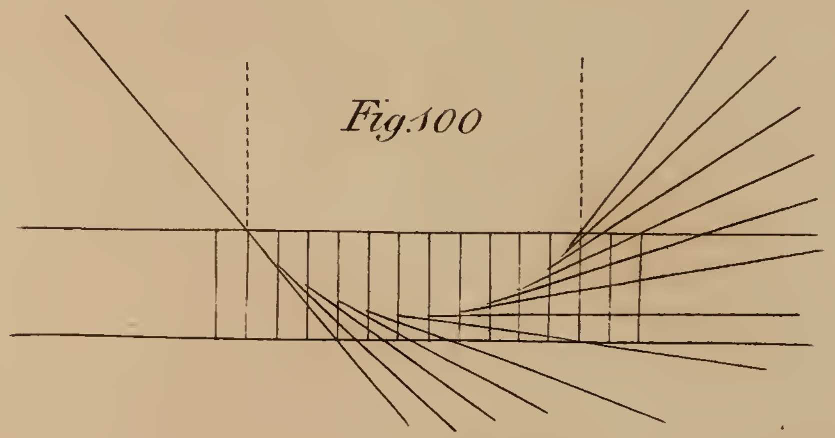

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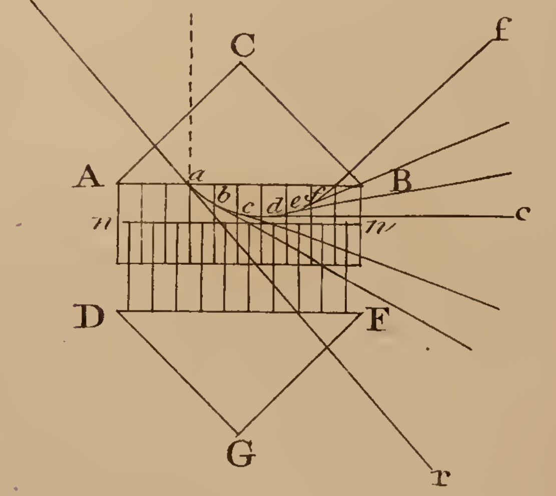

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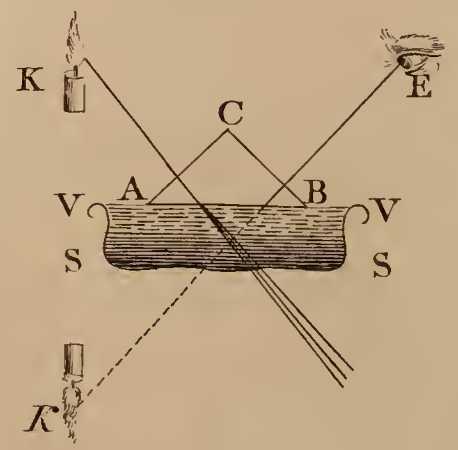

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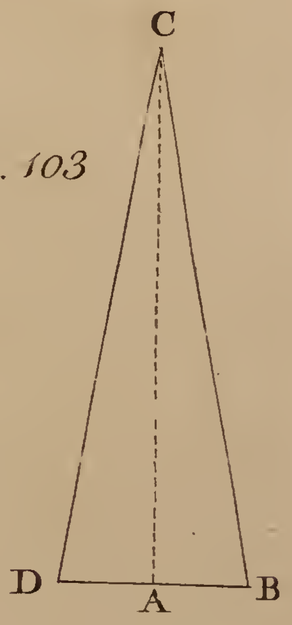

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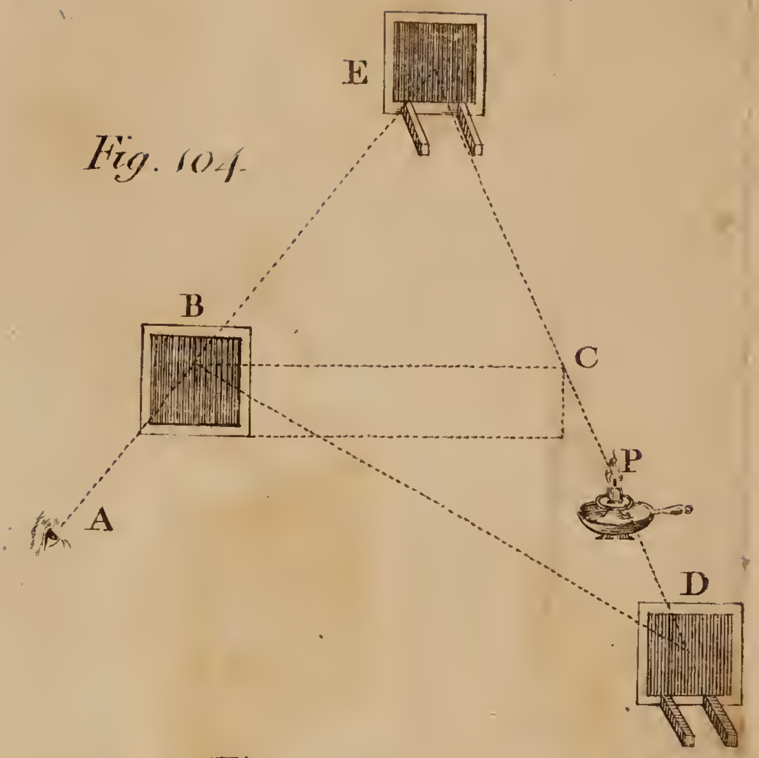

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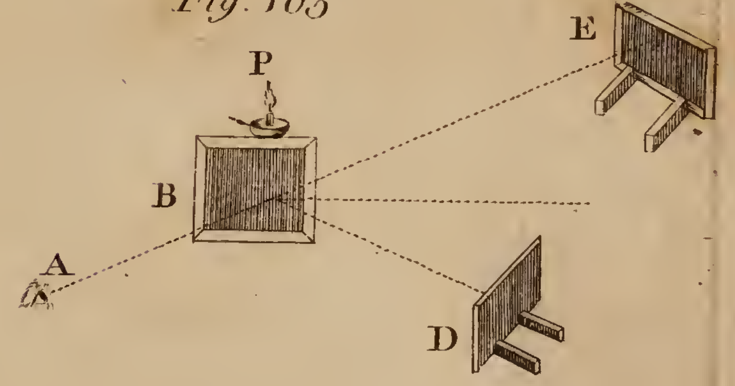


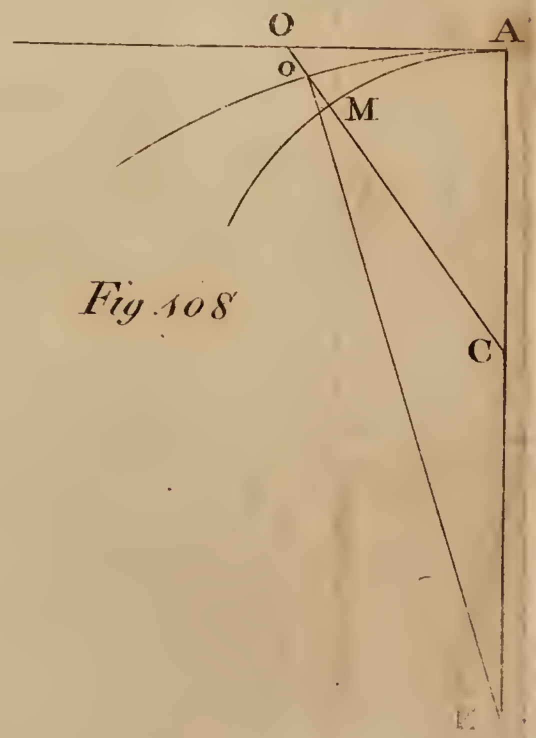
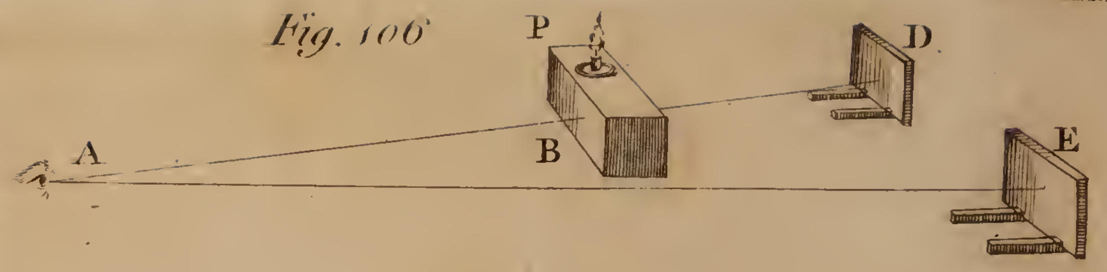


Fig. 108

Fig. 106



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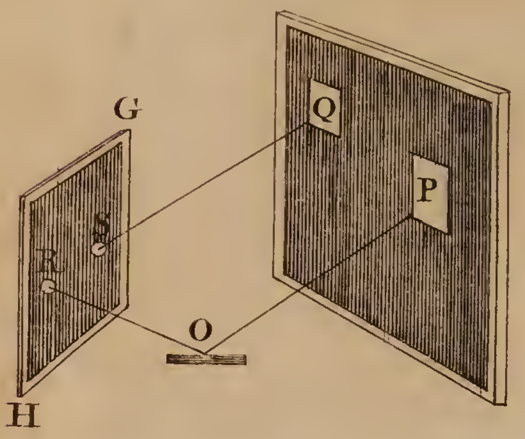

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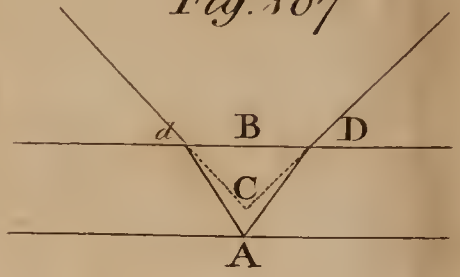

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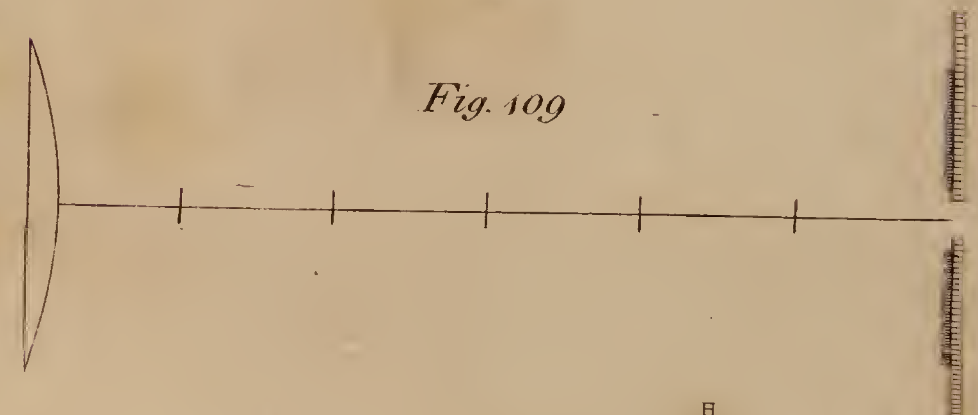

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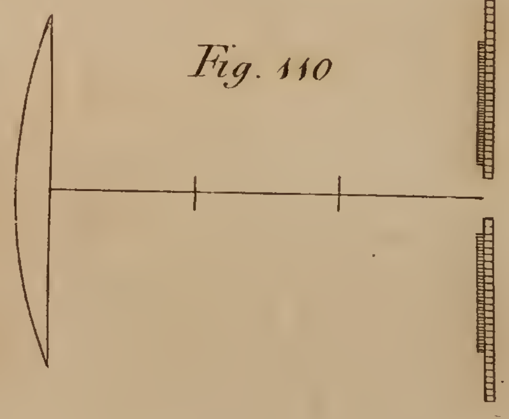


Pl. XVII.

Fig. 111.

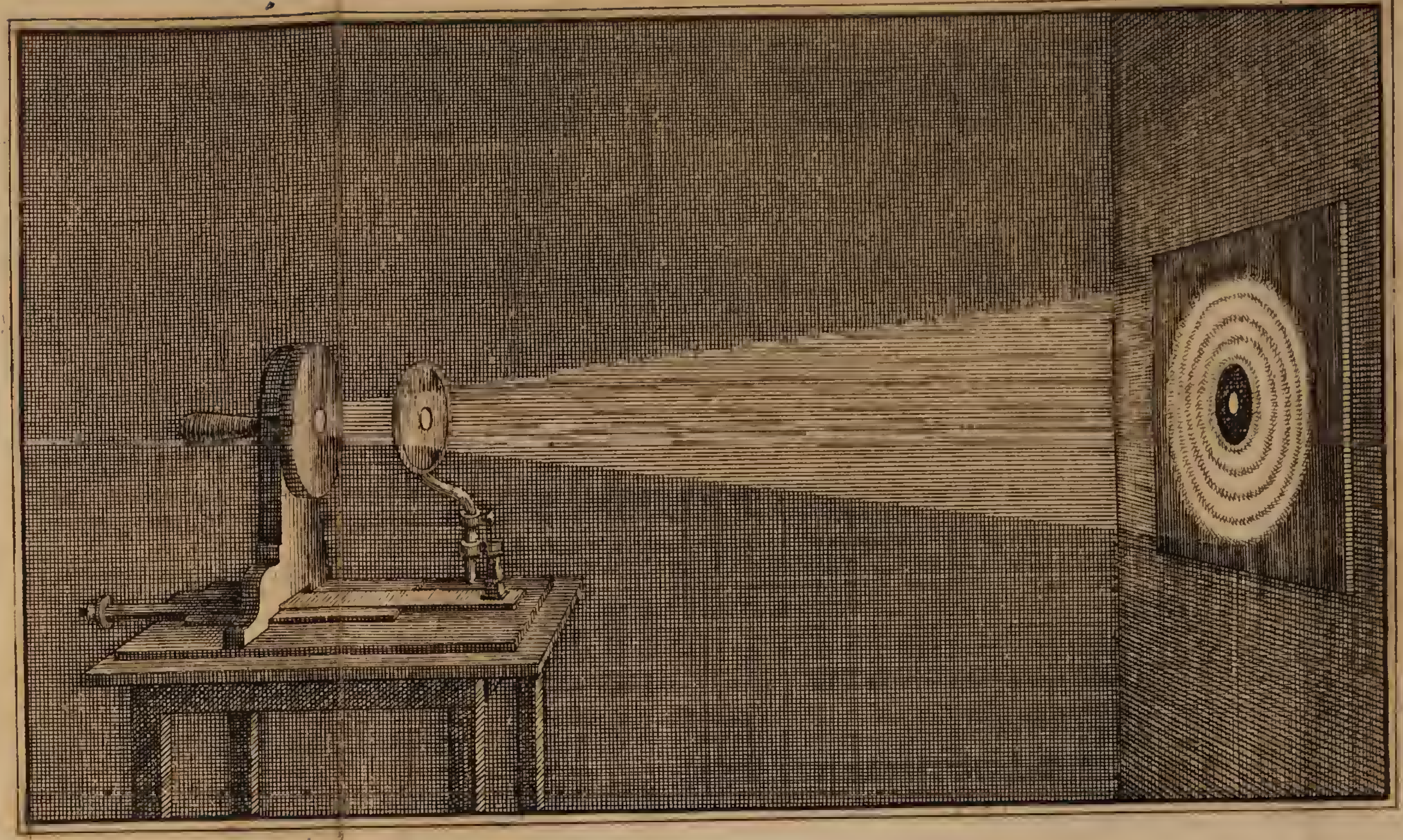
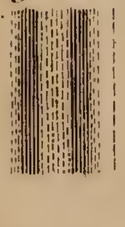


Fig. 112



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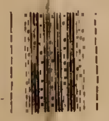

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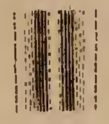

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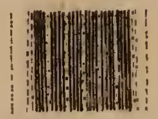

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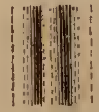

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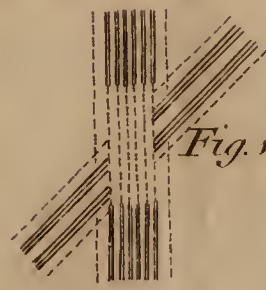

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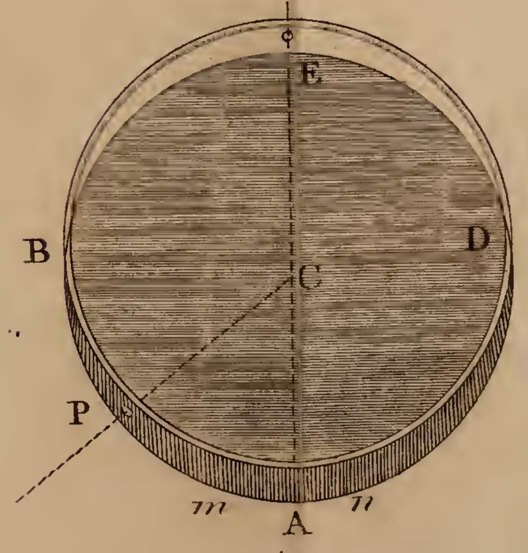

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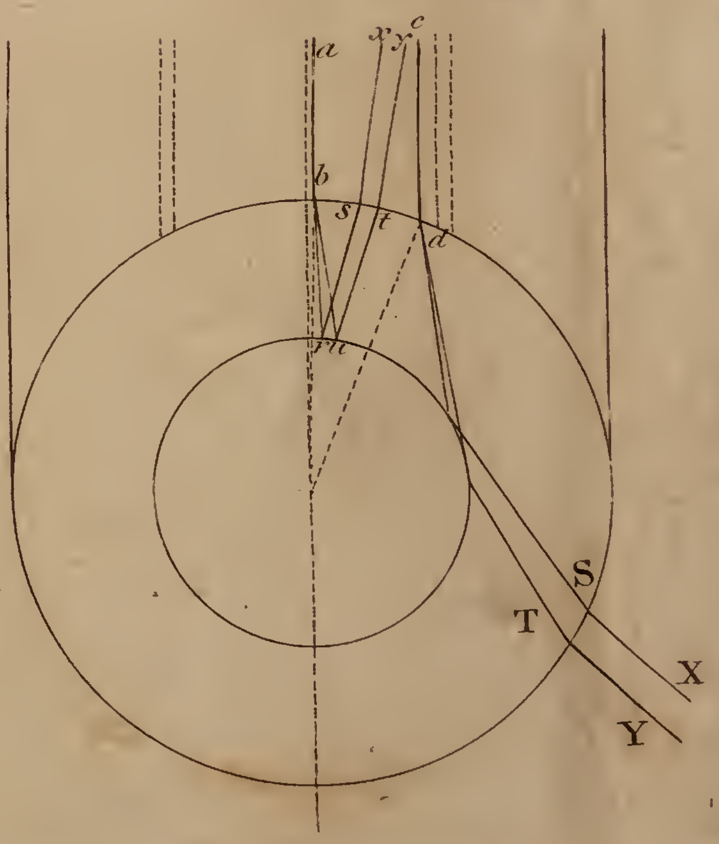

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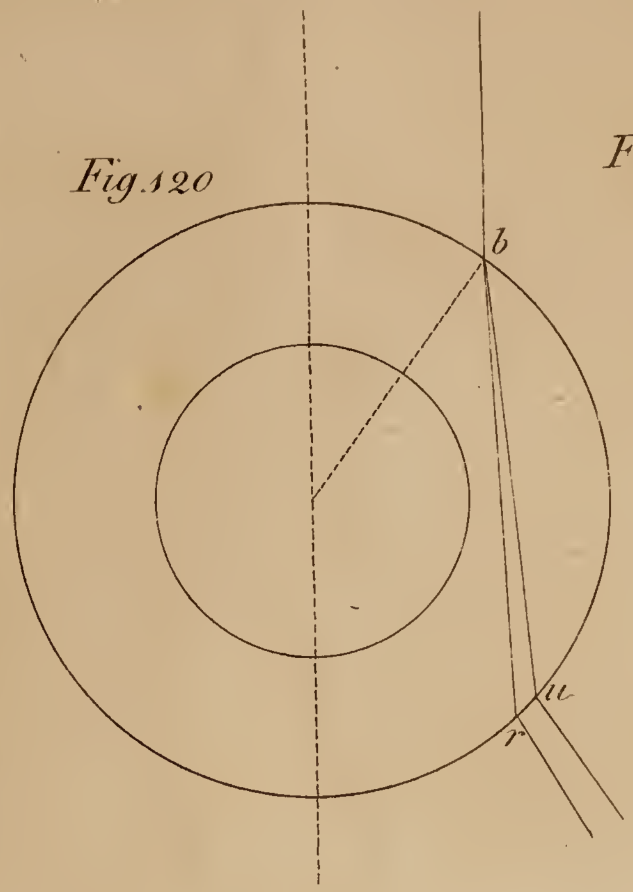

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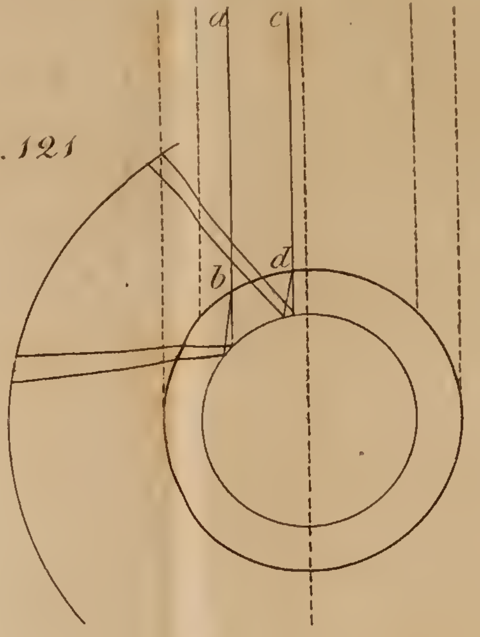

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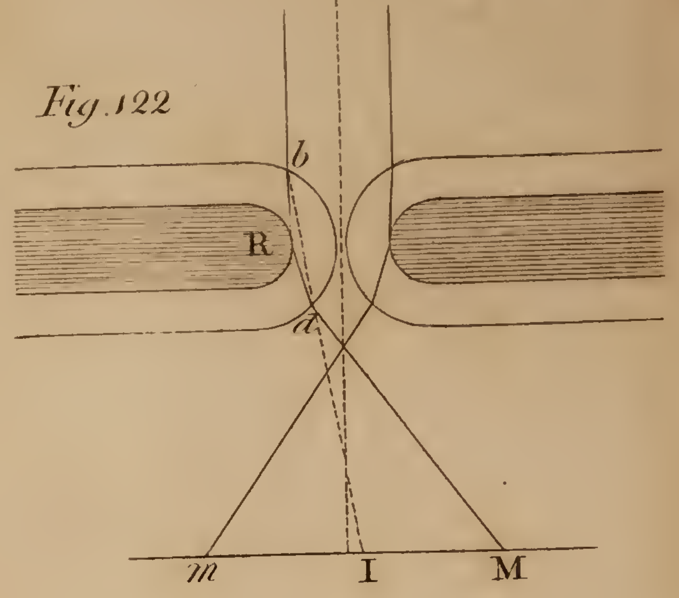

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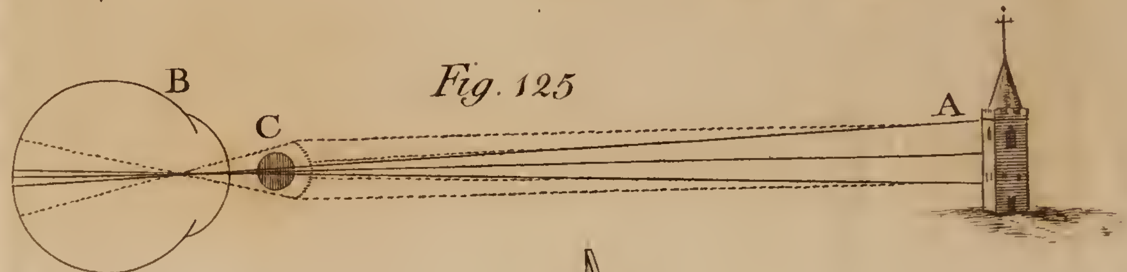

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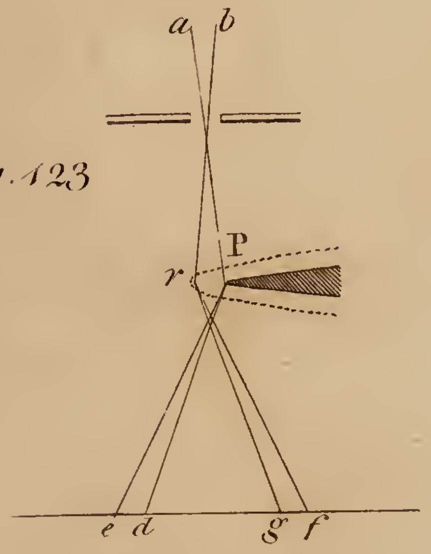

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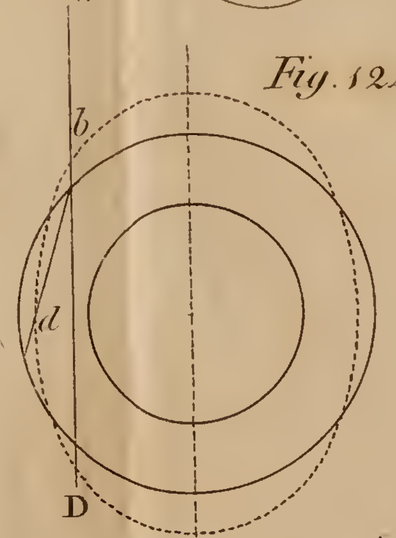

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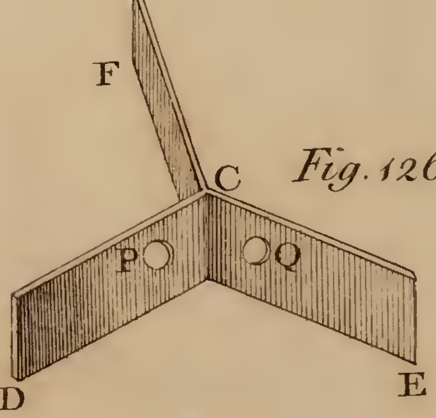

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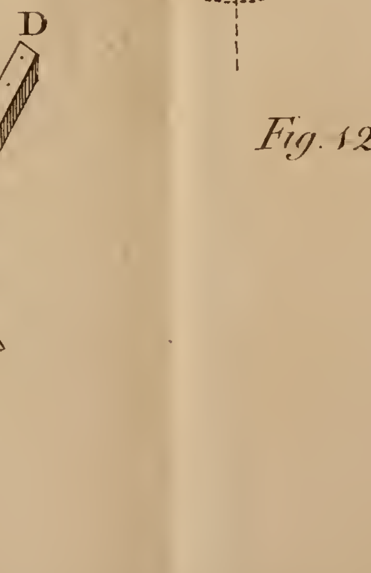

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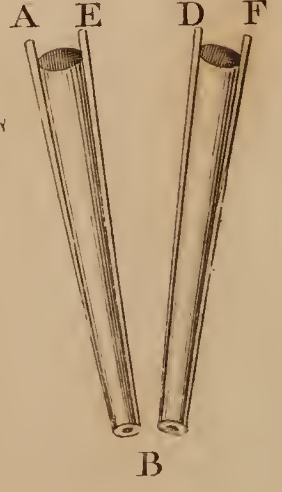

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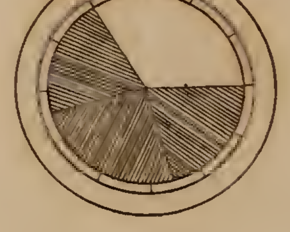
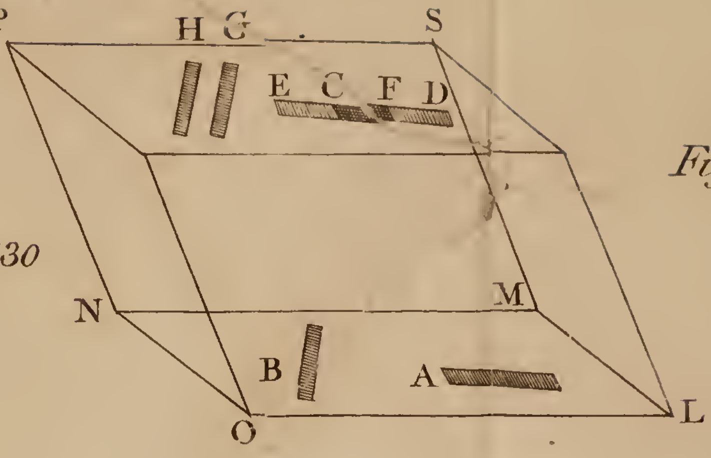


Pl. XIX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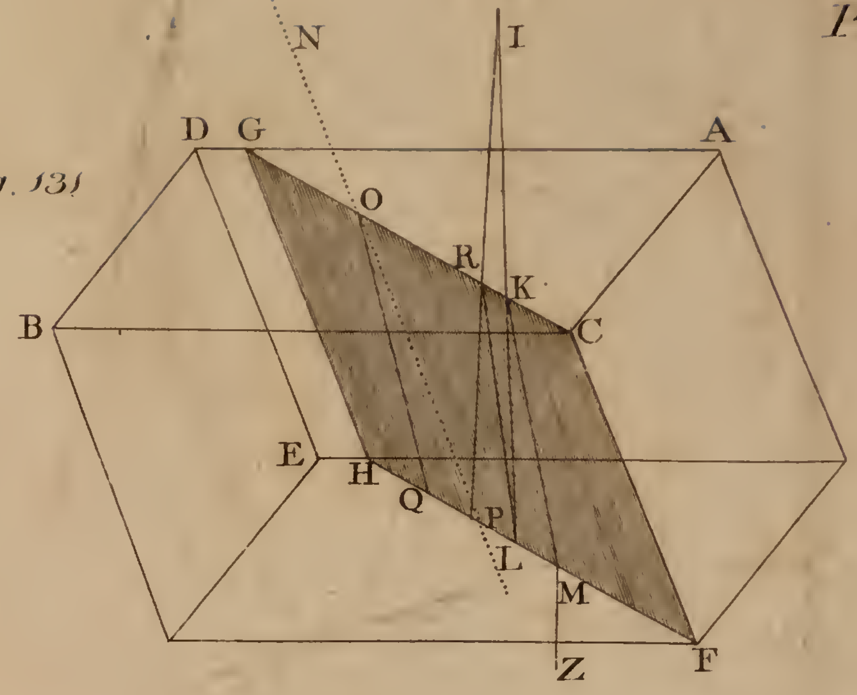
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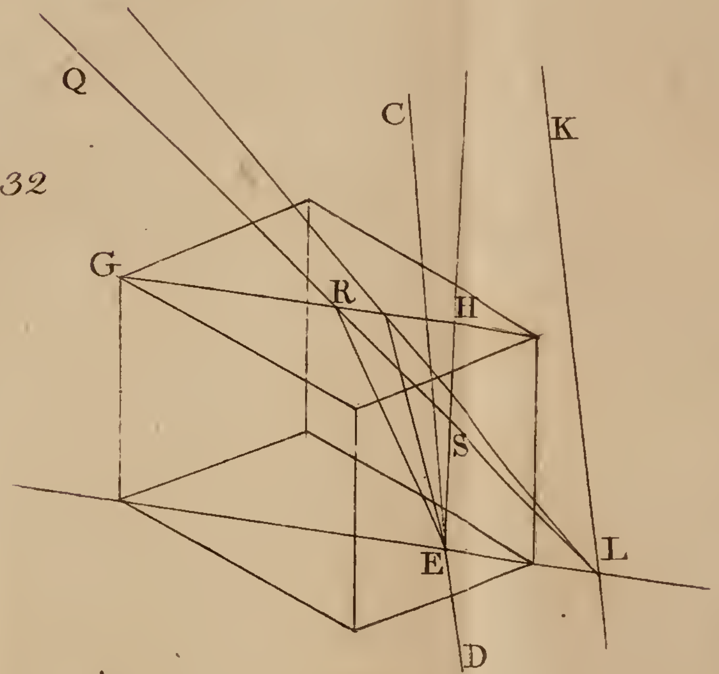
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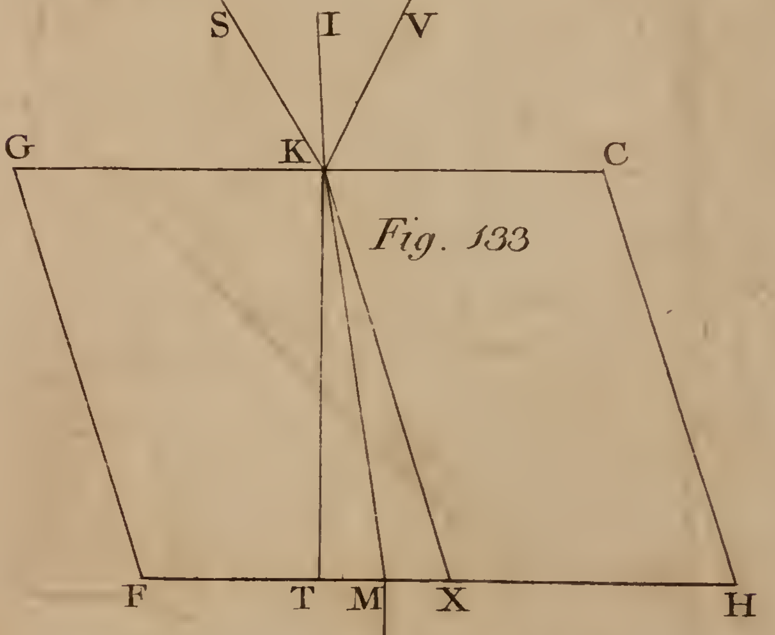
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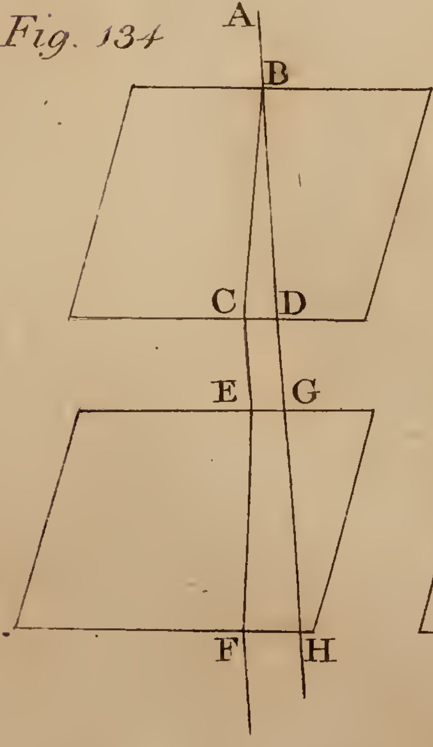
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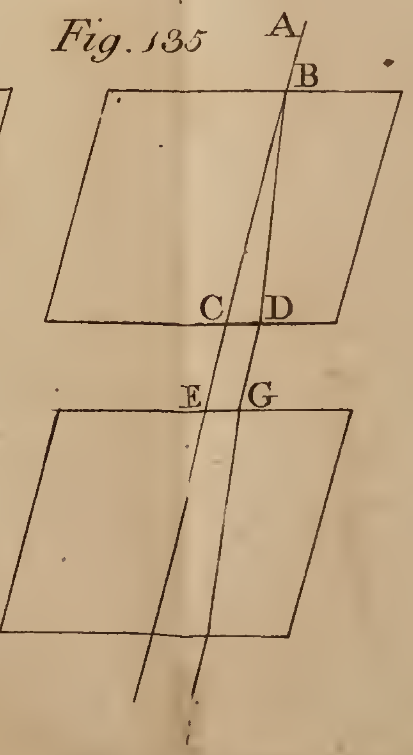
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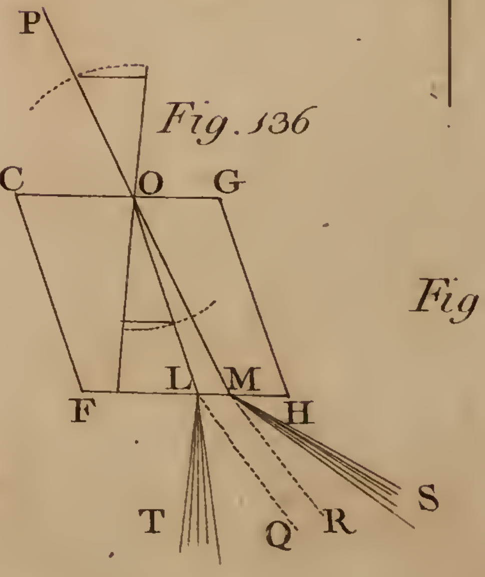
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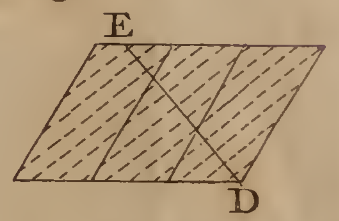
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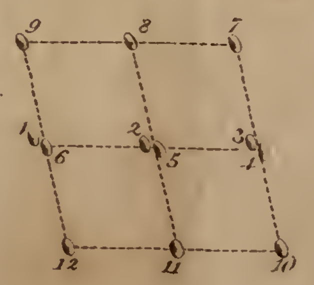
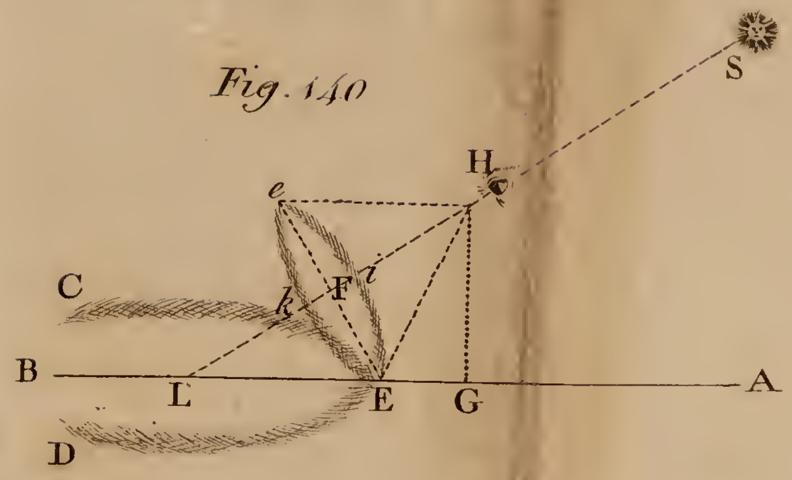


Fig. 137

Pl.XX

Fig. 140



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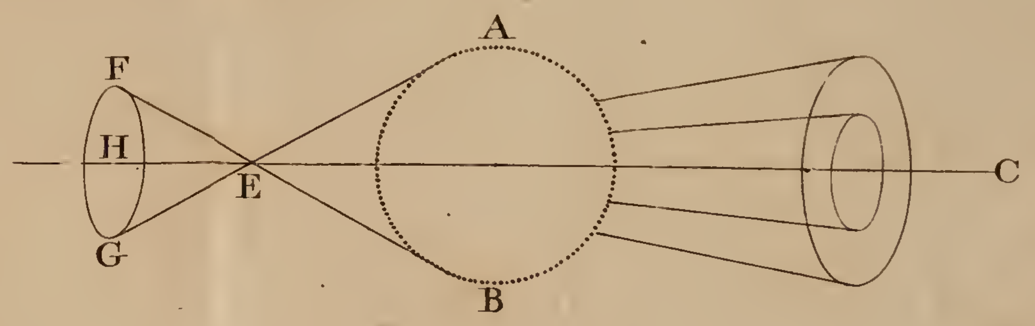

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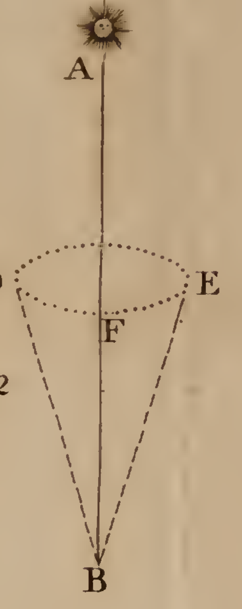

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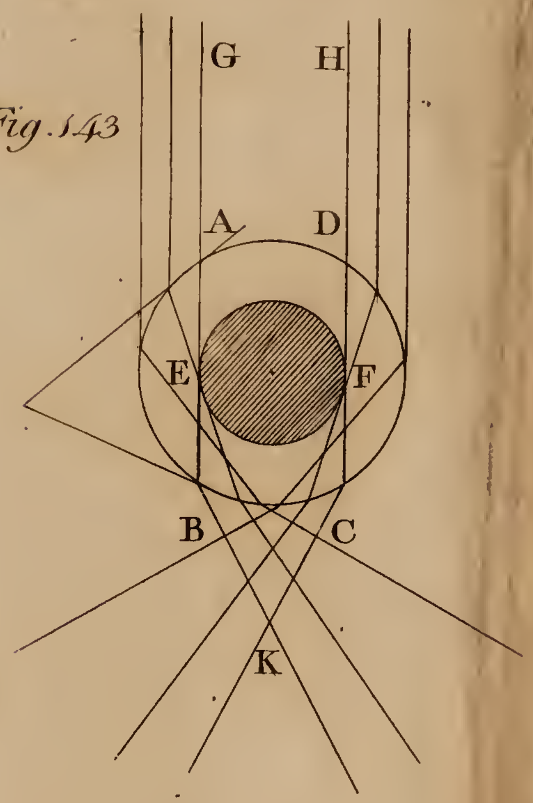

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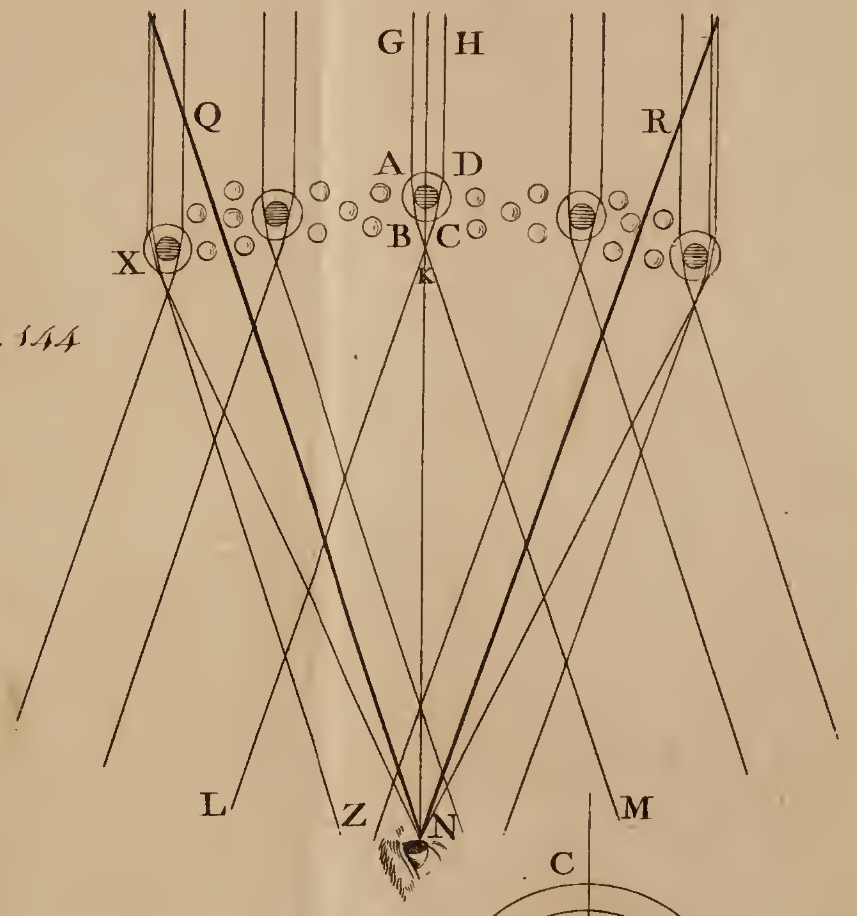

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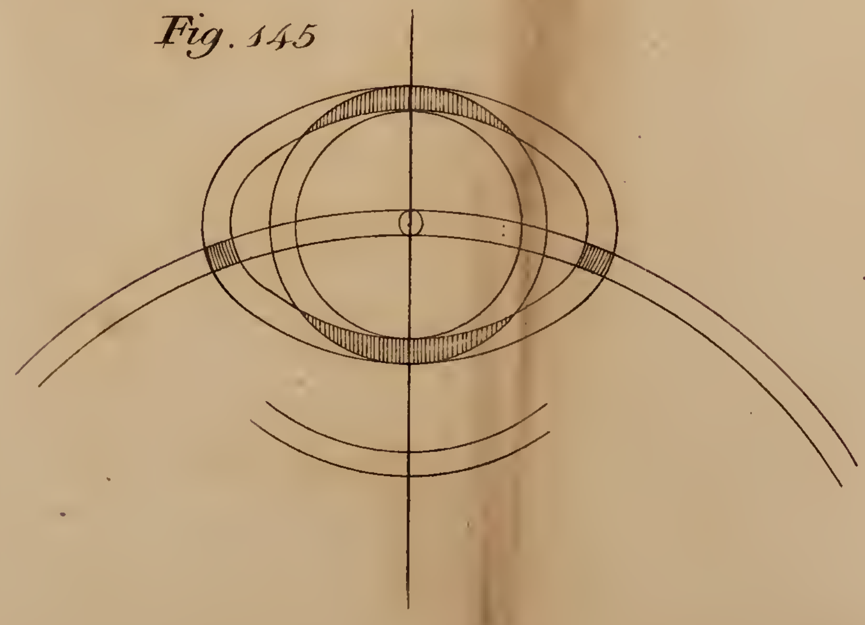

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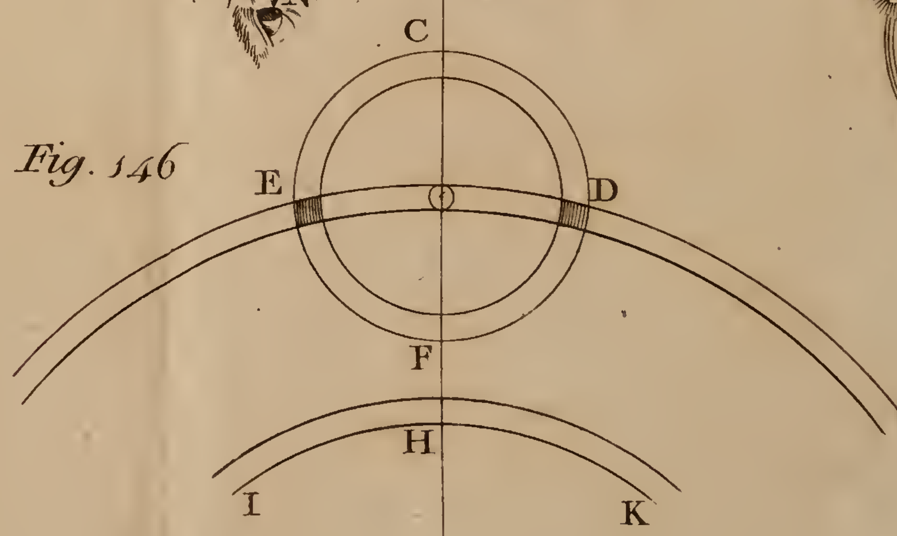

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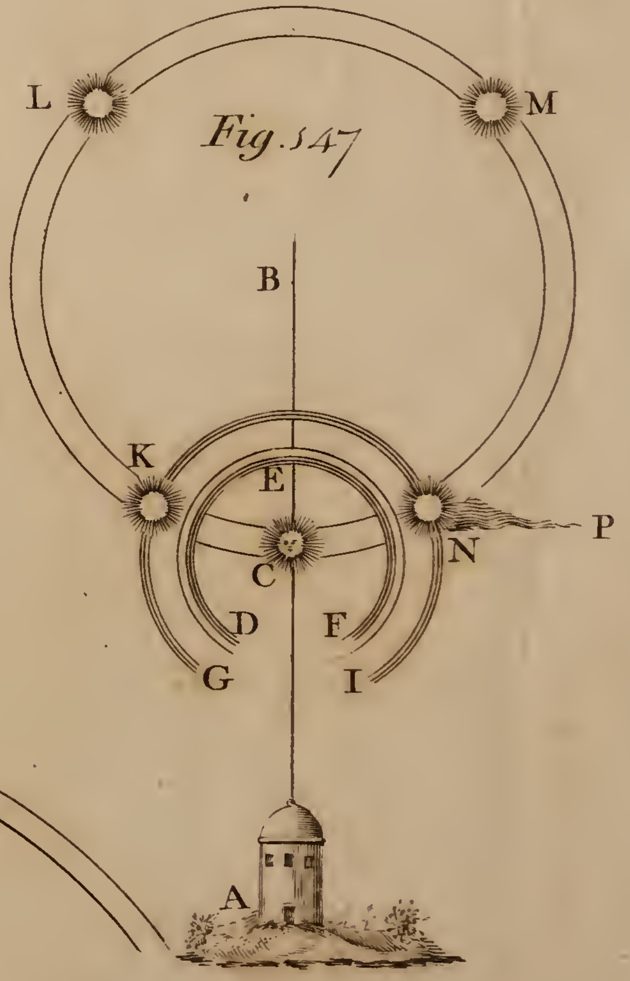


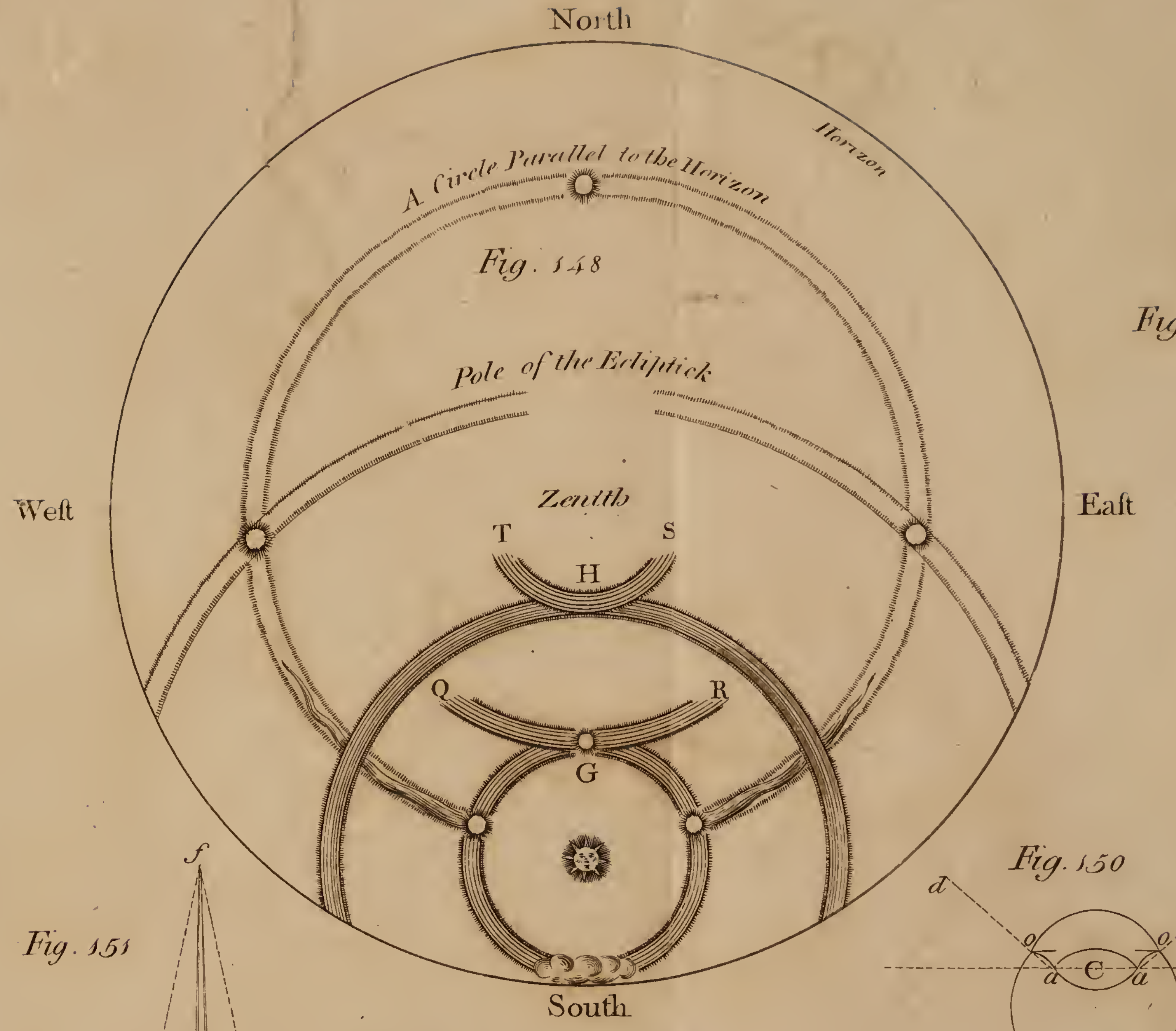
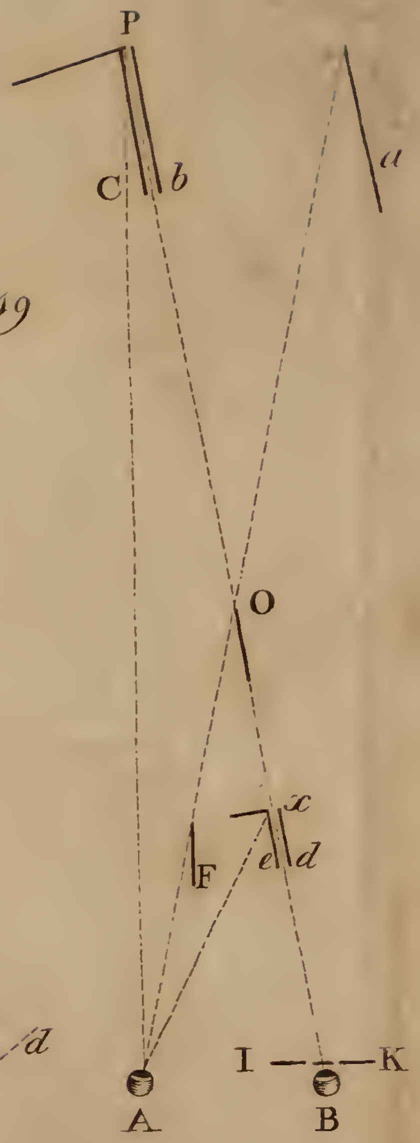


Fig. 148

Fig. 149



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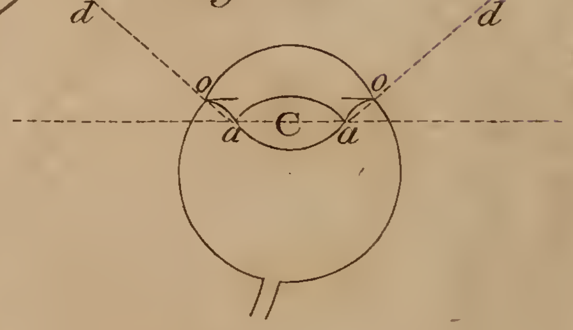

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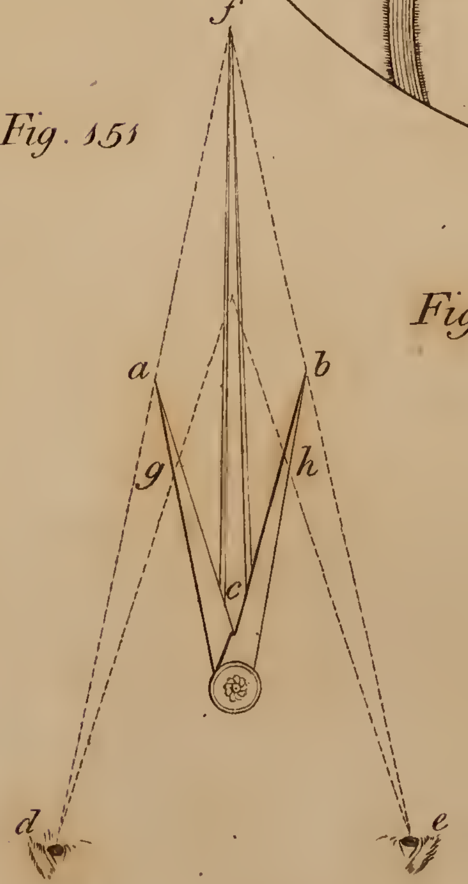

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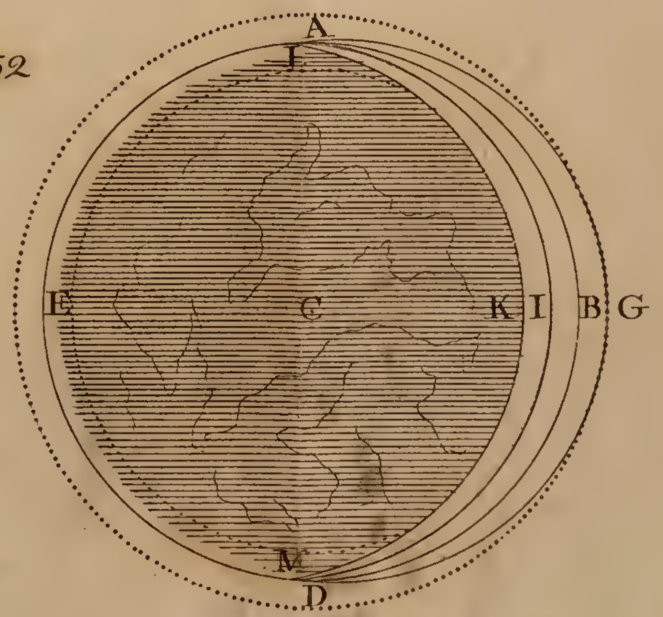

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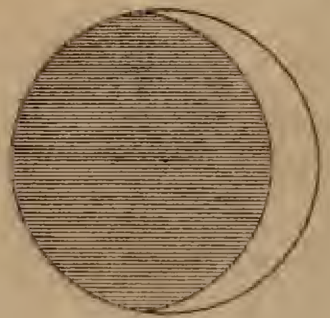

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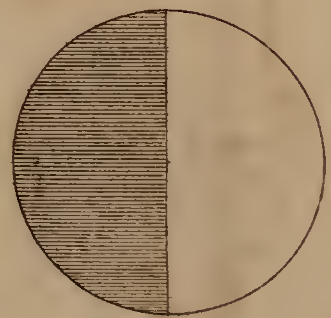


Pl. XXIII.

Fig. 162

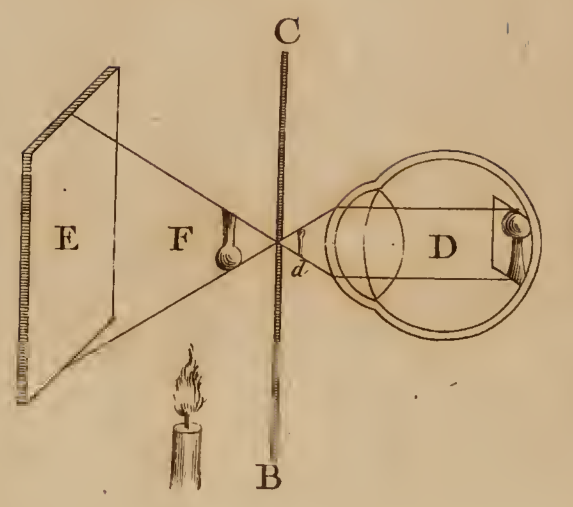
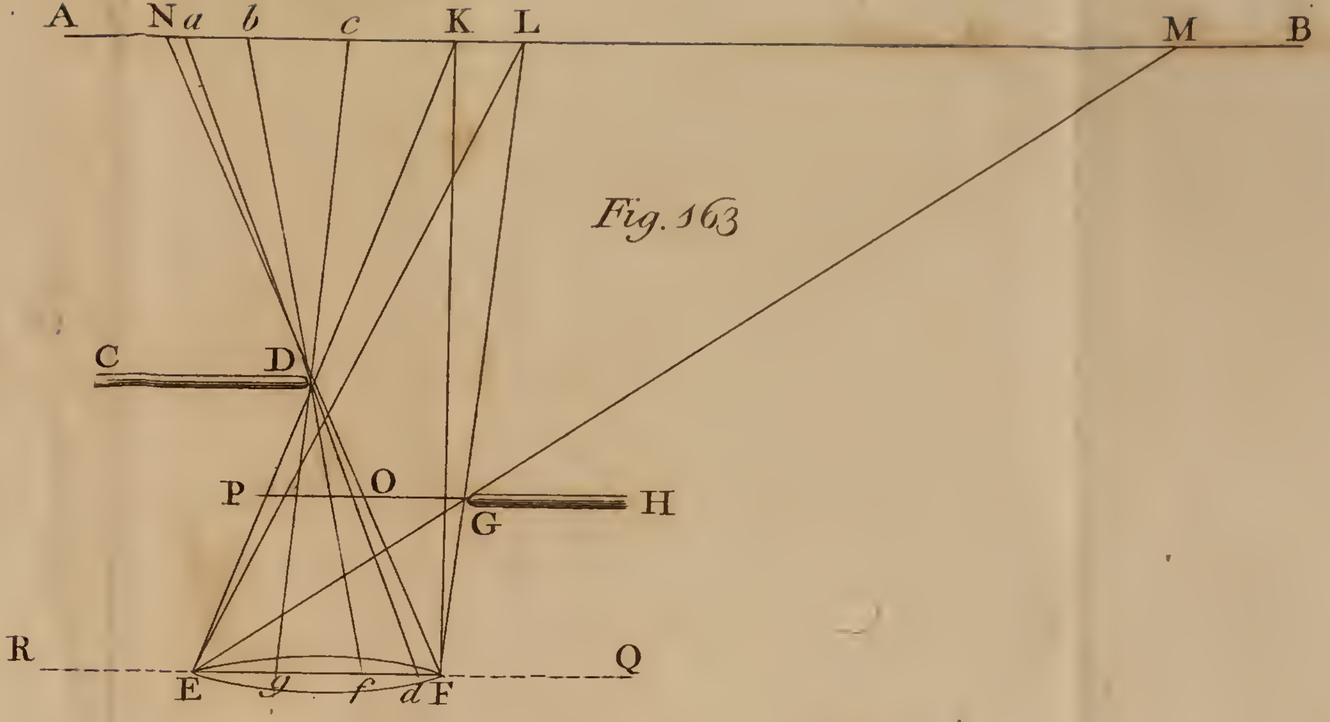


Fig. 163



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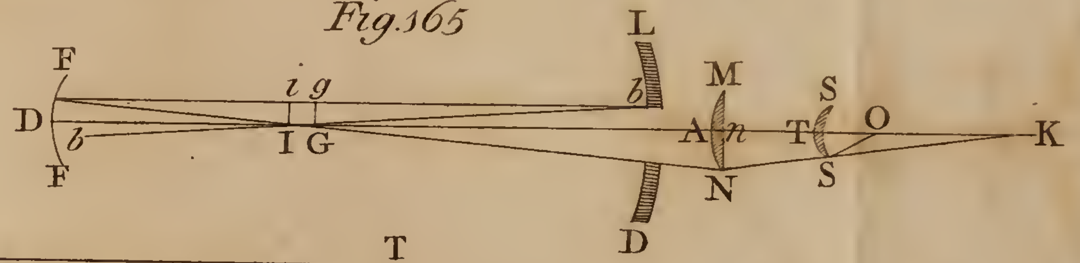

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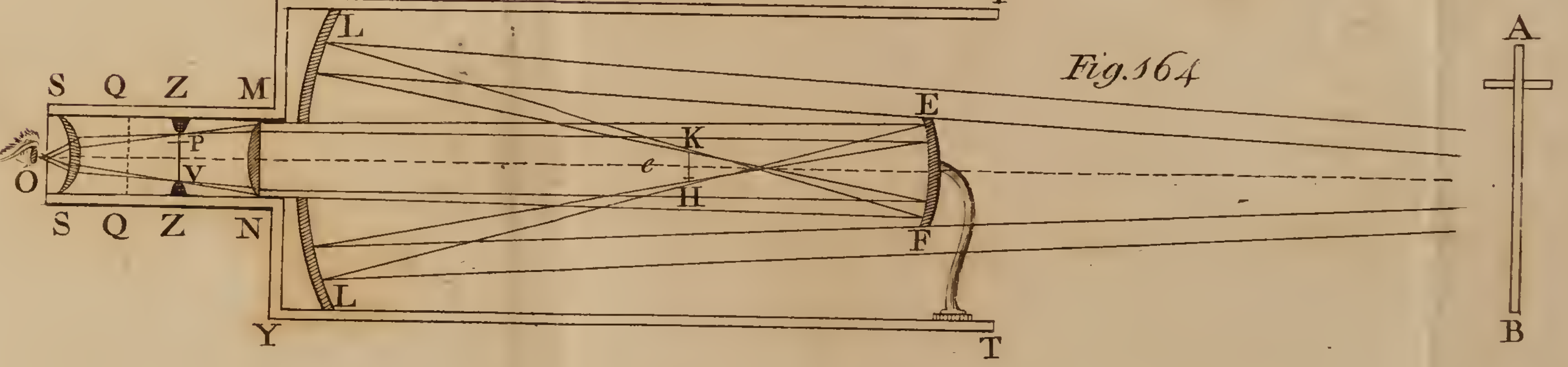


Fig. 167

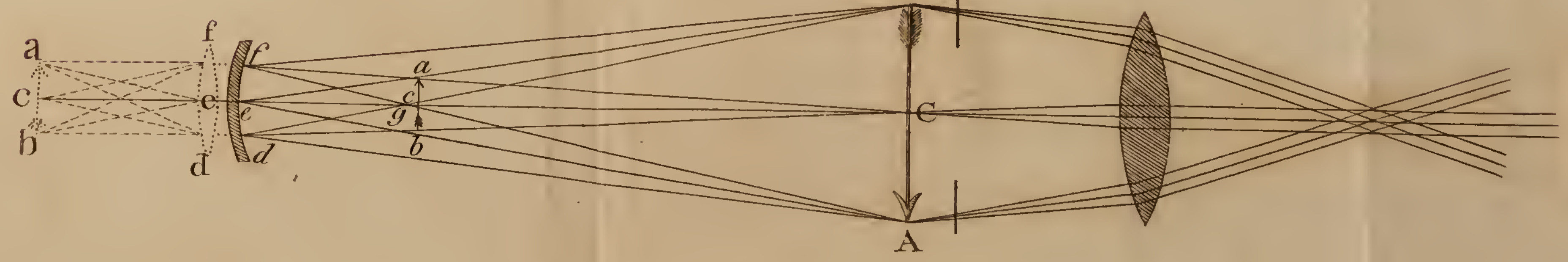
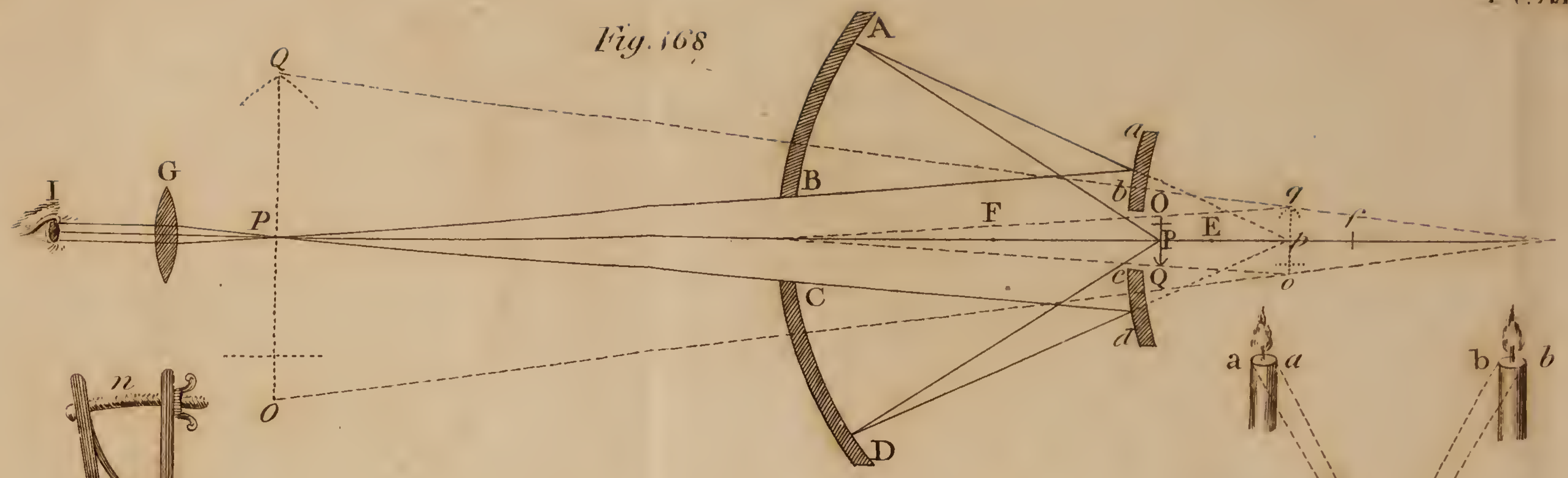


Fig. 168



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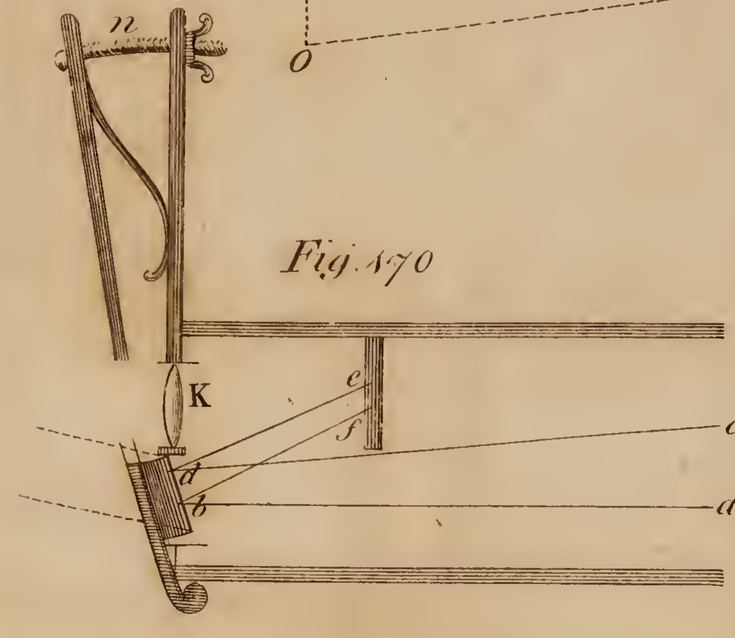

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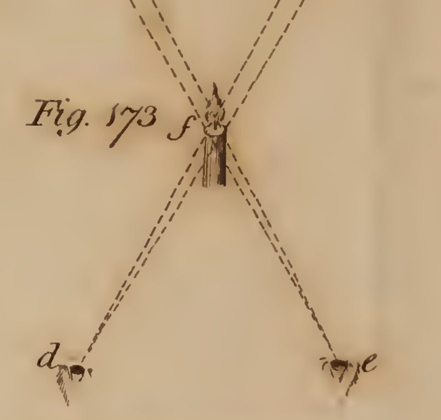

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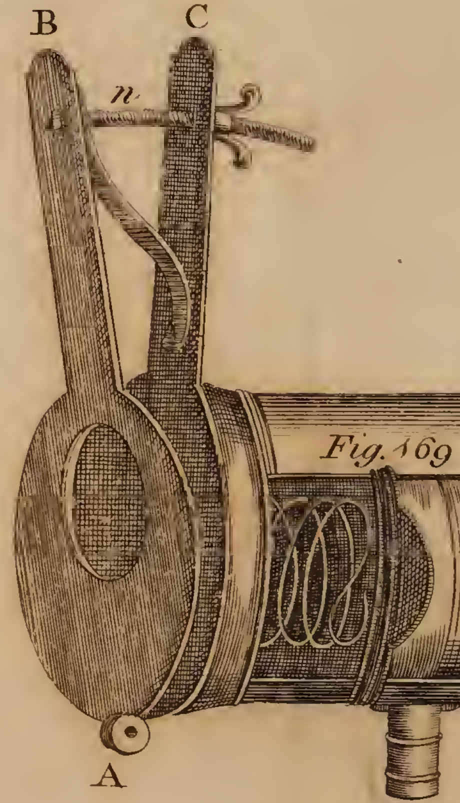

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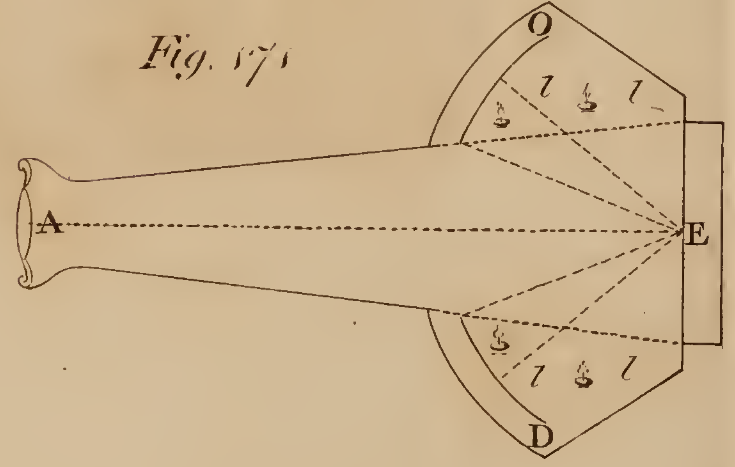

Fig. 172